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THE ROYAL SOCIETY OF NEW SOUTH WALES

Address:— Royal Society of New South Wales,
PO Box N112 Grosvenor Street,
NSW 2000,
Australia.

THE ROYAL SOCIETY OF NEW SOUTH WALES

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Science Centres and/or Science Museums for Australia*

R. S. BHATHAL

ABSTRACT: Australia is at the technological crossroads, i.e. it can either go down the path of technological obsolescence or it can have a technological renaissance and ride on the back of new high technology industries. However, to do this it urgently needs to disseminate information on new science and technology not only to the general public but also to the school going population. In short, Australia needs to emulate the example of its industrial competitors by setting up full fledged science centres to provide the necessary information and hands-on experience in the fields of new science and technology since the major science museums in the country have tended to be more concerned with the display and interpretation of the products of smokestack industries. It needs to create these new institutions so that it can expect to have a constant supply of creative young people to take up careers in science and technology and thus provide the necessary ballast for an economic and industrial take-off in the 21st century.

INTRODUCTION

Today, Australia is at the technological cross-road, i.e. it can either go down the path of technological obsolescence or it can have a technological renaissance and ride to future prosperity on the back of the high technology industries. The technological environment in Australia is filled with high unemployment, low productivity and inefficient industries protected by high tariff walls. The decline in the industrial estate, is not something new, but has been going on silently for a number of years. In fact, in 1965, for the first time in Australian history, white collar workers in technical, managerial and clerical positions outnumbered blue-collar workers. Industrial Australia was giving way to a new society where people were beginning to work with information rather than producing goods. However, Australia had not prepared itself for this shift to an information based society although it had been ushered in globally in 1957 by the launching of the Sputnik by the Russians (Bhathal, 1984; Cole, 1983).

The Labor Party manisfesto talks about sunrise and information based industries to get Australia moving again. While the Liberal Party, according to Puplick (1984) has for reasons of history and tradition adopted an anti-science bias and in a sense has no real policy for science and technology. But what are these sunrise or information based industries? Why are they different from the familiar smoke-stack industries of yesterday? Why should the lay person be concerned with them? Why are they productive and promise to bring economic wealth? What are the options that the high technology industries are promising to bring to Australian society? These are some of the questions that the lay person is asking. But who does he turn to for an answer?

Jones (1982), the Minister for Science, in reading the Labor Party Science and Technology Policy statement at the 1982 National Conference stated:

".....the aim is to initiate a continuous public information compaign in an attempt to demystify scientific processes, to raise levels of community understanding about science and technology so that the Australian people and their political representatives can be directly involved in choosing between options and determining priorities."

The question we need to ask is: How can we achieve this demystification of science and raise levels of community understanding about science and technology? Should we turn to the newspapers, the radio and television media, universities, think tanks, the science museums or the science centres.

THINK TANKS AND MASS MEDIA

In an attempt to come to grips with this problem the Federal Minister for Science has set up a Commission for the Future (Ford, 1985). According to him:

"Its emphasis will be on explaining future scenarios pointing out the range of options opened by new technology and then saying to the people, you must choose for yourselves."

Activities of the body, for which a sum of $166,000 was allocated in the last Budget, will include commissioning studies and discussion papers, producing audio-visual material and holding seminars and workshops. Whether this will achieve the demystification of scientific processes or raise community understanding about science and technology is difficult to ascertain at the moment. However, there has already been a spate of criticisms to this initiative coming mainly from journalists who don't have much sympathy or understanding for science.

One of them (Haupt, 1985) was even bold enough to suggest that:

"If scientific understanding is what the commission's on about, the money would be better spent providing free subscriptions to Scientific American to all who wished them."

This is just as ridiculous as saying that persons who wanted to get an understanding of the fine arts

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* Presidential Address delivered before the Royal Society of New South Wales at Macquarie University on April 3rd, 1985.

and crafts in Australia need only subscribe to Arts National.

There is no denying that a lay person can turn to newspapers, magazines, radio and television for information on science and technology. However, there are severe limitations and restrictions in the presentation of science through the mass media. The information is varied both in quality and quantity. In certain instances due to time and space constraints the mass media tends to present only a one-sided view (e.g. in the case of the nuclear arms and power controversies) or an incomplete view or not to present anything at all.

The mass media is also disadvantaged by not having the necessary props which give people first-hand experience of scientific phenomena. Challis (1983) goes even further to say that:
"Most editors and TV managers come from a background in the humanities. They do not have an understanding of where science fits in, they do not recognise science stories and their perception of what an audience wants is limited."
Kennedy (1983), the Editor-in-chief of the Bulletin, probably epitomises the view held by most editors of the mass media on science stories and the scientific establishment in his comment:
"Having a specialist science writer is not one of our priorities. I would rather have a couple of foreign correspondents. Science doesn't attract the attention of bright young reporters who want to be kingmakers in Canberra, rather than scratching around in universities and hospitals. The trouble with science and medicine is that Australia is not the cauldron, and specialist reporters would have to spend a lot of their time in institutes in Paris, the U.S.A., etc."

MUSEUMS

We shall now turn to the world of museums. When one hears the word museums, several reactions follow and some of these are:
(a) they are musty, dusty, dingy places which display rows upon rows of static technical devices of a by-gone age or jars full of pickled animals or plant specimens or stuffed animals.
(b) they are places where reluctant school children are taken out for an outing.
(c) they are places with a large number of "DO NOT TOUCH", or "SILENCE PLEASE", signs, etc.

Or one gets the following impression about museums as in the 1975 Piggot Report on Museums in Australia. It stated that the "major museums in Australia should be in the forefront in disseminating knowledge but in many departments even national museums are as up to date as an encyclopaedia of 1920 or 1954."

Edwards (1981), in his report on Museum Policy and Development in South Australia, notes of a particular national museum in Australia that "the museum has interesting exhibits but almost all of them relate to the developments that occurred between 1850 and 1900. In short, a visitor to this Museum in 1900 might well have seen almost the same displays as in 1981."

Most of the science related museums in Australia were founded between the years 1827 and 1891. I say science related because except for the Science Museum of Victoria which is comparable in terms of collections to the London Science Museum in Kensington, the rest of the museums have an assortment of collections which include natural history, anthropology, geology, applied arts, history, etc. Sydenham (1975) in his review of the facilities of museums of science and technology in Australia wrote:
"In the current Australian situation, physical sciences and the technologies are barely represented. Across the breadth of Australia there is no display of science to compare with overseas museums."
While O'Hagan (1976) noted:
"Our museums are essentially display museums, the curators are preponderantly scientists with backgrounds in the descriptive rather than the experimental sciences, there is more room for physicists, chemists, experimental biologists, engineers and technologists. This is not to blame anyone, it is the way things have developed here."
Birch (1976) the President of the Australian Academy of Science wrote that one of the shortcomings of science museums is that they
"....treat science as prose rather than poetry".
That is, the creative element in science is missing from the displays.

Although the history of science and technology has witnessed three major international revolutions, viz: the scientific revolution, the industrial revolution and the computer revolution, the traditional science museum has for the most part of its life concentrated on the industrial revolution. The main emphasis of science and technology museums has been:
(a) to acquire and preserve the scientific and technological heritage of Western or Eastern Civilization, and
(b) to explain the construction, use and operation of various tools, machines and instruments, not so much for the general public, but rather for the 'working man' of the day.
In regard to the second aim, it can be seen that initially these early museums were very much involved with contemporary science and technology (just as the science centres of today are), but in failing to update their collections and keep abreast of technological developments they quickly lost their vocational relevance and evolved into museums of the history of science and technology. (Orchiston and Bhathal, 1984).

Regrettably, as Auer (1974) points out, this remains the focus of many science-technology museums today. They have become sentinels of a by-gone era. In them we now find relics of the industrial revolution, sitting quietly in display galleries, or packed away in dusty store rooms. Those items that are on display seldom paint a picture of the economic, intellectual, cultural or social forces that were at play during the industrial revolution, and therefore provide the visitor with little insight into the rich environment in which these technological advances were experienced by people living through that age. Danilov (1975) believes these museums are still predominantly repositories for objects, rather than serving as educational institutions.

Although Acts of Parliament in Australia have charged museums with the preservation of the scientific and technological heritage of the Australian peoples, however, less than 10% of the

collections in the science and science related museums in Australia deal with the artefacts of home grown industry, inventions or innovations. Until recently no concerted effort was made to seek out Australian inventiveness in science and technology. As O'Hagan (1976) pointed out: *"Nowhere, for example, do we have any local equivalent on the theme in the entrance hall of the Museum of Science and Industry in Chicago namely - America's inventive genius - showing, in four multimedia theatres, the development of science and technology in that country over the last 200 years. Nor do we have those working displays illustrating physical, chemical and biological principles presented in a way that young and old can use and understand."*
While the classical or traditional science-technology museum has tried to come to grips with the products of the industrial revolution it has tended to ignore the scientific and computer revolutions. In doing this it has moved away from the mainstream of the scientific and technological enterprise. The scientific revolution was an intellectual revolution and taught people to think differently. In Butterfield's (1952) view the scientific revolution *"outshines everything since the rise of Christianity and reduces the Renaissance and the Reformation to the rank of mere episodes."*

The computer revolution or "the second industrial revolution" as it is sometimes called, promises to wreak more changes in the way in which work is carried out and information processed in a modern society. The first primitive electronic computers appeared in the late 1940s and ushered in an era of unprecedented scientific and technological development, which we are still very much immersed in today. This new age of sophisticated nuclear weapons, supersonic transport, lunar and planetary exploration, heart transplants, and smallpox eradication brought with it the inevitable anti-science movement (including computer-phobes), but the vast majority of people, even if a little apprehensive about contemporary and future science and technology, are tolerant and wish to learn more. They yearn to understand the basics of DNA, ecology, in vitro fertilisation, microcomputers, malignant tumors, genetic engineering, environmental pollution, sunrise industries, VLSI, etc., and be in a position to appraise the social, cultural and political consequences of scientific and technological innovations.

SCIENCE CENTRES

Many people assuming a public information role for science - technology museums turn to them for help but few came away satisfied. It is to that silent majority who wish to know about contemporary science and technology that the science centre movement addressed itself. The science centre movement which began in the United States has spread like a bush-fire to countries like Canada, Singapore, Japan, Hong Kong, France, etc. (Davids, 1982; Wilson, 1976; Bhathal and Tian, 1980; Japan Science Foundation, 1964). Japan and America are probably the only two countries in the world with the largest number of science centres which have been set up to ensure that they not only have a continuous supply of scientific personnel for the smooth functioning of the various sectors of their industrial economies but a scientifically literate public which perceives science and technology as the foundation of a modern industrial nation. (Bhathal, 1975). In this respect it is interesting to note that American global prestige and influence is given first to American technological know-how by Americans. The American public attributes U.S. influence in the world to its technological know-how and not to its achievements in the arts. (Martin, 1982).

The Museum of Science and Industry, Chicago, has been the driving force behind the science centre movement in the United States.

According to Danilov (1976, 1982) the characteristics of these new institutions are as follows:

(a) the emphasis is on contemporary science and its technological applications and implications, rather than, on historic objects from the past.
(b) rather than a 'hands off' policy, they encourage the public to touch the exhibits, and to interact by pushing buttons, turning cranks, lifting levers and listening to taped messages on telephones (or other AV equipment).
(c) the exhibits are usually education orientated, with many of the exhibits explaining scientific principles and processes instead of being mere displays of objects.
(d) they offer numerous special educational programmes - many of them of an innovative nature - to supplement the formal offerings of local schools and community organisations.
(e) the exhibits are usually displayed in a thematic form.

The key to the success of science centres is the participatory nature of the exhibits. (Bhathal, 1979; Oppenheimer, 1972, 1975; Bhathal, 1981). In studying the relatively short history of science centres it will be found that perceptions of what constitutes a participatory exhibit have undergone substantial changes. The earliest science centres merely contained push button exhibits but in view of their inherent problems these are now viewed as simplistic and have largely fallen into disrepute. Today the term participatory means a much more complex association of visitor and exhibit where the latter offers prospects for manipulation, experimentation and variation, and titillation of as many senses as possible. Establishment of feedback loop (between participator and participatee) is important and this has become easier in recent years with the advent of display gallery computer terminals and micro-computers. (Bhathal and Orchiston, 1981). Thier and Linn (1976), and Screven (1974) have shown that intimate participatory experiences as offered by science centres, enhance communication, precipitate self-discovery and maximize learning.

A study carried out by Koran, et al (1984) revealed that a significant number of museum visitors preferred the manipulatable setting when they were confronted with identical manipulatable and nonmanipulatable exhibits in a "free choice" environment. They also found that children preferred to engage in hands-on experiences significantly more than adults. Female children and female adults preferred to engage in hands-on experiences rather than the alternative. Their studies confirmed the basic philosophy of the science centre movement.

The implications of this study for inducing young girls to consider taking up careers in science and technology are rather interesting. The Meyer Report (1980) noted with some alarm that *"while more girls than boys satisfy tertiary requirements, they concentrate on subjects that are not technologically oriented. More than 25% of boys satisfied tertiary entry requirements with two mathematics, physics and chemistry, but only about 6% of girls did so; about 45% of boys did with at least general mathematics compared with less than 20% of girls."*
As a consequence of this we see less girls in the physical sciences and engineering faculties. Although 44% of students in Australia are females, only about 2% of these are in engineering. Figures indicate that less than 0.5% of Australian professional engineers are women as compared to 15% in France, 10% in America and over 40% in Russia where over 25% of inventors are also women.

It may be that if we want more young girls to take an interest in science and technology then the science centre type hands-on exhibits could be one way to attract them. By playing with scientific and engineering exhibits in a non-threatening science centre type environment there is every possibility for them to loose any real or imagined fear of the things of science and technology. This will hopefully encourage them to consider a career in the scientific and engineering fields.

Other studies carried out at the science centre in Singapore and some of the American centres showed that the most common variables to measure in deciding the qualities of a good interactive exhibit were the holding power of the exhibit and the frequency of use. When these variables were plotted it was found that exhibits generally fell into the four following categories:
(a) high holding power and low popularity
(b) low holding power and low popularity
(c) high holding power and high popularity
(d) low holding power and high popularity
Except for those which had low holding power and low popularity, all the other exhibits had some good features. Those which had high holding power and high popularity offered the visitor with an opportunity to make choices or to manipulate variables. These were usually multi-sensory. It was found that those exhibits which permitted manipulation of variables had visitors around them for a much longer time. For example, computer terminals and live animals had a greater holding power than push-button exhibits. Static exhibits were at the bottom of the scale. A multiplicity of variables to manipulate increased the opportunity for multi-sensory experience and this allowed people to approach exhibits on their own terms. Another aspect of a good exhibit was that it acted as a prop to link a pedagogical chain or it could be used in many different contexts. For example, the Relative Motion Swing (or Sand Pendulum) exhibit which has a swinging table beneath a pendulum of the same period, can be used in many contexts. One can use it in talking about Lissajous figures, about phase, amplitude and frequency, about damping, about kinetic and potential energy, about frame of reference and relative motion and polarized light. People use this exhibit in many different ways because they have a number of options to choose. Some just give the table a push as they walk by. Others very systematically let the table and the pendulum swing at right angles to each other, and try to reproduce the indicated circles and diagonal lines of relative motion, and in the process learn about relative phases and amplitudes by trial and error. Others who know all about pendulums use it to instruct their friends or children. The Bernoulli Blower which has an air stream to support a light-weight beach ball has several possibilities. The Bernoulli effect is strong enough so that one can feel an appreciable inward force if one tries to pull the ball out of the stream. People tap the ball and watch it oscillate in the air stream; they partically cover the orifice with a hand or direct the stream to one side; they remove the ball and try to throw it so that it is caught by the air stream. They can also relate it to an experiment on airflow over an aerofoil and thus understand why an aeroplane flies or capture some of the excitement that Hargrave felt when he experimented with airflow over curved surfaces which he reported to the members of the Royal Society of New South Wales. (Hargrave, 1893). Of course, some visitors do irrelevant things such as having their hair stream up in the air current or hold their shirts over the orifice and cool their bellies in the air stream.

The studies also indicated that as novelty and complexity increase, the attention span and number of visitor questions tended to increase. Attracting attention and evoking curiosity were two worthwhile objectives for any science centre exhibit. Science centres have adopted both a 'hands-on' and a 'minds-on' approach to the display of science and technology concepts, information and processes. They see themselves as institutions for the dissemination and 'popularisation' of science and technology to the general public. They also see themselves as demystifying science and technology and for making the new applications and implications of science accessible and understandable to the public. (Bhathal, 1981). The three dimensional interactive exhibit allows the visitor not only to get an intuitive grasp of scientific concepts but also to mix learning with pleasure. A science centre visitor is free to linger and backtrack, to explore items of particular interest and to pose questions and search for answers, something which the visitor is unable to do with a radio or television programme. By interacting with the exhibits the visitor is able to learn science through exploration and discovery. This is a much more preferable and easier method of studying scientific subject matter than the normal print-oriented newspaper article or the radio or television programme. It has also been found that the market penetration of the programmes of a science centre type institution is between 15 and 20% of the population as compared to less than 5% for science articles in the print media, or radio and television programmes.

In fact, in a science centre, science and technology have a high profile as compared to science reporting in the press or radio/TV media. An analysis of science reporting in the Sydney Morning Herald for the period March to July 1982 revealed that only about one page per month was devoted to science as compared to daily coverages of half to three-quarters of a page on the arts. It is, to say the least, astounding that the Sydney

Morning Herald can't find enough stories daily to fill its pages in a field of human endeavour that is changing so rapidly and multiplying knowledge daily.

In the case of radio, an ABC Audience Research survey carried out in Melbourne in 1978 showed that in a sample of 3760 homes there were 192 Science Show listeners or about 5 per cent of the random sample.

In a science centre type institution people can bring their own agendas and explore their special areas of interest in an interactive way. The science centre provides visitors with memorable three-dimensional visions and interactive experiences in a learning environment. Learning in a science centre is basically a visual and tactile experience which is qualitatively different from the kind of learning which results in a formal educational system. (Lee, 1978; Bhathal, 1982).

The success of the science centre movement in the U.S.A. has seen a remarkable shift in the number of visitors moving away from the history and art museums to science centres. In the United States visits to science and science museums with interactive exhibits and modern communication techniques have more than doubled from 14.4 million in 1973 to 36.5 million in 1975. According to a 1974 survey of 1820 institutions conducted by the National Endowment for the Arts in U.S.A. (1974), 38 per cent of all museum visits were to science centres/museums, 24 per cent to history museums and 14 per cent to art museums. 45% of the visitors to science centres were adults.

CENTRES AND/OR MUSEUMS

Having dealt with the traditional science museums and the new science centres one wants to explore the question as to whether these institutions can be married or whether they should thread their own separate paths. The science centre movement has moved rapidly along its own path while the traditional science museums have generally stuck to their heels. But recently there has been a change as more and more people in the traditional science museum world begin to realise that they cannot ignore the phenomenon of the science centre movement and the demand by the lay public to get information on developments in science and technology in a public institution. And this demand grows louder since it is the tax-payers money that is being used. Responsible participation in a democracy requires continuing education in science and technology. If politicians are to make informed decisions and the public is to support science and technology then science must be explained to the public in a public institution.

There are moves in a few countries to bring together the two concepts, i.e., the tradition of the traditional science museum and the modern science centre hands-on philosophy, into one institution. However, the going has not been easy. This is partly due to the fact that in the museum world there are two cultures. There are people who think artefacts are only important in the context of people and ideas. These two groups have a difficult time in communicating to each other. The problem is further compounded by the fact that there are some that actively dislike the public and others who would like education staff to keep school children out of the sanctified galleries. There is also the education/entertainment dichotomy in museums. This is reflected in the countless visitor surveys which separate "entertainment" and "education" as reasons for visits. Museum professionals are constantly arguing about the values of education versus entertainment. This is in contrast to studies which indicate that education was not an explicit objective of visitors and that the education - entertainment dichotomy was not recognised by visitors. The expectations of most visitors were varied and complex. However, they generally revolved around an expectation that their children would enjoy themselves. Children do not separate experience from learning. If you ask ten year olds what they learned, they will tell you what they did. Play and learning are intertwined in the child's mind. Children's museums and science centres are very popular with children and adults because they recognise the relationship between play and learning and provide multi-sensory experiences.

The National Air and Space Museum in Washington tried to combine the traditional object-oriented philosophy with the new thematic and interactive science centre approach in its aviation displays. It has used the most up-to-date communication techniques in an interesting and subtle way but much remains to be done. It has still to mount displays which bring to the public in simple understandable terms the recent advances in avionics, space technology and aviation materials technology. Basically its emphasis still remains on the traditional science museum philosophy. (Bryan, 1979).

In England the transition from the traditional museum to a modern one has not been easy. The battle is still going on. According to Miles and Alt (1979) a few years ago the British Museum of Natural History in London faced a major problem regarding its galleries. By the mid 1960s the Museum had reached a dead-end in its attempts to develop and improve the exhibitions. The exhibitions gave a narrow-based and incoherent view of biology. No attempt was made to explain the significance of organisms in nature. The Museum was suffering, so to say, from the "Dinosaur syndrome". Out of this self critical look came the new exhibitions on Human Biology, Introducing Ecology and Man's Place in Evolution. The change was not accepted gladly. There was and still is a bitter division of opinion over the proper role of such an institution. (Griggs, 1984). Do people visit a science museum to gaze in awe at a venerable exhibit? Or do they go to be excited, perhaps entertained and instructed and learn about recent advances in biology which affect their daily lives.

Dixon (1980), the former editor of the New Scientist, has answered the critics of the exhibition policy of the British Museum of Natural History rather succinctly when he wrote in a recent issue of the Sciences:
"You can walk into the Museum (i.e., the British Museum of Natural History) *today and be excited and instructed by models of DNA. The most recent departure, a display illustrating energy flow through food chains, is by far the most imaginative and enchanting presentation of ecology that I have*

encountered. Museums, like Zoos, should be more than just depositories for older objects. They should entice, entertain and inform us too. I have little doubt which approach is more likely to inspire a future Darwin."

Rather than trying to convince the Science Museum in London to develop interactive science exhibits in its display galleries, Gregory (1983), an ebullient professor of psychology at the University of Bristol has assembled together a group of educators and engineers to develop Britain's first science centre the Exploratory in Bristol. The idea for the Exploratory has its origins in Francis Bacon's uncompleted book New Atlantis, published in 1627. In the New Atlantis Bacon speculated about a "House of Salomon" crammed with experiments and natural wonders designed to tease the curious. Typical of the exhibits in the Exploratory is an elliptical billiard table: a ball struck from one particular position will always hit a ball in another, no matter how hard it is struck or in what direction. The message is that an ellipse has two "centres", whereas a circle has one. Another is a revolving chair, in which a child sits with a spinning bicycle wheel in his or her hands. Turning the wheel from vertical to horizontal speeds up the rate at which the chair spins: the principle of the gyroscope.

Not to be left behind by these new developments in Britain the message has finally got through to the staff of London's Science Museum. It now plans to set up on an experimental basis a new set of interactive exhibits called "Launchpad". (Wilson, 1984). These will be opened to the public within the existing Museum in 1986.

In France, in 1980, the former President Giscard d'Estaing set up a committee headed by Professor Maurice Levey to look into the reorganisation of one of the oldest and largest science museums in France, the Palace of Discovery. A report in Nature by Ritter (1980) noted that the Museum
"through lack of funds and support, largely degenerated into formal lecture-demonstrations by underpaid staff to audiences increasingly made up of reluctant school children and middle-aged professionals. Meanwhile the Conservatory of Arts and Crafts, set up at the time of the Revolution to be a collection of all the originals of instruments or machines invented or perfected in France, has remained a hodgepodge of machines ...."
Arising out of this report the French Government has approved plans for building one of the largest science centres in the world at Parc de la Villet in Paris. It will occupy an exhibition space of 400,000 sq. ft. and cost 4.4 billion francs in 1982 prices. It will attempt to combine the traditional exhibits with science centre type interactive exhibits in a number of contemporary themes such as energy, ecology, exploration of space, microelectronics, computers, etc.

In Australia there is a growing awareness of the need to inject the interactive exhibits philosophy of science centres into the traditional museum world. Science centres aim to show how science and innovation work in an intellectual and social climate. To achieve this, scientists, technologists, designers and educationists draw on techniques which involve amusement, game-playing, aesthetic feeling or anything related to our normal life. The exhibits tend to be chosen to illustrate scientific ways of thinking, important scientific themes, and relations between science, aesthetics and social and technological progress.

Science centres tease, excite, entice, provoke thought and motivate the visitor. The interactive nature of the exhibits provides the visitor with an insight into the real world of science and the scientist, and of how the latter works, thinks and explores the secrets of nature. The methods of interaction involve all the senses and thinking abilities of the participants.

Today, there are a few institutions in Australia that have begun the process of assimilating and trying out the ideas of a science centre type institution and attempting to answer some of the questions posed by the lay person who wants to know about modern science and technology and its impact on society. It is rather surprising to note that the push to introduce the science centre movement into Australia has come mainly from the universities rather than the museums. The museums have tended to jealously guard their traditional ways of thinking and doing things. One of the first experimental science centres to be set up in Australia was under the auspices of the physics department of the Australian National University. Named the Questacon, it was organised by Gore (1981), a physicist with the University. It has been in operation for about 4 years. Many of the exhibits there are similar to those found in overseas science centres such as the Exploratorium in San Francisco, the Ontario Science Centre in Canada, the Singapore Science Centre in Singapore, etc. Visitors to the Questacon can check out the Archimedian buoyancy principle, observe stress patterns in acrylic models placed between crossed polaroid filters, do an experiment with microwaves, etc. Essentially visitors can learn some of the principles of physics in a non-threatening environment. It is, however, a fairly small science centre and caters mainly to school children. It has recently been opened on some occasions to the general public.

Another institution, called the "Supernova" is in the process of being set up in Newcastle. The move is being spearheaded by members of the Faculty of Science of the University of Newcastle, in particular Edwards (1984) of the School of Biological Sciences. They are working with the Hunter Development Board and hope to obtain financing through the Steel Industry Assistance Programme for the Hunter Region. A feasibility study (Wills, et al, 1984) has been completed and submitted to the Government for consideration. If approved the Supernova will be the first fully fledged regional science centre in Australia.

Because of the tyranny of distance and the fact that technological change is hitting people most in some of the regional urban centres such as Wollongong, Newcastle, etc. it is becoming clear that governments and opposition parties should begin to develop policies for the setting up of full fledged science centres not only in their capital cities but also in the regional centres of population to disseminate information on new

science and technology to the public. The economic and social pay offs in the long run will certainly benefit the community at large.

In Melbourne the reorganised Museum of Victoria has begun its first forays into the world of science centres by having an exhibition called Experilearn. It consists of a series of "hands-on" exhibits on physics which allow the visitors to explore several principles of science. The exhibits are based on those found at the Exploratorium in San Francisco. Visitors can play with the exhibits to find out that white light is made up of red, green and blue colours, why an aeroplane flies, what is the difference between transverse and longitudinal waves, etc. The whole idea of the exhibition is to let people have fun and at the same time to discover for themselves some of the basic principles of science. The exhibition is a collaboration between Monash University physicists and educators and the Museum of Victoria. Although it covers only a very small exhibition space in the galleries of the Museum, it has certainly helped to bring a breath of fresh air and new ideas in the communication of scientific principles to the public through the medium of a museum.

In Sydney the new Power House Museum is in the process of imbibing the exhibits philosophy of science centres. It will, according to the Premier of New South Wales, Wran, "be one of the finest and most exciting of its kind in the world". The new Power House Museum is attempting to combine the traditional museum exhibition philosophy with the exciting "hands-on" science centre type interactive exhibits approach not only in its traditional science and technology displays but also in its historical and decorative arts displays.

The Australian Museum in Sydney has recently set up a small group to develop interactive exhibits in the area of biological sciences. This is a major departure for a museum which until now has tended to be the preserve of the traditional museum curator. It is to be hoped that the museum will develop exhibits which will explain some of the modern developments in the biological and medical sciences which have a direct impact on the lives of the citizens of New South Wales and Australia.

Despite these attempts to introduce the science centre type philosophy in their exhibitions the traditional museums in Australia are faced with severe restrictions and problems. These museums have a mandate to collect, conserve, restore and display old technology. At the best these traditional museums will only be able to devote less than ten per cent of their space and resources to disseminate information on contemporary science and technology through the science centre type interactive approach. They will not be able to devote the necessary time, effort and resources to show and explain the principles, applications and implications of the latest advances in science and technology on the grand scale that is found in science centres in overseas countries, countries which are industrial competitors on the world markets. It would thus appear that from a logistic and realistic point of view Australia will need to develop separate full fledged science centres and wherever possible try to inject the science centre type philosophy in the displays of the traditional museums.

An ambitious project at the national level will be the setting up of a National Science Centre in Canberra as a lasting memorial to Australia's bicentenary in 1988. Just as the artistic community has a National Gallery to show the best works of Australian artists in the national capital so will the National Science Centre (apart from its displays on scientific principles) show the best examples of modern Australian science, technology and engineering. It will show the Australian public that the basis of our society and our survival in the extremely competitive world lies in the appreciation of the strengths and weaknesses of science and technology. It will also show the Australian public that the nation recognises the endeavours of the many creative men and women who have taken up careers in science, technology and engineering. A national public institution is the best place to show the works of the scientific and engineering community in Australia. With the use of interactive exhibits their works can be explained in simple terms to the lay public.

A national committee which was set up by the Australian Bicentennial Authority and the Federal Department of Science and Technology in Canberra, has prepared a report (A National Science Centre for Australia, 1983) on the establishment of a National Science Centre in Canberra as a bicentennial project. The setting up of a national science centre to serve as a leading example for the dissemination of science to the public is extremely important at this crucial stage of Australia's economic and industrial development. Children who are still in primary schools in Australia today will grow up in a world dominated by computers and high technology. The science centre approach will provide the best opportunity for these children to obtain direct hands-on experience in areas of technology such as energy, computers, bio-technology, robotics, communications etc. They will begin to understand the power and limitations of high technology in solving some of the problems of industrial society. As a result of their access to a science centre and museums which use the science centre approach they will begin to realise that to live in an industrial society they must accept change, which at times, will be rapid and will change their life style profoundly.

Australia needs to nurture and establish full fledged science centres and inject the science centre approach into its traditional museums if it is not only to have a well informed public but also to keep up with its industrial competitors who are moving into high science and technology. It will also ensure for itself a scientifically literate public which will be able to see the issues where science and technology impact on society more clearly. Australia can also expect to have a constant supply of creative young people who will take up careers in science and technology and thus provide the necessary ballast for an economic and industrial take-off in the 21st century. Australia will then be in a position to take on the responsibilities of a major power in the Asian-Pacific region which has been described as the major growth area in the coming decades. If it does not create these new institutions for the dissemination of the results, advances and future possibilities of science and technology to the younger generation and the general public then it will gradually go down the road of technological obsolescence.

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Dr. R.S. Bhathal,
Power House Museum,
P.O. Box K346,
Haymarket, N.S.W., 2000,
Australia.

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Proposed Physical Mechanism Linking Changes in Solar Activity With Some Aspects of the Weather

V. KASTALSKY, C. B. KIRKPATRICK AND A. T. DAOUD

ABSTRACT. The proposed mechanism is based on a series of processes related to solar cosmic rays and is shown to be consistent with present observations and findings. The mechanism proposes that stratospheric ozone is partially destroyed with consequent fluctuations in temperature near the tropopause. These fluctuations are suspected to have drastic effects on the weather and hence the understanding of the proposed mechanism could lead to improved weather forecasting. Detailed elaboration of the mechanism will require the efforts of a multidisciplinary group.

INTRODUCTION

Despite an enormous increase in the density and frequency of meteorological observations, more accurate measurements, satellites, and advanced computational power, the quality of weather forecasting has not been comparably improved (see, for example, Ramage, 1976). One of the reasons could be that Middle Atmosphere data were considered not important and the measurements there were neglected. Fortunately, the International Middle Atmosphere Program (MAP) project will soon provide them.

Another reason is that current meteorological theory and methods for interpretation of the available data are inadequate. In our view, any satisfactory theory leading to better weather forecasting must include consideration of the influence of solar activity on the weather, in particular, the role of solar cosmic rays and the processes occurring near the tropopause. For over 100 years attempts have been made to link changes in solar activity with the weather (see, for example, remarks in Foreword to Herman and Goldberg, 1978). As recently pointed out by Akasofu (1983), the tendency towards increased specialisation in various disciplines tends to create a gap among interrelated disciplines. According to him "this is particularly serious in solar-terrestrial physics".

We propose a mechanism linking solar activity with the weather via the tropopause. This mechanism causes coupling effects whereby processes in the lower stratosphere activate the upper troposphere and hence influence the weather. Inclusion of considerations such as the above in meteorological theory could lead to better forecasting.

DESCRIPTION OF THE PROPOSED MECHANISM

The proposed mechanism can now be formulated as follows:

At certain phases of variable solar activity there appears in the stratosphere a considerable increase of the electron flux. Two kinds of electrons are considered at this stage of investigation:

(1) secondary electrons generated by very energetic solar protons penetrating right through the stratosphere,

(2) precipitated relativistic electrons affecting directly only upper stratosphere but generating bremsstrahlung X-rays which influence the lower part of the stratosphere.

As a result, large amounts of NO are produced causing the destruction of a significant part of the stratospheric ozone followed by perturbations of temperature distribution in the stratosphere.

There is a definite indication that even very small temperature disturbances above the tropopause will be followed by rather drastic changes in wind velocity. As a result some of the laminar stratospheric air adjacent to the tropopause will be transformed into turbulent tropospheric air, - accompanied by a vertical pressure wave and by changes in vorticity and consequently by displacement in the pressure field with obvious meteorological consequences (for the relevant mathematics see the Appendix).

By short term effects caused by solar flares we refer to related events occurring not more than a few days after a solar flare. We consider short term effects in the earth's atmosphere to be fundamental in an initial approximation of the mechanism. Long term effects, some of which could be due to accumulation of short term effects, are considered of secondary importance.

The proton component of primary solar cosmic rays is 83% to 89% (see Sandström, 1965). So, in the initial stage of the mechanism, processes related to solar protons must be very important. In the following paragraphs we provide some more detailed explanation of the processes involved in the proposed mechanism and we consider experimental and theoretical evidence supporting our ideas.

THE SOLAR ACTIVITY - STRATOSPHERE LINKS

Stratospheric temperature perturbations related to the proposed mechanism are caused there by destruction of ozone by NO, via the well-known pair of catalytic reactions:

$$NO + O_3 \rightarrow NO_2 + O_2$$
$$NO_2 + O \rightarrow NO + O_2$$

Now, as was convincingly described by Crutzen, Isaksen and Reid (1975), particularly large quantities of NO are produced by secondary electrons, generated by very energetic solar protons resulting from certain solar events and propagating right through the stratosphere. The results of their work show that the rate of production of NO by galactic rays is about 100 times smaller than that by solar protons and will be neglected at least at this stage.

Moreover, from the date of Nimbus 4 during three solar proton events in January and September 1971 and August 1972, ozone profiles were calculated (McPeters, Jackman, and Stassinopoulos, 1981). Ozone depletion due to the above events in the upper stratosphere was 10 - 30%. This depletion exceeded predictions by the usual atmospheric chemical models.

Another source of large amounts of NO in the stratosphere, also related to solar activity, was indicated by Thorne (1977). As he pointed out substantial numbers of relativistic electrons are precipitated (REP events) during periods of geomagnetic disturbances. Although such electrons can directly influence only the upper part of the stratosphere, the bremsstrahlung X-rays generated by them can penetrate to as low as 30 km. As a result, significant amounts of NO are produced. The vertical transport of NO, generated in this way, increases its content still lower in the stratosphere. Thorne also indicated that the NO production by the bremsstrahlung X-rays should predominate over that by galactic cosmic rays, provided that REP events occur more than a few percent of the time.

It is well known (see, for example, Manabe and Strickler, 1964) that ozone is the main contributor controlling temperature distribution in the stratosphere. Moreover, when the total amount of ozone varies, its vertical distribution varies more markedly near the tropopause (Manabe and Moller, 1961). One would expect, therefore, to have significant temperature disturbances near the tropopause due to the above processes.

These temperature disturbances constitute the essential part of the next stage of the mechanism namely, the stratosphere-troposphere coupling. We now present some theoretical considerations which are relevant to this part of the mechanism.

THEORETICAL CONSIDERATIONS

The basic equations for the motion of the Earth's atmosphere show that when air parcels move in one fixed direction with the same speed very small horizontal temperature gradients may give rise to very strong winds. For example, in the upper troposphere at latitude 30° a horizontal temperature gradient of 0.13°C per 100 km can produce a wind speed of 100 km/hour. Also a very small change in the angle (α) between the directions of the geo-potential gradient and the temperature gradient may correspond to a large change in air speed. So slightly different values of the angle α below and above the tropopause could be accompanied by large differences in the speed and direction of the corresponding winds and their vorticity.

If the density of the air at each point varies with time the wind velocity may have an appreciable vertical component accompanied by a pressure wave thus causing the tropopause to rise, as happens after a solar flare.

The reader interested in some mathematical details of these considerations is referred to the Appendix. Because of the complex situation in the tropopause region, the mathematics applied to it should, as closely as possible, reflect the actual physical conditions. We think, for example, that in order to take properly into account the effect of the wind shear and the genesis of atmospheric vortices at the tropopause, it is necessary to express the pressure not just as hydrostatic pressure but as a pressure tensor.

THE STRATOSPHERE - TROPOSPHERE COUPLING

From the beginning of this investigation (Kastalsky, 1972) the importance for weather analysis of the tropopause region, where lower stratosphere and upper troposphere are in a condition of quasi-equilibrium, was discussed. As Danielsen (1959) pointed out, the usually accepted and oversimplified picture of the tropopause arises from the smoothing of the observational data in routine processing; this smoothing is, however, not necessarily warranted by the actual signal to noise ratio of the measuring technique. Apparently this situation still exists despite improvements in measurements.

Consider, for example, conclusions of the following investigations:

(1) The potential vorticity conservation law does not hold in certain examples of meteorological discontinuities where, in fact, large values of potential vorticity are actually created (Freeman, Portig, Graves and Hanna, 1965).

(2) The origin of fluid vortices are attributed to conditions similar to those existing at atmospheric temperature inversions (Heighes,1968).

The above conclusions can certainly be applied to the tropopause since it is effectively a permanent meteorological discontinuity extending essentially over the globe and it is, of course, a site of temperature inversion.

(3) "The stratified medium can generate vorticity as soon as it is disturbed from its equilibrium position" (Meng, 1978).

Now the tropopause region is a stratified medium where disturbances of equilibrium between stratospheric and tropospheric air adjacent to the tropopause could be associated with the commencement of a cyclogenesis.

(4) Reiter (1976) suggested that cyclogenesis could be triggered by cooling processes above the tropopause and this is in agreement with our mechanism.

(5) The next investigation under consideration analyses winter conditions in the years 1964 - 1971 "showing that wintertime 300-mb troughs in the westerlies (cyclonic waves) that en-

tered the North Pacific area from 2 to 4 days after a major geomagnetic index rise were likely to undergo significantly greater deepening than troughs entering the same region at other times" (Roberts and Olson, 1973).

It should be emphasised that:

(a) The 2-4 days delay in the tropospheric response following geomagnetic disturbance could be due to the delay required for vertical transport of NO produced by an REP event.

(b) This is a short term effect.

(c) The 300-mb level is the level of the tropopause region at the latitude under consideration.

(d) The authors measured the trough development by means of the so-called vorticity area index, defined as the area for which the absolute vorticity exceeds a certain value.

The above results tend to confirm the previous ones (Macdonald and Roberts, 1961).

These conclusions indicate that there is a definite possibility of drastic changes in vorticity distribution near the tropopause due to temperature fluctuation above it, caused primarily by processes related to changes in solar activity.

We know that the stationary or progressive character of the air motion is determined by factors to be found in vorticity distribution and that displacement of the pressure field is only a secondary effect, as expounded by Rosby (1940). This explains the reason for changes in the movement of pressure systems and even in the configuration of pressure patterns by creating, for example, a so called upper tropospheric trough so often upsetting weather forecasts.

SHORT TERM EFFECTS

For the initial approximation of processes related to the proposed mechanism we consider short term effects as the most important ones. There is a strong indication from observational material that this is so, namely:

(1) There is evidence (Schuurmans and Oort, 1969) about changes in the stratosphere within six hours after a solar flare, the maximum response occurring near the tropopause, the tropopause rises and its temperature decreases.

We conclude that:

(a) We are dealing with short term responses.

(b) The tropopause region is of particular importance.

The above authors also indicate that these responses can be caused by the action of very energetic solar particles.

(2) Kulkarni (1968) states that: "Ozone and the tropopause pressure are not very well correlated on a seasonal basis although there is a good correlation between a short term variation of ozone and the tropopause height."

He did not consider the solar activity at all and tried to explain the situation in a different way but these correlations support our mechanism.

(3) The paper by Roberts and Olson mentioned above gives another example of short term response near the tropopause to geomagnetic disturbance (also related to changes in solar activity). This time the delay is of the order of 2 to 4 days. Roberts and Olson consider the important changes in vorticity as a measuring device for this response.

SOLAR ACTIVITY - TROPICAL CYCLONES

As the next step of our investigation we intend to study the actual process of formation of original atmospheric vortices near the tropopause where, as is well known, most if not all, major atmospheric disturbances, including tropical cyclones, start. In particular, the formation of the original vortex near the tropopause is the first necessary condition for the genesis of a tropical cyclone, supply of energy from turbulent tropospheric air being the second necessary condition. According to present meteorological theory this energy is supplied from warm air-sea interaction.

Without rejecting the influence of tropical conditions we think that the real situation is more complicated. Why, for example, are some very strong winds coming from polar regions to mid latitudes approaching the strength of tropical cyclonic winds? From our point of view the behaviour of tropical cyclones indicates a complicated interdependence of conditions in the lower troposphere with processes related to changes in solar activity.

Significant correlations between solar activity and different aspects of tropical cyclones were known as long ago as 1874 (as reported by Meadows, 1975). From solar information supplied by the Australian Department of Science in Sydney and data on tropical cyclones compiled by Miss Carol Bailey, from Coleman (1971), a few significant correlations, similar to ones obtained previously by other authors from the Indian and Atlantic oceans, were obtained. Moreover, we have found two very interesting correlations.

Firstly, Fig. 1 indicates an increased frequency of magnetically disturbed days before the start of a hurricane, the maximum occurring three days before its onset. There is a striking similarity between these results and those obtained by Roberts and Olson (as mentioned above).

This leads to quite a definite possibility that the deepening of atmospheric troughs near polar regions and the deepening of low pressure regions down to a hurricane in the tropics are related to geomagnetic disturbances caused by processes related to changes in solar activity.

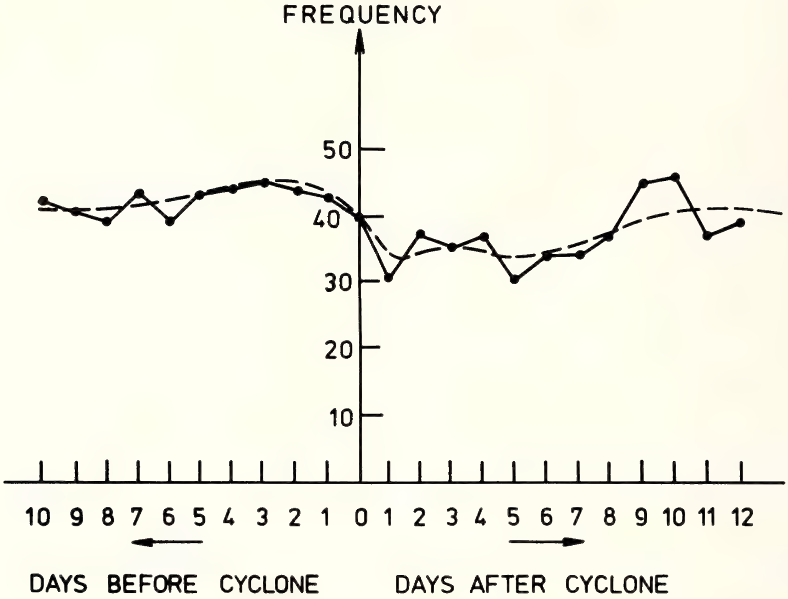

Fig. 1. Plot of frequency of magnetically disturbed days relative to the occurrence of cyclones in the Australian region (as averaged for 131 cyclones for 14 years between 1956 and 1969.)

The second interesting correlation refers to certain very puzzling aspects of tropical cyclone propagation: Why, despite accurate measurements of all kinds of atmospheric parameters does there appear to be no explanation and no possibility of prediction of sudden unexpected and drastic changes of the direction of propagation of hurricanes?

To check the possibility of this phenomenon being caused by processes related to solar activity the ratio of the number of such drastic (over 90^{o}) changes in direction of propagation of hurricanes to their total number in the Australian region over the period 1950-1969 is compared with corresponding sunspot numbers in Fig. 2.

It is interesting to note that unsmoothed data (curve 2) seems to contradict smoothed results (curve 1) when the maximum on curve 2 in 1965 coincides with the minimum on curve 3 (smoothed annual sunspot number) and very close to a minimum in curve 1. However, it is just the opposite - the combination of both results explains better what is really happening. It confirms that these drastic changes of propagation of the hurricanes are associated with the processes related to solar flares. Since flares do happen during both high and low solar activity so they should be followed by a proper response of individual cyclones. The smoothed curve, however, underlines the simple fact that there are more flares during the sunspot number maximun than during the minimum.

This example indicates also that some of the condradictions put forward by objectors to solar activity - weather relationships are more apparent than real.

At the beginning of our project the influence of solar activity on tropical cyclones seemed to be under suspicion. The question was:

How the solar cosmic rays could possibly penetrate the earth's magnetic field over the tropics?

Recent literature contains experimental evidence bearing on this question. For example:

(a) Neher (1952) found that the relative meson intensity along the geographic meridian 80^{o}W varied from 500 at the Equator to 800 at latitude 64^{o}. The measurements were made with counter telescopes at an altitude of 9 km. and are presented graphically on p.119 in Sandström (1965).

(b) The paper, delivered during the 17th International Cosmic Ray Conference in Paris (Ageshin et al., 1981) - reported the results of the latitude effect measurements for the ionising component of the cosmic rays at sea level. They found that the latitude effect of cosmic rays in the path from Leningrad to the equator is of the order of 10 to 12%.

(c) Sandström, in the fifth chapter of the above mentioned book, points out that even early cosmic ray measurements indicated paths inconsistent with those expected in a centred dipole geomagnetic field. This explains why some of the very energetic cosmic rays can penetrate to sea level.

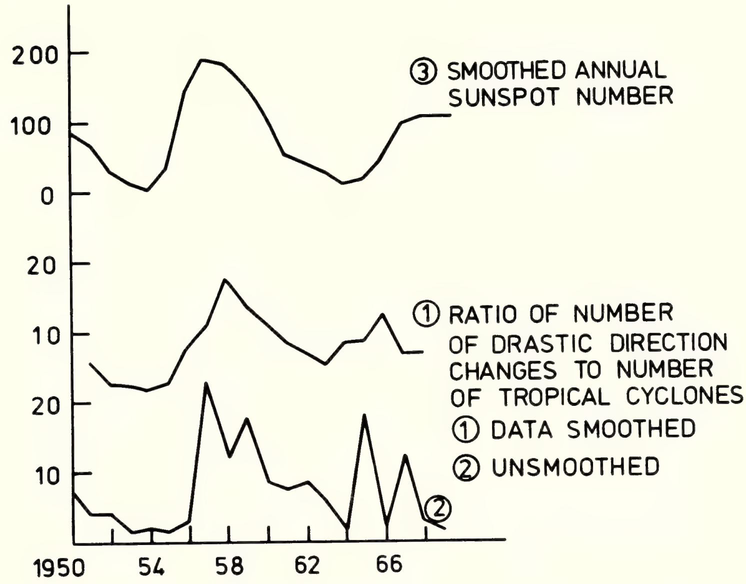

Fig. 2. Comparison between (curves 1 and 2) the time variation in the ratio of the number of drastic (i.e. greater than 90^{o}) changes in direction of propagation of tropical cyclones to the tropical cyclone number in the Australian region over the period 1950-1969 and (curve 3) the corresponding time variation in the sunspot number.

Another paper from the above mentioned Paris Conference (Altukov and Charakhchyan, 1981) refers to aircraft measurements of cosmic ray density variations at 7-19 km. They found out that the short period variations predominate during the disturbed geomagnetic period. These results also support our mechanism.

We also suggest an inexpensive "Trial and Error Experiment for Tropical Cyclone Modification" which has a strong possibility of being successful.

From the beginning of this investigation one of the authors (Kastalsky, 1972) has been proposing this trial and error experiment. In particular it has been proposed at the poster session on "International Conference on Tropical Cyclones" (25-29 November 1979 in Perth, Australia).

As in all experiments of this kind there could be no absolute certainty of its success. However, more and more theoretical and experimental physical information about different aspects of hurricanes indicate very strongly the high probability of such a success. This probability has increased especially strongly during the last few years for the following reasons:

(1) C.B. Kirkpatrick, former senior lecturer in Mathematics at the University of N.S.W. who joined this project in atmospheric physics has found theoretical evidence strongly supporting original ideas about the necessity for revision of atmospheric situations where small parameters can exercise controlling influences. In particular, he found that under certain conditions a very small horizontal temperature gradient can cause drastic changes in wind velocity (see the Appendix).

(2) These theoretical results are in agreement with experimental evidence given in the article by Gray (1979). It is shown there that:
"The cyclone's upper-level temperature anomaly and its gradient can be related to surface pressure and wind" and, therefore to the intensity of the tropical cyclone.

Our experiment aims at:

a) Decreasing of the above temperature anomaly and, therefore, the intensity of the cyclone, and

b) Upsetting of (dynamically crucial) frictionally-driven circulation in the eye of the cyclone which maintains the intense storm (see Carrier, Hammond and George, 1971).

One of the suggested ways to do so is to spray the eye of the cyclone just above its upper part with a suitable agent which must be considerably cooler than the core of the cyclone in order to reduce the temperature anomaly and, therefore the intensity of the cyclone.

The spraying will have also a tendency to upset the crucial vertical circulation as well as the nearly adiabatic conditions in the eye.

We would start the procedure of spraying the interior of the eye of a cyclone in the following way:-

(1) In order to obtain best results, we eventually will have to try several different spraying agents. We, however, would start with crushed ice kept at a temperature considerably lower than that in the upper part of the eye of the cyclone (approximately - 70^{o} C).

(2) We would start the spraying before the cyclone is fully developed.

(3) Starting just inside the periphery of the eye the sprayer aircraft would follow a spiral path (as circular as possible) so as to spray an annular region of the eye of thickness say about one half of a kilometre.

(4) Observations of responses of a hurricane to spraying from the aircraft as well as from the boundary layer are absolutely essential.

(5) The more spray a selected aircraft can carry the better are the chances of success and of course the simultaneous use of more than one aircraft may be a possibility.

DISCUSSION

The problem of existence of links between changes in solar activity and the weather has been controversial for many years. One of the main objections is:

How can the relatively small amounts of energy involved in the relevant processes related to changes in solar activity affect appreciably the turbulent, highly energetic lower atmosphere? (See, for example, Burrows, 1973).

Our answer is: Under certain circumstances small parameters can exercise a controlling influence. Examples:-

(1) We have proved that under certain conditions horizontal temperature gradient of 0.13^{o} c/100 km can produce wind speed of 100 km/hr.

(2) Despite the fact that the concentrations of ozone in the stratosphere correspond to partial pressures of some micromillibars as compared with the pressure of this region in the range 300 to 1 mb it absorbs about 5% of total energy received by the earth from the sun and it is a major factor controlling the temperature distribution there.

One of the rather neglected atmospheric regions where certain small parameters can exercise a controlling influence is the tropopause region where comparatively stable stratospheric air is in a condition of quasi-equilibrium with turbulent trospospheric air.

A large number of objectors point out many statistical inconsistencies found in different correlations related to the problem under consideration. The apparent existence of these inconsistencies (which involve essentially long term correlations) was explained by Loginov (1971). He pointed out that both supporters and objectors of a definite atmospheric response to changes in solar activity had presented observational material which on face value could not be faulted. However, the controversy is only apparent. The reason is that solar cycles are considerably more complicated than previously realised, and solar activity is now being characterised not just by the relative number of sunspots but by a number of other indices. One of our investigations (see the earlier explanation of Fig. 2) indicates the real nature of one of such "inconsistencies" mentioned above.

To this we can add another complicating factor, namely, the processes in the lower troposphere (for example, rainfall) can be only partially dependent on changes in solar activity and this should be particularly strongly indicated in long term correlations. One would not expect, however, very significant interference from processes not related to changes in solar activity during atmospheric responses to short term effects.

In the summary of the recent report "The Future of the Nation's Weather Services" by the U.S. National Advisory Committee on Oceans and Atmosphere (NACOA, 1983) one of the recommendations states: "We find that the new technology of weather observations, communications and data processing is indispensable to the advancement of the Nation's weather services". The question is: Is this recommendation as well as the others, like improved organization, sufficient to improve weather forecasting and modification in order to protect communities from disasters of weather extremes?

Why, despite already existing technological improvements, are there no accepted answers to many meteorological problems referred to mostly as "uncertainties"? For example:

(i) Is ignorance of the processes which form upper tropospheric troughs leading to inaccurate forecasts of the weather?

Some troughs increase their intensity to nearly hurricane levels like the one near Brisbane in Australia in June 1983, causing severe flooding with losses in human lives and destruction of enormous amounts of property.

(ii) Why do tropical cyclones so often drastically change their direction of propagation?

We have definite answers in terms of our mechanism, namely:

(a) Sudden changes in vorticity distribution due to processes related to solar activity (as described above) are causing sudden changes in the pressure system.

(b) There is a definite positive correlation between sudden and drastic changes in the direction of propagation of tropical cyclones both with sunspot numbers and flares (as explained above).

(c) In the Appendix we discuss the dependence of the strength of winds on the magnitude of the horizontal temperature gradient, which in the

case of hurricanes depends on the magnitude of the temperature anomaly as described by Gray (1979). The relevance of our mechanism for certain aspects of tropical cyclones is also supported by Gray (1979). He provided material showing that the intensity of tropical cyclones "is proportional to the magnitude of upper-tropospheric temperature anomaly at the cyclone's center". This anomaly in terms of the positive difference between the ambient temperature and the temperature of the upper part of the eye of the cyclone (300 - 200 mb) increases and, therefore, the intensity of the cyclone also increases when the ambient temperature near the tropopause decreases due to processes related to changes in solar activity (as described above).

It is of interest to add that Dr. R.C. Sheets (1979) from the National Hurricane and Experimental Meteorology Laboratory in U.S.A. mentioned that the intensities of the cyclone and cyclonic rainfall are now known to be independent of each other. This again is in agreement with our approach, intensity being mainly dependent on solar activity and rainfall on air-sea-interactions.

Fortunately, additional necessary data will become available from the International Middle Atmosphere Program (MAP) which will give us a basis for numerical model of the mechanism.

CONCLUDING REMARKS

Some of our ideas originated in 1957 and were revised in 1970. The revised version of the proposed mechanism is now supported by much experimental and theoretical evidence. In order to be able to give adequate warning of forthcoming changes in weather pattern, it is necessary to organize a multidisciplinary group project. This is absolutely necessary because of the extremely complicated interactions between solar and terrestrial processes. We regard our proposed mechanism as an appropriate starting point, indicating the main processes involved in the actual mechanism and the proper direction of future research for the group project.

Needless to say, we will welcome any new participant for any part of the investigation involving the chain of processes between solar and terrestrial surfaces. In our view the detailed elaboration of the proposed mechanism will not only contribute to considerable improvement of weather forecasting but, by elucidation of conditions under which small parameters exercise a controlling influence, will also provide new means for more effective weather modification.

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APPENDIX

THEORETICAL CONSIDERATIONS

Basic Equations:

$$D\underset{\sim}{V}/Dt + 2\underset{\sim}{\Omega} \times \underset{\sim}{V} + \nabla\psi + \nabla p/\rho = 0$$

$$\nabla.(\rho\underset{\sim}{V}) + \partial\rho/\partial t = 0; \quad p = R\rho T$$

$$C_V DT/Dt - RTDL/Dt = DH/Dt$$

in which $\underset{\sim}{V}$ = Velocity; $D/Dt = \partial/\partial t + \underset{\sim}{V}.\nabla$;

$\underset{\sim}{\Omega}$ = Angular Velocity of Earth (7.29×10^{-5} radians/sec.)

ψ = Geopotential = $\int_0^z g(x,y,z')dz'$

DH/Dt = Rate of Heat Intake per Unit Mass

R = Specific Gas Constant for Dry Air

C_V = Specific Heat Capacity at Constant Volume

L = ln ρ(natural logarithm of Density)

$2\underset{\sim}{\Omega} \times \underset{\sim}{V}$ = Coriolis Acceleration

T = Absolute Temperature

p = Hydrostatic Pressure

An important approximate solution of these equations yields

$$\underset{\sim}{V} \approx (\nabla\Gamma \times \nabla\psi)/(2\underset{\sim}{\Omega}.\nabla T)$$

This formula describes a quasi-horizontal geostrophic wind tangential to the geopotential surfaces and at right angles to the temperature gradient. The wind speed V is given by the formula

$$V \approx (g/2\Omega) \sec\theta \sin\alpha$$

where θ is the angle between $\underset{\sim}{\Omega}$ and ∇T and α is the angle between $-\nabla\psi$ and ∇T and $\tilde{g}$ is the constant for gravity at the origin (see Fig. 3).

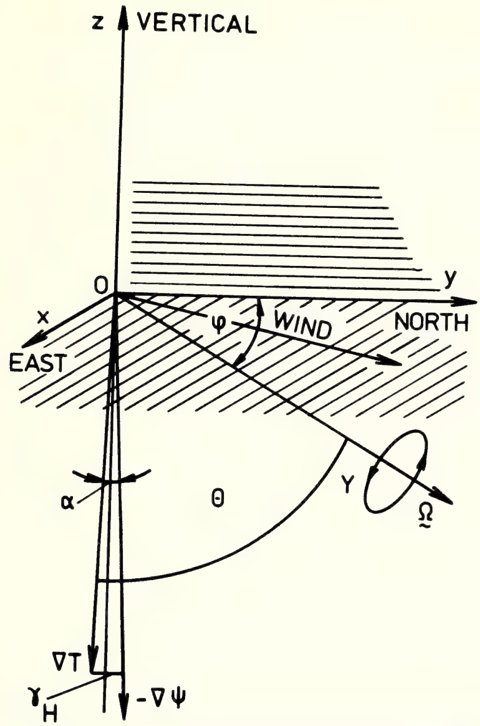

Fig. 3. Diagram showing some important vectors and the local coordinate system.

Now $g/2\Omega \simeq 67$ km/sec, and $|\cos\theta| \simeq |\sin\phi|$, where ϕ = latitude. So, for a wind speed of 100 km/hour and latitude $\phi = 30^{\circ}$ we obtain

$$\sin\alpha \simeq 2 \times 10^{-4}$$

Clearly the angle α is very small even for gales and a very small change in α corresponds to a large change in wind velocity. But

$$\sin\alpha = \gamma_H/|\nabla T|$$

where γ_H is the quasi-horizontal component of the temperature gradient (i.e. tangential to the geopotential). In the upper troposphere

$|\nabla T| \simeq 6.5^{\circ}$C/km so, for latitude 30° and

$V = 100$ km/hour

$$\gamma_H \simeq 0.13^{\circ}\text{C per 100 km.}$$

For undisturbed conditions in the lower stratosphere $|\nabla T|$ is very small but again α must be very small at mid-latitudes so the temperature gradient must be quasi-vertical when this solution of the governing equations is applicable. Also slightly different values of α above and below the tropopause could be accompanied by large differences in the speed and direction of the corresponding winds.

This solution for the speed V is accurate to about 1% and can be made more accurate by not neglecting the variation of gravitation with height which affects the value of the wind speed and gives rise to a small shear in the direction of $\nabla\psi$.

These, of course, are results applicable to special conditions. But even if some restricting conditions are introduced, as, for example, wind shear, and the final value of the wind speed will be many times smaller, this result gives a strong indication that under certain circumstances this small parameter, namely a horizontal temperature gradient can exercise a controlling influence. If wind velocity changes drastically, so must the curl and consequently its vertical component, so called vorticity, that is, even small changes in temperature due to changes in solar activity could cause changes in vorticity.

If these temperature changes do occur near the tropopause the drastic change in wind velocity could change essentially laminar stratospheric flow just above the tropopause into a turbulent tropospheric flow, that is, the tropopause effectively rises which is what happens after solar flares according to observations reported by Schuurmans and Oort (1973).

The above results could, however, have another interesting application not related to changes in solar activity. Because of the direct mixing of the tropospheric and stratospheric air at the polar and tropical tropopause breaks (Fig. 4), the naturally occurring horizontal temperature gradient could be one of the causes and, possibly, the main cause of the formation of jet streams. So called Clear Air Turbulence (CAT) could also be caused by a similar situation at other places of the tropopause region. The inclination of the tropopause to horizontal direction could also play an important role.

In the previous solution it was assumed that the density of air at a point did not vary with time. On removing this restriction we obtain an approximate solution (described below) in which the velocity may have an appreciable vertical component accompanied by a pressure wave as indicated below. This solution is a supporting factor for our explanation of the experimental fact of the tropopause rising after solar flare.

A Solution where $\partial\rho/\partial t \neq 0$

In this solution k_1, k_2, k_3 are constant direction numbers for the temperature gradient using axes Ox(East), Oy(North) and Oz(Vertical). OY is parallel to $\underset{\sim}{\Omega}$

$$\frac{\partial T}{\partial x} = k_1\,\beta T, \text{ etc. } \frac{\partial T}{\partial Y} = \beta T$$

$$T = \exp[J(\nu)] \text{ where } J'(\nu) = \beta$$

$$\nu = k_1 x + k_2 y + k_3 z - wt/(\sin\phi)$$

where $\quad k_2\cos\phi + k_3\sin\phi = 1$

w = vertical velocity (arbitrary)

Geopotential $\simeq gz$ (g constant)

$\underset{\sim}{V} = (u,v,w)$ (a constant velocity)

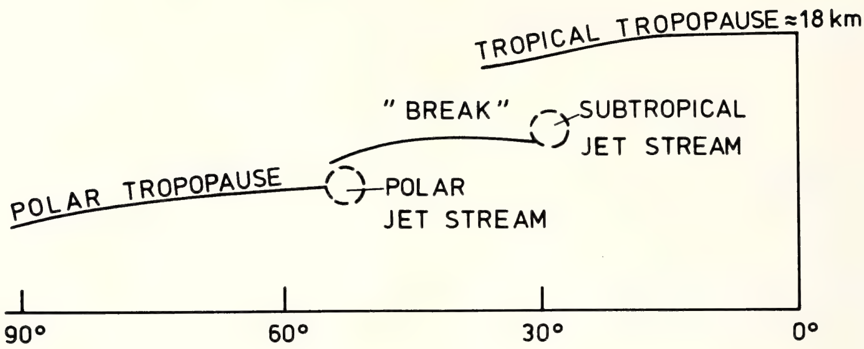

Fig. 4. The tropopause complex in schematic meridional cross-section. One may add the "third tropopause" or a "leaf" above the overlapping parts of the tropical and middle latitude tropopauses (e.g. Haltiner and Martin, 1957, Fig. 23.9, p.426).

East Wind: $u = k_2(g/2\Omega)$

North Wind: $v = w \cot \phi - k_1(g/2\Omega)$

$$\partial p/\partial t = \rho(\beta + \varepsilon/T)w/\sin\phi$$

$$p = p_0 \exp(-\varepsilon \int_0^\nu \frac{d\nu}{T})$$

where $\varepsilon = g \sin\phi/R, \phi$ = latitude.

Velocity of Pressure Waves (etc.) ≈ w if ∇T is quasi-vertical. This solution reduces to the Geostrophic (steady) solution on putting $w = 0$.

V. Kastalsky and C.B. Kirkpatrick,
School of Physics,
A.T. Daoud,
School of Mathematics,
University of New South Wales,
P.O. Box 1,
KENSINGTON, N.S.W. 2033. AUSTRALIA.

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Weber Transform of Certain Generalized Functions of Rapid Growth*

R. S. PATHAK AND H. K. SAHOO

ABSTRACT. It is shown that the classical Weber transform

$$W_\mu[g(x)](y) := G(y) = \int_a^b (xy)^{\frac{1}{2}} C_\mu(xy, ay) g(x)\,dx$$

is a continuous linear mapping from a testing function space $G^\mu_{a,b}$ into $H_{a,b}$ and also the inverse Weber transform W^{-1}_μ is a continuous linear mapping from $H_{a,b}$ into $G^\mu_{a,b}$. An operational calculus is developed and is applied to the solution of certain generalized differential equations.

INTRODUCTION

In this paper we consider the Weber transform G(y) of a function g(x) which is such that g(x) belongs to $L_2(0,\infty)$ and $g(x) = 0$ for $x > b$ and define

$$W_\mu[g(x)](y) := G(y) = \int_a^b (xy)^{\frac{1}{2}} C_\mu(xy, ay) g(x)\,dx, \tag{1}$$

where $C_\mu(xy,ay) = J_\mu(xy)Y_\mu(ay) - J_\mu(ay)Y_\mu(xy)$ and μ is a real number. Obviously, g(x) belongs to $L_1(0,\infty)$ and hence by a theorem of Titchmarsh (1924)

$$W^{-1}_\mu[G(y)](x) := g(x) = \int_0^\infty \frac{(xy)^{\frac{1}{2}} C_\mu(xy,ay)}{Q^2_\mu(ay)} G(y)\,dy, \tag{2}$$

where $Q^2_\mu(ay) = J^2_\mu(ay) + Y^2_\mu(ay)$.

It has been shown by Griffith (1956, 1957) that (1) can be extended to a complex variable $z = y + i\beta$ by

$$G(z) = \int_a^b (xz)^{\frac{1}{2}} C_\mu(xz, az) g(x)\,dx. \tag{3}$$

The following theorem due to Griffith plays a fundamental role in the present investigation.

Griffith's Theorem

In order that g(x) belongs to $L_2(0,\infty)$ and

$$g(x) = 0 \text{ for } x > b > a,$$

it is necessary and sufficient that

(a) $G(y)/Q(ay)$ belongs to $L_2(0,\infty)$;

(b) $y^{-\frac{1}{2}}G(y)$ is analytic in $z = y + i\beta$ for $\beta \geq 0$;

(c) $y^{-\frac{1}{2}}G(y)$ is an even function of y;

(d) $z^{\frac{1}{2}}G(z) = O(e^{(b-a)\beta})$, as $z \to \infty$, $\beta \geq 0$.

We construct a testing function space $G^\mu_{a,b}$ of certain C^∞-functions $\phi(x)$ that are identically zero for all sufficiently large x. We construct another testing function space $H_{a,b}$ which consists of certain entire analytic functions of specified growth. With suitable topologies assigned to $G^\mu_{a,b}$ and $H_{a,b}$, it is shown by means of the Griffith theorem that the μth order Weber transformation is a continuous linear mapping from $G^\mu_{a,b}$ into $H_{a,b}$ for every real μ. An application of this transformation is given to develop an operational calculus involving the operator S_μ.

THE TESTING FUNCTION SPACES $G^\mu_{a,b}$ AND G^μ_a AND THEIR DUALS

Let $b > a > 0$ and μ be a real number. Then $G^\mu_{a,b}$ is the space of all smooth complex valued functions ϕ on $a < x < \infty$ such that $\phi(x) = 0$ on $b < x < \infty$, and

$$\gamma^\mu_k(\phi) = \sup_{a<x<\infty} |S^k_\mu \phi(k)| < \infty \tag{4}$$

for each $k = 0,1,2,\ldots$ and $S_\mu = D^2 - \frac{4\mu^2 - 1}{4x^2}$.

Alternately, we could use the following seminorms which generate the same topology in $G^\mu_{a,b}$:

$$\rho^\mu_k(\phi) = \max_{0\leq r\leq k} \gamma^\mu_r(\phi). \tag{5}$$

$G^\mu_{a,b}$ is defined as the linear space of all complex valued smooth functions $\phi(x)$ on $a < x < \infty$ for which γ^μ_k is finite for every $k = 0,1,2,\ldots$. Each $\gamma^\mu_k(\phi)$ is a seminorm on $G^\mu_{a,b}$ and γ^μ_0 is a norm. Therefore, $\{\gamma^\mu_k\}^\infty_{k=0}$ is a countable multinorm on $G^\mu_{a,b}$. The topology over $G^\mu_{a,b}$ is generated by $\{\gamma^\mu_k\}$.

Lemma 1

$G^\mu_{a,b}$ is complete and hence a Fréchet space.

Proof

Let $\{\phi_\nu\}^\infty_{\nu=1}$ be a Cauchy sequence in $G^\mu_{a,b}$ and let Ω be an arbitrary compact subset of $I = \{x: a < x < \infty\}$. Also, let η be a fixed point in I, and let D^{-1} denote the integration operator

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$$D^{-1} = \int_{\eta}^{t} \ldots dx. \tag{6}$$

So that for any smooth function $\xi(t)$ on I

$$D^{-1}D\xi(t) = \xi(t) - \xi(\eta). \tag{7}$$

By a straight forward computation one gets

$$S_{\mu}\phi_{\nu}(t) = t^{-\mu-\frac{1}{2}}Dt^{2\mu+1}Dt^{-\mu-\frac{1}{2}}\phi_{\nu}(t). \tag{8}$$

Hence, in view of the seminorm γ_{1}^{k}, $S_{\mu}\phi_{\nu}(t)$ converges uniformly on every Ω as $\nu \to \infty$. Moreover, we have

$$t^{-2\mu-1}D^{-1}t^{\mu+\frac{1}{2}}S_{\mu}\phi_{\nu}(t) = D_{t}t^{-\mu-\frac{1}{2}}\phi_{\nu}(t) - (\eta/t)^{2\mu+1}D_{\eta}^{}\eta^{-\mu-\frac{1}{2}}\phi_{\nu}(\eta). \tag{9}$$

The left-hand side of (9) converges uniformly on every Ω as $\nu \to \infty$ since multiplication by a power of t or the application of D^{-1} preserves this property. Moreover, $\phi_{\nu}(t)$ converges in the same way, in view of the seminorm γ_{0}^{μ}. We now see from (9) that $D_{t}t^{-\mu-\frac{1}{2}}\phi_{\nu}(t)$ also converges uniformly on every Ω, which implies in turn that $D_{t}\phi_{\nu}(t)$ does the same. Next, by virtue of the application of the operator S_{μ}, it follows that $D^{2}\phi_{\nu}(t)$ also converges uniformly on every Ω.

We repeat this argument with ϕ_{ν} replaced by $S_{\mu}^{k}\phi_{\nu}$ and $S_{\mu}\phi_{\nu}$ replaced by $S_{\mu}^{k+1}\phi_{\nu}$. This shows that for every non-negative integer k, $D^{k}\phi_{\nu}(t)$ converges uniformly on every Ω. Consequently, there exists a smooth function $\phi(t)$ on I such that for each k and t,

$$D^{k}\phi_{\nu}(t) \to D^{k}\phi(t) \text{ as } \nu \to \infty.$$

Next, the assumption that $\{\phi_{\nu}\}$ is a Cauchy sequence in $G_{a,b}^{\mu}$ can be restated as follows. For each non-negative integer k and any given $\varepsilon > 0$, there exists a real N_k, such that for every ν, $n > N_k$, we have $\gamma_{k}^{\mu}(\phi_{\nu} - \phi_{n}) < \varepsilon$. Therefore, when $n \to \infty$, we obtain

$$\gamma_{k}^{\mu}(\phi_{\nu} - \phi) \le \varepsilon \tag{10}$$

for all $\nu > N_k$. In other words $\gamma_{k}^{\mu}(\phi_{\nu} - \phi) \to 0$ as $\nu \to \infty$ for each $k = 0,1,2,\ldots$.

Finally, there exists a constant C_k not depending on ν such that $\gamma_{k}^{\mu}(\phi_{\nu}) < C_k$. Therefore, by (9) we have

$$\gamma_{k}^{\mu}(\phi) \le \gamma_{k}^{\mu}(\phi_{\nu}) + \gamma_{k}^{\mu}(\phi - \phi_{\nu}) \le C_k + \varepsilon. \tag{11}$$

Thus ϕ is in $G_{a,b}^{\mu}$ and is the limit in $G_{a,b}^{\mu}$ of $\{\phi_{\nu}\}$. This completes the proof.

$(G_{a,b}^{\mu})'$ is the dual of $G_{a,b}^{\mu}$ and by Theorem 1.8-3 of Zemanian (1968) it is also complete. The members of $(G_{a,b}^{\mu})'$ are generalized functions.

For any fixed μ, G_{a}^{μ} is defined to be the strict inductive limit (Friedman, 1963) of $G_{a,b}^{\mu}$ where b traverses a monotonically increasing sequence of positive numbers and tends to infinity. Thus, $\phi \in G_{a}^{\mu}$ if and only if $\phi \in G_{a,b}^{\mu}$ for some b. A sequence $\{\phi_{\nu}\}$ converges to ϕ in G_{a}^{μ} if and only if ϕ and all ϕ_{ν} are in $G_{a,b}^{\mu}$ for some fixed b, and $\{\phi_{\nu}\} \to \phi$ in $G_{a,b}^{\mu}$. G_{a}^{μ} is sequentially complete.

We now list some properties of $G_{a,b}^{\mu}$ and $(G_{a,b}^{\mu})'$.

1. If $c < d$, then $G_{a,c}^{\mu} \subset G_{a,d}^{\mu}$, and the topology of $G_{a,c}^{\mu}$ is identical to the topology induced on it by $G_{a,d}^{\mu}$.

2. For each $f \in (G_{a,b}^{\mu})'$, there exists a non-negative integer r and a positive constant C such that

$$|\langle f,\phi\rangle| \le C \max_{0\le k\le r} \gamma_{k}^{\mu}(\phi) \tag{12}$$

for every $\phi \in G_{a,b}^{\mu}$. Here C and r depend on f but not on ϕ.

3. The differential operator S_{μ} is a continuous linear mapping of $G_{a,b}^{\mu}$ into $G_{a,b}^{\mu}$, as is implied by the equation

$$\gamma_{k}^{\mu}(S_{\mu}\phi) = \gamma_{k+1}^{\mu}(\phi), \ \phi \in G_{a,b}^{\mu}.$$

Thus the adjoint S_{μ}' of S_{μ} is a generalized differential operator on $(G_{a,b}^{\mu})'$ into $(G_{a,b}^{\mu})'$ defined by

$$\langle S_{\mu}'f,\phi\rangle := \langle f,S_{\mu}\phi\rangle. \tag{13}$$

Since $\underline{S}_{\mu} = S_{\mu}$, we also have that $\underline{S}_{\mu}' = S_{\mu}'$. Because of the symmetry of S_{μ}, we shall henceforth denote the generalized differential operator S_{μ}' by S_{μ}, thereby dropping the prime. The symbol S_{μ} denotes either a conventional or a generalized differential operator depending on the ways in which it is used. If it acts on a testing function in $G_{a,b}^{\mu}$, it is a conventional operator; and if it acts on a generalized function in $(G_{a,b}^{\mu})'$, it is a generalized operator.

Let a be any real number, and let f(t) be a locally integrable function on $a < t < \infty$ such that f(t) is absolutely integrable on $a < t < \infty$. Then, f(t) generates a regular member of $(G_{a,b}^{\mu})'$ which we shall denote by f, through the definition

$$\langle f,\phi\rangle := \int_{a}^{\infty} f(t)\phi(t)dt, \ \phi \in G_{a,b}^{\mu}. \tag{14}$$

That f is truly a member of $(G_{a,b}^{\mu})'$ can be seen from the inequality

$$|\langle f,\phi\rangle| \le \gamma_{0}^{\mu}(\phi) \int_{a}^{\infty} |f(t)|dt < \infty, \ \phi \in G_{a,b}^{\mu}. \tag{15}$$

For $a > 0$, $G_{a,b}^{\mu}$ can be identified with a subspace of $(G_{a,b}^{\mu})'$. For, if $\psi(t)$ and $\theta(t)$ are both in $G_{a,b}^{\mu}$ and differ somewhere, we can choose a non-negative testing function $\phi(t) \in D(I)$ whose support is contained in an interval on which $\psi - \theta$ is never equal to zero. In this case, $\langle\psi,\phi\rangle \ne \langle\theta,\phi\rangle$ where $\phi(t) \ne 0$. This verifies that ψ and θ, taken as members of $(G_{a,b}^{\mu})'$ must differ.

Thus, for $a > 0$, $G_{a,b}^{\mu}$ can be identified with a subspace of $(G_{a,b}^{\mu})'$ and we write

$$G_{a,b}^{\mu} \subset (G_{a,b}^{\mu})'.$$

THE TESTING FUNCTION SPACES $H_{a,b}$ AND H_a

Let y and w be real variables and let $\eta = y+iw$ where $b > a > 0$; Φ is a member of $H_{a,b}$ if and only if $\eta^{-\frac{1}{2}}\Phi(\eta)$ is an even analytic function of η and

$$\alpha_k(\Phi) := \alpha_{a,b,k}(\Phi)$$

$$:= \sup_{\eta} |e^{-(b-a)|w|}\eta^{2k+\frac{1}{2}}\Phi(\eta)| < \infty. \qquad (16)$$

The topology of $H_{a,b}$ is the one generated by using the seminorms $\alpha_{a,b,k}$, $k = 0,1,2,\ldots$. An equivalent topology is generated by the following set of norms:

$$\beta_{a,b,k}(\Phi) = \max_{0\le r\le k} \alpha_{a,b,r}(\Phi),\ k=0,1,2,\ldots . \qquad (17)$$

Thus, $H_{a,b}$ is a locally convex Hausdorff topological vector space that satisfies the first axiom of countability (Friedman, 1963). It is easily shown that $\beta_{a,b,p}$ and $\beta_{a,b,q}$ are in concordance for every choice of p and q (Zemanian, 1966). Hence, $H_{a,b}$ is a countable normed space (Friedman, 1963). Clearly, $H_{a,b}$ is sequentially complete. Its dual is denoted by $H'_{a,b}$, which is also complete by Theorem 1.8-3 of Zemanian (1966).

Lemma 2

If $b < c$, then $H_{a,b} \subset H_{a,c}$, and the topology generated on $H_{a,b}$ by $H_{a,c}$ is identical to the topology of $H_{a,b}$.

Proof

Let Φ belong to $H_{a,b}$. By the Phragmen - Lindelöf theorem (Titchmarsh, 1939), we have for $w \ge 0$

$$|e^{i(c-a)\eta}\eta^{2k+\frac{1}{2}}\Phi(\eta)| = e^{-(c-a)w}|\eta^{2k+\frac{1}{2}}\Phi(\eta)|$$

$$\le \sup_{y} |y^{2k+1/2}\Phi(y)|,$$

and for $w \le 0$,

$$|e^{i(c-a)\eta}\eta^{2k+\frac{1}{2}}\Phi(\eta)| = e^{(c-a)w}|\eta^{2k+\frac{1}{2}}\Phi(\eta)|$$

$$\le \sup_{y} |y^{2k+\frac{1}{2}}\Phi(y)|.$$

Thus,

$$\alpha_{a,c,k}(\Phi) = \sup_{\eta} |e^{-(c-a)|w|}\eta^{2k+1}\Phi(\eta)|$$

$$\le \sup_{y} |y^{2k+1/2}\Phi(y)|$$

$$\le \alpha_{a,b,k}.$$

Conversely,

$$\alpha_{a,b,k}(\Phi) \le \sup_{\eta} |e^{-(a-b)|w|}\eta^{2k+\frac{1}{2}}\Phi(\eta)|$$

$$\le \sup_{\eta} |e^{-(a-c)|w|}\eta^{2k+\frac{1}{2}}\Phi(\eta)|$$

$$= \alpha_{a,c,k}(\Phi).$$

Therefore,

$$\alpha_{a,b,k}(\Phi) = \alpha_{a,c,k}(\Phi).$$

Lemma 3

The operation $\Phi \to \eta^{2r}\Phi$, $r = 1,2,\ldots$, is an isomorphism from $H_{a,b}$ onto itself.

Proof

We have

$$\alpha_{a,b,k}(\eta^{2r}\Phi) = \alpha_{a,b,k+r}(\Phi).$$

Lemma 4

The operation $f \to \eta^{2r}f$, $r = 1,2,\ldots$, is an isomorphism from $H'_{a,b}$ onto itself.

Proof

The proof follows from Theorem 1.10-2 of Zemanian (1966).

Lemma 5

Let $Y(\eta)$ be an even entire function such that

$$|Y(\eta)| < Qe^{c|w|}(1 + |\eta|^{2m})$$

where Q and c are positive constants and m is a non-negative integer, then $\Phi \to Y\Phi$ is a continuous linear mapping of $H_{a,b}$ into $H_{a,b+c}$.

Proof

We have

$$\alpha_{a,b+c,k}(Y\Phi) \le \sup_{\eta}\left|\frac{e^{-c|w|}Y(\eta)}{1+|\eta|^{2m}}\right|$$

$$\times \sup_{\eta}\left|e^{-(b-a)|w|}(1+|\eta|^{2m})\eta^{2k+\frac{1}{2}}\Phi(\eta)\right|$$

$$\le Q[\alpha_{a,b,k}(\Phi) + \alpha_{a,b,k+m}(\Phi)].$$

H_a will denote the strict inductive limit (Friedman, 1963) of $H_{a,b}$ where b traverses a monotonically increasing sequence of positive numbers tending to infinity. The dual of H_a is denoted by H'_a.

Lemma 6

$F \to YF$ is a continuous linear mapping of H'_a into H'_a.

The proof follows by using Theorem 1.10-2 of Zemanian (1968).

Theorem 2

The Weber transform W_μ is a continuous linear mapping from $G^\mu_{a,b}$ into $H_{a,b}$ and the inverse Weber transform W_μ^{-1} is a continuous linear mapping from $H_{a,b}$ into $G^\mu_{a,b}$.

Proof

First, let us assume that $\phi \in G^\mu_{a,b}$ and $\mu \ge 0$. Then, $\phi(x) \in L_2(a,b)$ and we set

$$\Phi(\eta) = \int_a^b (x\eta)^{\frac{1}{2}}C_\mu(x\eta,a\eta)\phi(x)\,dx.$$

By Griffith's theorem, $\eta^{-\frac{1}{2}}\Phi(\eta)$ is an even and analytic function of η. Therefore

$$(-1)^m\eta^{2m}\Phi(\eta) = \int_a^b (-1)^m\eta^{2m}(x\eta)^{\frac{1}{2}}C_\mu(x\eta,a\eta)\phi(x)\,dx$$

$$= \int_a^b S_\mu^m[(x\eta)^{\frac{1}{2}}C_\mu(x\eta,a\eta)]\phi(x)\,dx$$

$$= \int_a^b S_\mu^m[\phi(x)](x\eta)^{\frac{1}{2}}C_\mu(x\eta,a\eta)\,dx$$

(by integration by parts).

Therefore

$$(-1)^m\eta^{2m+\frac{1}{2}}\Phi(\eta) = \int_a^b (x\eta)C_\mu(x\eta,a\eta)[S_\mu^m\phi(x)]x^{-\frac{1}{2}}\,dx.$$

For $a < x < b$, $(x\eta) \to 0 \Rightarrow \eta \to 0$. In this case

$$x\eta = O(a\eta)$$

and

$$|J_\mu(a\eta)J_{-\mu}(x\eta)| = O(1).$$

Also, as $x\eta \to 0$,

$$C_0(x\eta,a\eta) = O(\log \tfrac{x}{a}).$$

Moreover, as $(x\eta) \to \infty$,

$$J_\mu(a\eta)J_{-\mu}(x\eta) = O[(x\eta)^{-1}].$$

Thus, $(x\eta)C_\mu(x\eta,a\eta)$ is bounded by a constant M_μ for all $\eta \in \mathbb{C}$ and $a < x < b$, $a > 0$. Therefore

$$\alpha_{a,b,m}(\Phi) = \sup_\eta\left|\eta^{2m+\frac{1}{2}}\Phi(\eta)e^{-(b-a)|w|}\right|$$

$$\le M_\mu \sup_{a<x<b} |S_\mu^m\phi(x)| \int_a^b x^{-\frac{1}{2}}\,dx$$

$$\le M'_\mu\gamma_m^\mu(\phi).$$

Thus, the linear transformation W_μ is a continuous linear mapping of $G_{a,b}^\mu$ into $H_{a,b}$. The above result is also true for $\mu < 0$ because $C_\mu(x\eta,a\eta)$ is an even function of μ.

Conversely, let $\Phi(\eta)$ belong to $H_{a,b}$. Then $\Phi(\eta)$ satisfies conditions of Griffith's theroem and is the μth order Weber transform of $\phi(x) \in L_2(0,\infty)$ which is zero everywhere for $x > b$. Hence by the inversion formula (2), we have

$$\phi(x) = \int_0^\infty \Phi(y)(xy)^{\frac{1}{2}}\frac{C_\mu(xy,ay)}{[Q_\mu(ay)]^2}\,dy.$$

By differentiating under the integral sign, we get

$$(-1)^kS_\mu^k\phi(x) = \int_0^\infty y^{2k}\Phi(y)(xy)^{\frac{1}{2}}\frac{C_\mu(xy,ay)}{[Q_\mu(ay)]^2}\,dy.$$

For $a < x < b$, $x^{\frac{1}{2}}C_\mu(xy,ay)$ is bounded for all $y \in (0,\infty)$; and

$$[Q_\mu(y)]^2 = \begin{cases} O(y) & \text{as } y \to \infty \\ O(y^{2\mu}) & \text{as } y \to 0 \\ O[(\log y)^{-2}] & \text{as } y \to 0 \text{ for } \mu = 0. \end{cases}$$

Therefore

$$|S_\mu^k\phi(x)| \le M_\mu \sup_{0<y<\infty} |y^{2k+\frac{1}{2}}\Phi(y)(1+y^2)^2\int_0^\infty \frac{[Q_\mu(y)]^2}{(1+y^2)^2}\,dy|$$

$$\le M'_\mu \sup_{0<y<\infty} |y^{2k+\frac{1}{2}}\Phi(y)(1+y^2)^2|$$

$$\le M'_\mu \sup_{0<y<\infty} |y^{2k}(1+y^2)^2y^{\frac{1}{2}}\Phi(y)|$$

$$\le M'_\mu \sup_y |(1+y^2)^{k+2}y^{\frac{1}{2}}\Phi(y)|$$

$$= M'_\mu \sup_y \sum_{r=0}^{k+2}\binom{k+2}{r}|y^{2r+\frac{1}{2}}\Phi(y)|$$

$$\le M'_\mu \sum_{r=0}^{k+2}\binom{k+2}{r}\sup_\eta |e^{-(b-a)|w|}\eta^{2r+\frac{1}{2}}\Phi(\eta)|$$

$$\le M'_\mu \sum_{r=0}^{k+2}\binom{k+2}{r}\alpha_{a,b,r}(\Phi).$$

Therefore W_μ^{-1} is a continuous linear mapping from $H_{a,b}$ into $G_{a,b}^\mu$.

THE WEBER TRANSFORMATION OF $G_a'^\mu$

For $f \in G_a'^\mu$, $\phi \in G_a^\mu$ and $\Phi = W_\mu\phi \in H_a$ we define the generalized Weber transformation of f denoted by $F = W'_\mu f \in H'_a$ by

$$\langle F,\Phi\rangle = \langle f,\phi\rangle. \tag{18}$$

We can also define the inverse Weber transform $f = (W'_\mu)^{-1}F$, when $F \in H'_a$, by

$$\langle (W'_\mu)^{-1}F,(W_\mu^{-1})\Phi\rangle = \langle F,\Phi\rangle \tag{19}$$

for all $\Phi \in H_a$.

The following theorem is a consequence of Theorem 2 and Theorem 1.10-2 of Zemanian (1968).

Theorem 3

W'_μ is a continuous linear mapping from $G_a'^\mu$ into H'_a, and $(W'_\mu)^{-1}$ is a continuous linear mapping from H'_a into $G_a'^\mu$.

AN OPERATIONAL CALCULUS

The generalized Weber transform can be used in solving boundary value problems with distributional boundary conditions.

The following theorem is fundamental in this connection.

Theorem 4

If $f \in G_a'^\mu$ and $F = W'_\mu f \in H'_a$, then

$$W'_\mu(S^k f) = (-1)^k \eta^{2k} W'_\mu f, \; k = 0,1,2,\ldots \; . \quad (20)$$

Proof

From the fefinition of the operator S_μ and (18), we have

$$\begin{aligned} \langle W'_\mu(S^k f), \Phi\rangle &= \langle S^k_\mu f, \phi\rangle \\ &= \langle f, S^k_\mu \phi\rangle \\ &= \langle W'_\mu f, W_\mu(S^k_\mu \phi)\rangle \\ &= \langle W'_\mu f, (-1)^k \eta^{2k} W_\mu \phi\rangle \\ &= (-1)^k \eta^{2k} \langle W'_\mu f, \Phi\rangle . \end{aligned}$$

Hence the proof.

AN APPLICATION

We seek the solution $u \in G'^{\mu}_a$ of the differential equation

$$S^k_\mu u = g, \quad (21)$$

where $g \in G'^{\mu}_a$ such that $W'_\mu g \in H'_a$.

Applying Theorem 4, we have

$$(-\eta^2)^k U(\eta) = G(\eta), \quad (22)$$

where $U(\eta) = W_\mu u$ and $G(\eta) = W_\mu g$ are elements of H'_a. Also, we may add to (22) the general solution $H(\eta)$ of the homogeneous equation.

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Department of Mathematics,
Banaras Hindu University,
Varanasi 221005, India.

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The Electromagnetic Pinch: From Pollock to the Joint European Torus*

R. S. PEASE

INTRODUCTION

This review of the electromagnetic pinch starts with an exhibit taken from Pollock's work, carefully preserved and drawn to attention of modern research by Professor C. Watson-Munro. It is a compressed and distorted length of copper tube originally part of the lightning conductor on the Hartley Vale kerosene refinery in New South Wales (Figure 1). It was known to have been struck by lightning. Pollock and Barraclough (1905) from the Department of Mechanical Engineering at Sydney University carried out an analysis to see whether or not the compression could have arisen from the flow of electric current. They concluded that the compressive forces, due to the interaction of the large current flow with its own magnetic field could have been responsible for the compression and distortion. As far as I know, this is the first identified piece of observational data on the electromagnetic pinch; and the first theoretical discussion of the effect.

Pollock and Barraclough did not, perhaps, have accurate data on the currents and duration of lightning strokes, and therefore believed that the compression of the tube was aided by a softening of the copper because its temperature was raised. A repetition of their analysis with modern data (Fink and Carroll, 1968) suggests that if the duration of the stroke was its normal length - not much more than 10^{-4}s - then the heating effect would have been small. The required compressive effect in unsoftened copper of the strength they quote would have been provided by currents exceeding about 150kA. About 1% of all lightning strokes have peak currents greater than about 150kA, so that although the stroke is unusual, the basic conclusion drawn by Pollock would seem to be correct.

Because the electric nature of lightning and lightning conductors were well known by 1905, it is perhaps surprising that the effect had not been observed earlier. However, the use of copper tube upon which the observation depended may have depended on the understanding of the skin effect, which was developed only in the later part of the nineteenth century. The actual arrangement sketched in Figure 1 shows another interesting feature. There was a keen dispute as to whether or not the tops of lightning conductors should be made sharp or blunt (Seeger, 1973). It would appear that the top here is basically spherical but with some sharp points (some of which were dislodged by the lightning flash) added by way of compromise.

* Pollock Memorial Lecture for 1984 delivered at the University of Sydney on 28th November, 1984.

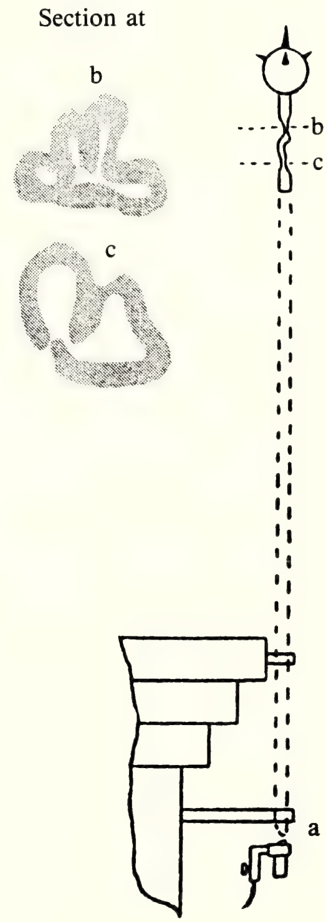

Fig. 1. Lightning Conductor at the Hartley Vale Kerosene Plant, N.S.W., examined by Pollock and Barraclough, 1905.

Finally, it should be remarked that only part of the copper tube collapsed, most of its length was unaffected, so that the effect must have been rather marginal in this case.

DEFINITIONS

The electromagnetic pinch is the constriction of a compressible conducting column when a large current is passed along it. The current generates a magnetic field $\underline{B}$ which must penetrate to the

region where there is a current density $\underline{j}$. The force per unit volume there is $\underline{j} \times \underline{B}$, and in general gives rise to a gradient of pressure p and an acceleration of the material of mass density ρ and velocity v, which can be described by the simple magnetohydrodynamic fluid equation:

$$\underline{j} \times \underline{B} = \text{grad } p + \frac{D}{Dt}(\rho v) \qquad \ldots\ldots\ldots\ldots(1)$$

In the simplest case of a straight cylindrical conductor of radius a with the acceleration term neglected, the pressure given by the effect can be calculated simply, provided the distribution of current as a function of radius r is known. For example, the pressure on axis p(o) is given in the simple case of a uniform current distribution by

$$p(o) = I^2/\pi a^2 \qquad \ldots\ldots\ldots\ldots(2a),$$

and for case of strong skin current

$$p(o) = I^2/2\pi a^2 \qquad \ldots\ldots\ldots\ldots(2b).$$

Here I is the total current in e.m.u. The difference between (2a) and (2b), even though both cases have the same magnetic field on the surface of the conductor, reminds us that the concept of magnetic pressure characterized by the quantity $H^2/8\pi$, which is a very useful concept, does not always yield an exact pressure in the presence of curved lines of force.

A more general form of the pressure balance, which is independent of the current distribution, was first given by Bennet in 1934, for the case of a gaseous medium characterized by a gas pressure p

$$p = nkT$$

where n is number density and k Boltzmann constant and T the temperature. By integrating the expression over the radius and assuming negligible pressure at the outside one can obtain the general expression

$$\beta_\theta I^2 = 2\,k\,(N_e\overline{T}_e + N_i\overline{T}_i) \qquad \ldots\ldots\ldots\ldots(3).$$

where N_e and N_i are the number of electrons and ions respectively per unit length of the discharge; T_e and T_i are their mean temperatures; and β_θ is a coefficient which we shall need later, and which is unity in the simple pinch.

The inertial term in Equation (1) is important in rapidly rising current discharges through low pressure gases, where both the mass density and velocity of the ionized gas change with time as the discharge collapses. The inertial term is likely to have played a significant part in the collapse of Professor Pollock's lightning conductor.

CONFIRMATORY WORK IN LIQUID METALS

Shortly after Pollock's discovery, Northrupp (1907) working in America, published a paper "Some months ago my friend Carl Hering described to me a surprising and apparently new phenomenon which he had observed, namely in passing a large alternating current through a liquid conductor contained in a trough the liquid contracted in cross sections and flowed uphill, length wise of the trough, climbing up upon the electrodes." Northrupp identified the effect as due to the self-magnetic-pinch effect, and he ascribes the use of the word "pinch" to Hering who he said "jocosely called it the pinch phenomenon", a name which has stuck ever since.

Northrupp himself extensively studied the way the forces acted on this pinch, and showed by experiments with liquid potassium in kerosene that indeed this effect happened. There is nothing mysterious about it, but it was at that time a new manifestation of the electromagnetic forces between currents first analysed by Ampére. One interesting example of Northrupp's work showed that the pinch effect could be used to produce continuous motion of mercury (Figure 2). The current flows in and out of the double-walled vessel containing mercury as shown. There are insulating walls, which constrict the current channel in the centre, so that it produces a local high-pressure pinch effect. The mercury therefore is squeezed out of this pinch, axially upwards (with a manometer on the top, he could measure the mercury rising in the manometer when the currents pass). In this particular experiment he connected the mercury round through a separate channel, by which it could get back into the pinch region. This is an interesting example of the continuous motion of fluid produced just by a current flowing through it, without any use of external magnetic field (as in a conventional electromagnetic pump). The effect might seem to be the inverse of the self-excited dynamo; perhaps if one took some mercury and circulated it vigorously in the apparatus such as in Figure 2, a current could be generated. However, the analogy with geophysical processes is perhaps not sound, because of the presence of insulators.

The subject of the pinch, generally speaking, did not attract much attention in the years following these reports of this effect in solids and liquids. More recently, experiments have been conducted in falling columns of sodium and of mercury, mostly to show that the pinch equilibrium is unstable, and that the magnetic forces, although they are there, very quickly can get out of equilibrium (Dattner, Lehnert and Lindquist, 1958; Bickerton and Spalding, 1962). Finally, there are now industrial applications of magnetic pressure, in which metals are formed by means of pulsed electromagnetic fields. For example, at the exhibition accompanying the Fourth U.N. Conference on the Peaceful Uses of Atomic Energy (in 1971), the Soviet Union showed magnetic crimping of aluminium cans on to uranium fuel elements.

RADIATIVE COLLAPSE OF DEGENERATE PINCHES

Since the magnetic forces have no mechanical upper limit, it is interesting to speculate on how large a pressure might be generated. Northrupp himself was clearly very interested in how far you might be able to carry this phenomenon. For instance, consider a one-micron-radius copper wire; such a conductor can be said to have a current-carrying capacity of about 350 megamps; that is, if all the electrons in the copper wire move with the velocity of light, then the current would be about 350 MA. Of course, it is perhaps a challenge for the engineers to get 350 MA through a one-micron radius piece of copper. But if one did there would be a very substantial pressure generated, and the question is: what would happen?

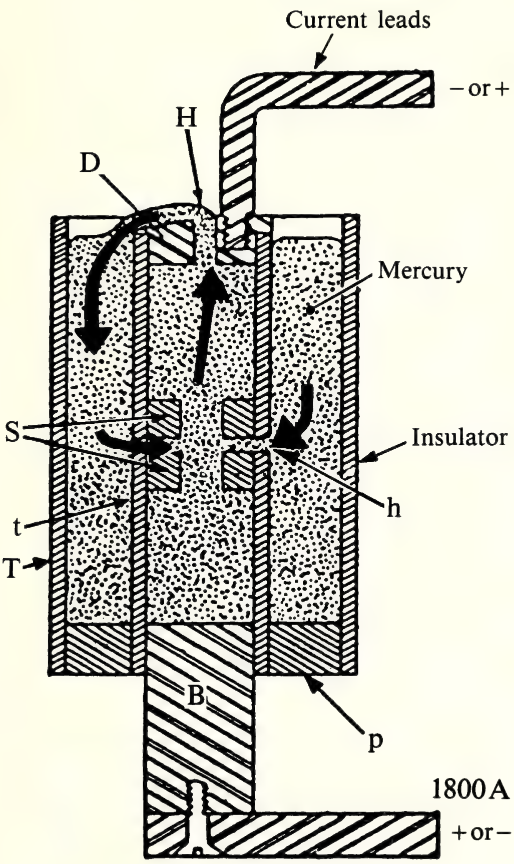

Figure 2. Apparatus for pinch effect to produce continuous circulation of conducting fluid (Northrupp, 1907). B, D, - metal electrodes; p, - insulating plug; T, τ - outer and inner insulating tubes; S, - insulating constriction; h, G, - flow channels for mercury. Arrows show direction of flow (independent of polarity).

Now the copper would certainly vaporize and form a hot ionized gas or plasma essentially fully ionized. Classically the way the pressure increases as the radius decreases would mean that, provided the gas is conserved and is adiabatic, then the gas pressure rises more rapidly than the magnetic pressure, so that the compressing force of the current is resisted at some radius, although perhaps quite a small one.

If there is some radiative cooling, as is believed to be responsible for the collapse of stars, then the gas would cool, and it would be unable to resist the magnetic forces. The radiated power can be calculated with a fairly straightforward formula, at least in the non-relativistic limit, and compared with the ohmic heating, from the current flow. Because the resistivity of an ionized gas decreases as the temperature is raised, then it can be shown that, if the current is large enough, the radiative cooling will exceed the resistive heating, the gas will cool itself, and the pinch radii will collapse. If the current is fairly small, then the ohmic heating in the gas will overcome radiative loss, and the pressure prevents the collapse of the conductor. For the simplest case of radiative cooling by Bremsstrahlung, the critical current separating the two regimes is given (Pease, 1957) by

$$I_{crit} = \left(\frac{mc^2}{e}\right) \left(\frac{hc}{e^2}\right)^{\frac{1}{2}} \left(\frac{Z+1}{2z}\right) \left(\frac{1}{\beta_\theta}\right) .2 [3^{\frac{1}{2}} \text{Log } \Lambda]^{\frac{1}{2}} \text{ e.m.u.} \qquad \ldots\ldots\ldots\ldots (4)$$

Here the electron charge e is in e.s.u., the ionic charge, supposedly fully stripped is Z and Log Λ is the coulomb logarithm which appears in ion-electron collision terms. For the case of Z=1 (hydrogen), $\beta_\theta = 1$ (no trapped magnetic field) I_{crit} is about 1.7 MA.

Numerical factors apart, this is the current carried by a conductor containing one electron per classical radius of electron moving at the speed of light, multiplied by the square root of the fine structure constant. It comes out at roughly this quantity irrespective of the number of particles per unit length, or of the radius of the discharge; and is relatively insensitive to the nuclear charge Z. So that this collapse is current dependent, and not geometry dependent. Of course, if the radiation loss is enhanced by (for example) line radiation, then the critical current will be substantially less (Ashby and Hughes, 1981). This calculation is non-relativistic. Nevertheless, at currents greatly below 1 megamp one would expect the wire to explode; whereas with much larger currents we might expect it to collapse. Some recent work is being carried out to see if a radiative collapse can be detected (Hammel, Scudder and Schlachter, 1983). Of course, as the pinch collapses the inductance of the circuit increases and so to keep the current constant additional energy has to be fed in; otherwise the current will fall off. In any case it is likely (as mentioned above) to be unstable.

If, nonetheless, the filament collapses under the self-magnetic pressure, then presumably the Fermi pressure of a degenerate state would determine the equilibrium radius (Fowler, 1955). If there are N electrons per unit length, moving at some fraction η of the velocity of light, then in the non-relativistic case the radius a_{nR} of the pinch is given by

$$a_{nR} \simeq \frac{1}{\pi^{\frac{1}{2}}} \left(\frac{h^2}{me^2}\right)^{\frac{3}{4}} N_e^{-\frac{1}{4}} \cdot \eta^{-\frac{3}{2}} \qquad \ldots\ldots\ldots\ldots (5)$$

Here h is Planck's constant, m and e the mass and charge of the electrons; the pressure of the ions is neglected.

Consequently, when $N_e > 10^{10}$ per m length, as is invariably the case in gas discharges, the radius of the pinch could be somewhat less than the Bohr radius depending on the current as represented by η. The radius given in equation (5) is the lower limit given by complete degeneracy. Some non-degeneracy must be allowed in order that the discharge can radiate, which would expand the radius. Taking the numerical example of the 1 micron radius copper wire when $N_e = 7 \times 10^{18} m^{-1}$, if $\eta = 1/100$, then I = 3.5 Mamps, and a $\simeq 10^{-9}$ m. Equation (3) still holds, but the electron

temperature is replaced by the Fermi energy.

If larger currents are carried then the Fermi energy becomes relativistic. As is well known from the study of gravitational collapse, the Fermi pressure then varies more slowly with density, (Fowler, 1955), namely:

$$p = (hc/8).\ n^{\frac{4}{3}} \qquad \text{...........(6a)}$$

and the pressure balance would thus require a still smaller radius

$$a_R \simeq \frac{\pi}{8} N_e^{-1} \left(\frac{hc}{e^2}\right)^{\frac{3}{2}} \eta^{-3} \qquad \text{...........(6b)}$$

If we take the same copper wire, with $\eta = 1/10$ (a current of 35 MA) equation (6b) leads to a radius of ~ 10^{-13}m. Such speculative radius is unlikely to be achieved because of instabilities, but this particular case has not, to my knowledge, been analysed for stability.

GASEOUS PINCH DISCHARGES

In the 1940s, the possibility of using the pinch to provide the temperatures and high pressures needed to produce thermonuclear reactions was realized by a number of workers, including P.C. Thonemann from Australia (1958), and G.P. Thompson (1946) in England.

In the Soviet Union and in America a considerable number of experiments were carried out in straight gaseous discharge tubes, generally made of quartz or other high quality ceramic, currents of up to about 2 MA were passed, and these produced temperatures of about 2 million degrees at relatively modest densities, about 10^{17}cm^{-3}. These discharges were energized by powerful banks of capacitors and were of short duration, i.e. 1 - 10 microseconds (Figure 3). The chief results of this work, which is extensively reviewed (Artsimovich, 1964), are that the primary phenomenon observed were dynamic in nature, and rather far removed from the steady-state calculations dealt with above. The collapse phase of the pinch during the rising current is well-described by the inertial terms of equation (1) developed into the so-called snow plough model, independently in the U.S. and USSR (Rosenbluth and Garwin, 1954; Leontovitch and Osovets, 1956).

Thereafter, the pinch column tends to break up due to the onset of instabilities predicted by Kruskal and Schwarzchild (1954). The rapid increases of the circuit inductance produced strong electric fields which, aided by the ionization produced by the radiation from the compressed column, induced secondary breakdown at the walls of the tube; it is these secondary breakdowns which effectively prevented the achievement of higher pinch currents.

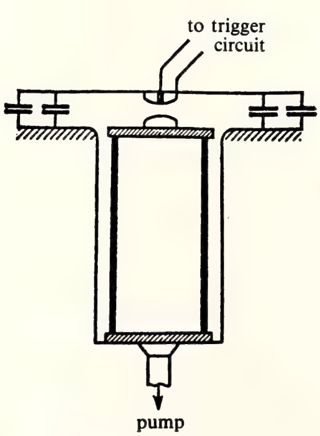

Figure 3. Diagram of capacitor - driven electric discharge apparatus, used for studies in U.S.A.and U.S.S.R. of the pinch effect in low pressure gases.

This simple type of linear geometry pinch discharge is still studied, but mostly in a form known as the "plasma focus" which uses a form chosen to maximize the compression and to minimize the influence of the walls and electrodes (Filippov, Filippova and Vinogradow, 1962). A fine current carrying filament is formed, with currents in the range 1-2 MA, lasting for about 100 nanoseconds. The duration of this stage is again apparently limited by instabilities; an example of these is shown in an optical interferogram at Figure 4. In the later stages of the discharges strong beams of quite high energy ions and electrons (1 MeV - 10 MeV) are formed, a remarkable result in view of the fact that the potential applied across the electrodes does not exceed 100 kV. The detailed physics, including the question of pressure balance, remains to be resolved. In the early stages, with a filamentary discharge of 2 or 3 mm diameter, electron densities reach 10^{19}cm^{-3} with temperatures up to about 10 M^{o}K (3rd International Workshop on Plasma Focus Research, Stuttgart, 1984). When operated in deuterium or in deuterium-tritium, these discharges are a strong source of nuclear reactions - up to about 10^{12} reactions in D-T have been observed, and the yield increases rapidly with current. However, most of the reactions are thought to result from interaction of the accelerated particles, and not from thermonuclear reactions.

The relation of the particle motions in the plasmas to the macroscopic quantities such as current density and pressure has been elucidated by Haines (1978).

The highest compressions observed occur in simple vacuum sparks, where the current carrying material is provided by the metallic electrodes. In these spark discharges, small hot spots a few microns in diameter, are formed with densities of about 10^{22}cm^{-3} and temperatures of 10 - 20 M^{o}K (Vikrev et al, 1981; Negus and Peacock, 1979). These produce confinement of the particles comparable to that needed in fusion reactors, but it is probable that the power required to maintain them is relatively very large, so that the energy confinement time - which is the crucial factor - is much less than the particle confinement time. It cannot be excluded that these hot spots are formed from metallic particles released from the electrode, rather than from an electromagnetic compression. The phenomenon in this respect is perhaps more like that encountered when large currents are passed through thin wires.

Figure 4. Interferogram of a pinched discharge in a plasma focus geometry. The length of the discharge shown is 3 cm. The discharge current is 0.85 MA, $n_e \sim 5 \times 10^{19} cm^{-3}$, $T_e \sim 2keV$, exposure time $1.2 \times 10^{-9}s$.

The interferometer is set up to produce a series of horizontal fringes; on introducing a plasma these fringes are displaced, producing (approximately) contours of plasma density. The implosion of a current-carrying skin produces a conical plasma at one end. A-A is a region of low compression, B-B of high compression. A series of instabilities can be seen breaking out from the main core (Peacock, Hobby and Morgan, 1971).

Taken together, there is still much to be understood in these simple types of pinches. But for thermonuclear research the emphasis has for a long time now been on the quasi-steady pinch stablized by longitudinal magnetic fields. Moreover, to avoid thermal conduction losses to and contamination from electrodes, these discharges are induced in toroidal chambers by pulsed transformer action, as pioneered by Cousins and Ware (1951).

TOROIDAL STABILIZED DISCHARGES

The instability of a simple pinch discharge, as illustrated in Figure 4, is a primary obstacle to obtaining in a pinch the high temperatures (about 100 million degrees) and good thermal insulation required to achieve a net output of energy from thermonuclear fusion reactions of hydrogen isotopes. In the 1950s, experiments on stabilizing the discharges by adding externally generated longitudinal fields and by using highly conducting metal-walled toroidal chambers were undertaken independently in several laboratories (Levine, 1955; Beztachenko et al, 1956; and Bickerton, 1958). Figure 5 illustrates the generic arrangement. The early experiments showed considerable improvements in the stability of quasi-steady state discharges. An example is the large Zeta machine built by Thonemann and Carruthers at Harwell (Butt et al, 1958), initially designed for 100kA currents, when temperatures in the range 1-2 $M^{o}K$ were established. However, detailed observation showed that in the initial experiments, not all the fluid instabilities were suppressed, that the thermal insulation of the gas by the magnetic fields was disappointingly low, and that the fusion reactions observed were primarily due to a distortion of the Maxwellian distribution of the ion velocities, rather than to the high temperatures (Burton et al, 1962; Afrosimov et al, 1960). Later developments on Zeta, using currents of up to 1 MA, showed that the origin of the main fluid instabilities was due to shortcomings in the detailed distribution of magnetic field components (Robinson and King, 1969). The critical observations showed moreover that the required configuration could, at sufficiently high conductivity of the gas, be generated by the flow of current itself in the plasma. The fluctuation of the current shown in Figure 6 indicates the presence of instabilities; but they cease for a period of about 3 milliseconds during the discharge when the configuration relaxes to a configuration calculated to be stable on the basis of the ideal fluid equation (1).

The essential requirement in this case is that the strength of the stabilizing field as a function of minor radius should fall to zero and change sign in the outer region of the discharge. In addition, there has to be no turning point in the magnitude of the stabilizing field in the outer regions; this last corresponds to the more general requirement that the pitch length (q) of the lines of force should always be changing as a function of minor radius r, i.e. $dq/dr \neq 0$ except at $r = 0$. The experimental results were in accord with this theory (Rosenbluth, 1958). A more complete theory given by Taylor (1974), predicts a current density distribution determined by

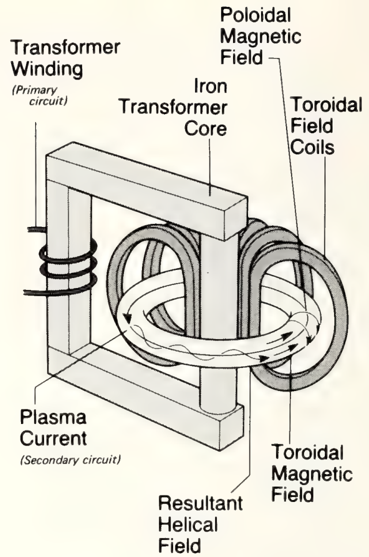

Figure 5. The toroidal pinch, stabilized with an externally generated toroidal field. The pitch of the helical lines is short compared to the major circumference in the reverse field pinch case; and is long in the Tokamak case.

$$\underline{j} = \mu \underline{B} \qquad \text{..........(7)}$$

where μ is a constant in space. It accounts for the self-stabilization phenomenon in the following qualitative sense. When currents flow through a flexible compressible conductor, the forces exerted on the conductor will always tend to minimise the total associated magnetic energy $\int H^2/8\pi$ subject to the conservation of flux; in a normal wire, the resulting motion tends to increase the circuit inductance. If resistivity is small but finite, the configuration must change to one that minimizes the magnetic energy integral. The configuration found in Zeta nearly satisfies that requirement. A more modern experimental result (Ohkawa and Kerst, 1961; Bodin and Keen, 1977) shows j/B plotted against radii in a pinch experiment. In particular, theory accounts for the formation of the reverse field in the outer region. It does not explain the detailed mechanism by which the necessary current flows are driven: these are still unknown.

The configuration found to arise is close to that originally calculated by Rosenbluth (1958) and which was also experimented on by trying to set up the configuration by deliberately programmed fields by a number of groups (Ohkawa and Kerst, 1961; Bodin and Keen, 1977). Both this reversed field pinch configuration and the spontaneous relaxation phenomenon are now the subject of considerable research (see references in Pease, 1985).

However, from the point of view of thermonuclear applications, it is essential that the

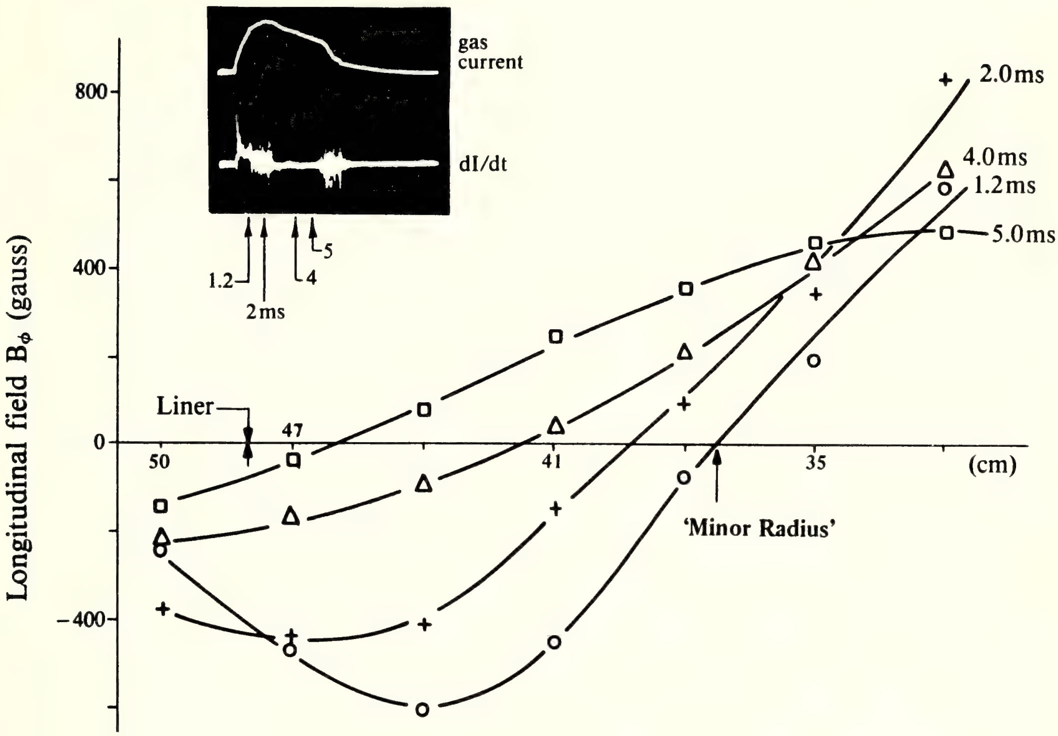

Figure 6. Self-stabilization and field configuration in the outer region of the discharge in ZETA (Robinson and King, 1969). The inset oscillogram shows the current I rising to 420kA peak; the fluctuation in the dI/dt trace drops to a low value for about 3ms, indicating a stable period. During stability the sign of the stabilizing field is reversed in the outer regions, and there are no turning points.

configuration actually departs from the prescription given by Taylor (equation (7)) in two respects: First that there must be some current flow $j_\perp$ perpendicular to the field - otherwise no pressure gradient can be sustained; and secondly that in the outer regions, where the gas is cold and/or the walls of the torus intrude, the current density is liable to fall to zero even though the magnetic field strength stays finite, i.e. μ cannot be constant everywhere as asked for by relaxation theory. A key question of modern thermonuclear research on pinches is whether or not sufficient departures can be obtained to obtain high temperature plasma confinement without encountering instabilities which reduce the thermal insulation, and allow the discharge to relax back towards the force-free Taylor state.

THE TOKAMAK CONFIGURATION

The Tokamak configuration differs from that investigated in Zeta by having a very strong stabilizing field; it was developed primarily in the Soviet Union (Artsimovich, 1972), although there was some early work at ANU, Canberra (Liley, 1968). The field has to be sufficiently strong for the pitch length of the lines of force to exceed the major circumferences $2\pi R$ of the torus; this is represented by a safety factor

$$q(r) \equiv \frac{rB_\phi}{RB_\theta} \gtrsim 1 \quad \text{for stability.}$$

Here B_ϕ is the stabilizing field, B_θ the field due to the pinch current. All the above considerations of self-stabilization apply, but the configuration apparently allows greater departures from the Taylor prescription (equation (7)), including especially the outer regions where the connection to the zero current region, necessarily present close to the chamber walls, can be sustained more easily than in the reverse field pinch. As a result, longer duration discharges are obtained (although the initial phases of the discharge normally show fluctuations indicative of instabilities). An important turning-point observation made on the Russian Tokamak T-3 installation, using the then novel technique of Thomson scattering of laser light to measure the quantity nkT_e as a function of position, enabled the first demonstration of equation (3) in quasi steady state to be achieved. In this case, the stabilizing field itself takes up some of the pinch pressure; and the coefficient β_θ has the theoretical value of about ½, close to that observed (Liley et al, 1968). Furthermore, the temperatures

obtained exceeded 10 MK at currents of up to about 200 kA.

JOINT EUROPEAN TORUS

The results obtained on these toroidal discharges, and particularly on the Tokamak, restored confidence that stabilized toroidal discharges could lead to a confined thermonuclear plasma. It appeared that the stability and the high temperatures resulted from rather general principles, and therefore could be extrapolated to apparatus capable of producing conditions close to those needed in an energy-producing thermonuclear reactor. Consequently, in discussing these matters with our European colleagues in 1970 we suggested that a collaborative venture might result in a bigger advance than could be taken individually. A number of considerations were in mind at that time in discussing what eventually became the Joint European Torus (Willson, 1981). First, to create conditions in which heating of the gas by the thermonuclear reactions could take place, then it is essential that the 3.5 MeV alpha particles from D-T fusion reactions be trapped within the plasma by the magnetic fields.

It turns out that to confine charged particles in a torus, the primary consideration is that the Larmor radius in the poloidal field component B_θ should be less than the minor radius; whatever the strength of the stabilizing field B_ϕ, in the long mean-free-path conditions expected, the radial excursion of charged particles in a toroid is about $(r/R)^{\frac{1}{2}}$x Larmor radius in B_θ. And this requires, from straightforward Larmor radius consideration, that the current should be about 3MA. A second consideration is that to get into the Bremsstrahlung cooling dominated condition, currents of about $1.7/\beta_\theta$(MA) were required. Thirdly, if we calculated thermal conduction losses to the walls due to coulomb collisions between the particles alone, then currents exceeding about 1.5 MA were necessary to achieve the thermal insulation needed in a self-sustained thermonuclear reactor. This third consideration was the most uncertain; because it seemed, as we shall see is indeed true today, the thermal conduction losses are greater than expected from collisions alone; there must be other contributing mechanisms; and it was clear that a margin in hand is needed over the bare minimum.

An estimate was obtained by empirical extrapolation from smaller machines. From discussions throughout Europe, it was agreed to design for about 3MA, with an extension capability to about 5MA which gives a margin of about ten times. And finally, although at the beginning there was some discussion as to whether or not the torus should be a Tokamak or some other system, the Tokamak choice was a relatively easy decision(JET Design Team Rep. 1974,1975; Comm.of European Communities Rep., 1976)

The design of a toroidal discharge of up to 5MA with currents lasting for up to 20 seconds (to exceed substantially the calculated energy confinement time)was undertaken by a European team led by R.H. Rebut. The choice of the dimensions is a compromise between size on the one hand and field strength on the other, and was chosen to minimize the total cost. The pulse transformer has a capacity of 34 V-s (compared with Zeta's 7.4 V-s), and is obtained in part by allowing the iron ore to saturate. The minor cross-section of the torus is D-shaped, partly because this has theoretical advantages for the discharge, but also because the shear stresses in the coils providing the 35 kilogauss stabilizing field are minimized with this shape. In this respect, JET differs from its somewhat smaller American counterpart,TFTR, which came into operation at the end of 1982, and which has circular toroidal field coils. The apparatus and its assembly have been fully described in the JET Design Team Reports 1974 and 1975. Table 1 summarizes the main parameters of JET and of other large Tokamaks.

JET has operated now for rather over a year and the first results have been described (Rebut et al, 1985), and are encouraging. Figure 7 shows the main characteristics of a pulse in JET, with the current rising to about 3.6MA, flowing through ionized deuterium with a number density of about $10^{19}m^{-3}$. The e.m.f. required to start the current is about 25 volts/turn, and drops to about 1 volt in the sustained phase. The pinch current in this case fills a large part of the minor cross-section. The pinch pressure is mostly supported by a small compression of the 3.5T stabilizing magnetic field; the value of β_θ in this case rises only to about 0.15. This relatively low value of gas pressure shows qualitatively that the ohmic heating is being counteracted by heat losses greater than expected than by collisions alone. Nonetheless the central temperature (measured by infra-red emission) rises to 3.6 keV or about 40 million degrees.

The first and straightforward deduction from Figure 7 is that the main presumption upon which JET was built, namely that stable quasi-steady discharge would be obtained, has been fully vindicated. The level of magnetic field fluctuations observed is extremely low (about 10^{-4} of the mean pinch field) in good conditions (Rebut et al, 1985).

However, if the safety factor q is too low, or the discharge too elongated, and in particular if the density n is too high, then the discharge will indeed disrupt and the current falls to zero in a few tens of milliseconds. Such disruptions have been known since the earliest toroidal discharges. But at megampere currents the consequences can be more serious, and therefore there is some concern to avoid them. Disruptions and their relation to density and to radiation collapse were discussed by Ashby and Hughes, 1981, and by Rebut et al, 1985.

Calculations of how these 3MA discharges should behave have been made by Hughes and are shown in Figure 8. These computations use modern neo-classical theory of the conduction losses and of particle diffusion (Rosenbluth, Hazeldene and Hinton, 1972), but they exclude the effect of impurities. In addition, these calculations assume that fresh hydrogen gas is fed in from the walls of the discharge, as is done in practice; otherwise the gas density would fall to zero at the wall. The temperature and density predicted are quite close to those observed. The biggest discrepancy is between the observed and calculated energy confinement times. Table II shows a comparison between the observed and calculated values of the product $n_e\tau_e$ where n_e is the mean electron density,

TABLE I

Engineering Parameters of Large Tokamaks

	TFTR (U.S.)	JET (EUROPE)	T-15 (USSR)	JT-60 (JAPAN)
Major Radius (m)	2.65	2.96	2.43	3.0
Minor Radius (m)	1.04	(1.25[a]) (2.10)	0.7	0.95
Field Strength (T)	5.2	3.5*	3.5/5.0	4.5
Peak Current (MA)	2.5*	5*	1.4/2.0	2.7
Pulse Duration (s)	3.0	5-20	5	10
Heating Power (MW)	24	25	9	30
Date of first operation	1/83	6/83	1987	4/85

[a]Horizontal: *achieved figures.

Special features:

TFTR: can operate with D-T fuel; and in strongly ion heated energy-multiplying mode.

JET: can operate with D-T fuel for up to 10^4 pulses at full yield.

T-15 Hydrogen operation only. Main toroidal field coils are superconducting.

JT-60: Hydrogen operation only, normal conductors; has a magnetic divertor.

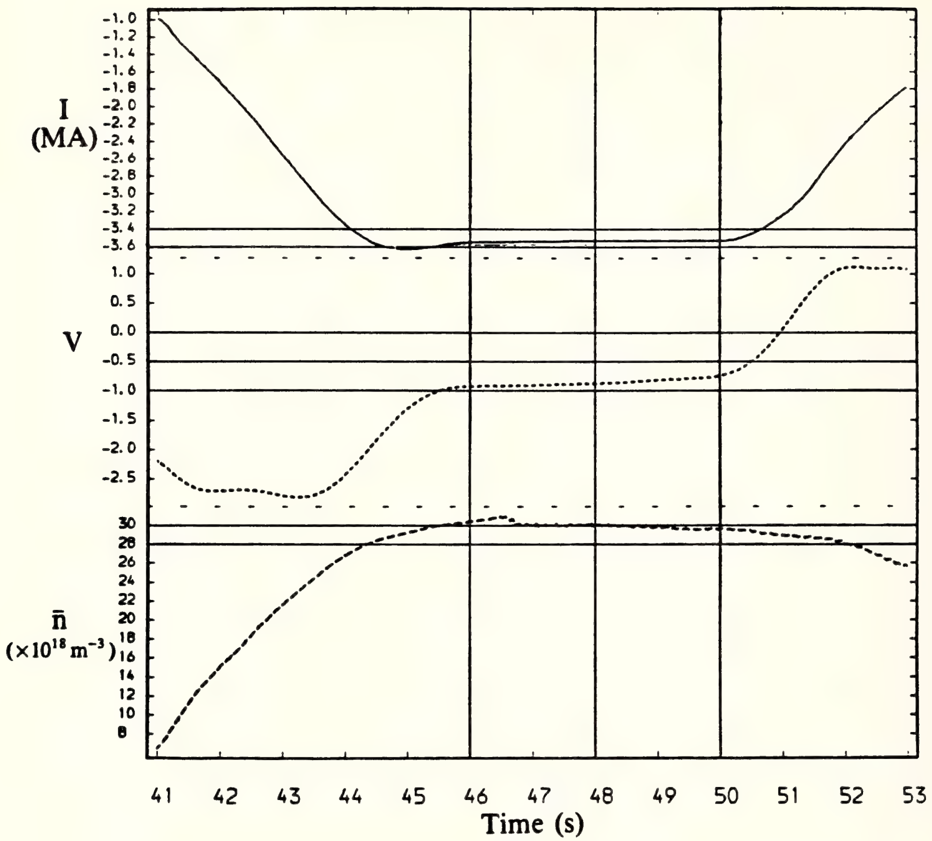

Figure 7 (a). Current, volts per turn (V) and line-average electron density as a function of time in JET (shot 3052)

and τ is the energy confinement time. I have used the approximate neo-classical formula for the thermal insulation, namely

$$n\tau_e \propto \frac{I^2\sqrt{T}}{Z_{eff}M_i^{\frac{1}{2}}} \cdot \left(\frac{R}{r}\right)^{\frac{1}{2}}$$

to extrapolate from the Hughes numerical calculations.

Here Z_{eff} is the effective charge of the positive ions, which can be measured by comparing the observed resistance with that calculated from neo-classical theory. The temperature T is the ion temperature, which is close to the electron temperature.

In the case of radiation the product $n\tau_e$ also scales as $\sqrt{T}/Z_{eff}$, but in this case with electron temperature T_e. In the case where the energy confinement time τ_e is not given in the published literature, I have used the relation

$$\tau_e = \tfrac{3}{4}\beta_\theta/\Omega$$

where Ω is the observed resistance per unit length, together with corrections for the small variation with time of I and of n_e,T_e. As can be seen, the observed thermal insulation as represented by the quantity $n\tau_e$ is up to about 20 times less than the ideal calculation, although at lower currents in TFTR this disagreement is much less.

The primary reason for the discrepancy (there is no direct evidence in JET experiments as such) is believed to be an excess in the effective electron thermal conductivity. The most detailed comparison between experiment and theory has been carried out using the Alcator Tokamak results (Gondhalekar et al, 1978). This conductivity has the opposite variation with density to that expected from collisions; and it is electron thermal conductivity which is anomalous. The same variation of confinement time with density n is shown empirically in JET (Figure 9).

The empirical law shown in Figure 9 suggests that if or when the number density n can be increased in JET, the product $n\tau_e$ should increase; and on that basis even a doubling of n will achieve a value of $n\tau_e$ close to that needed for self-sustained heating by the α-particles. The critical temperature required is about three times that so far achieved. To raise the temperature

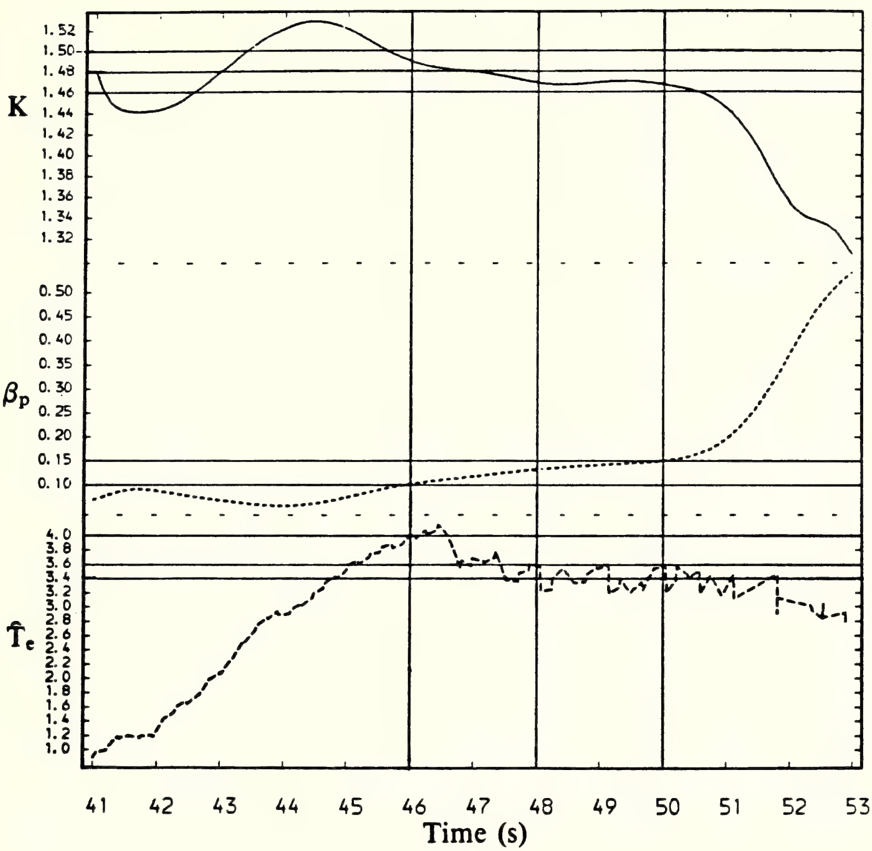

Figure 7 (b). Ellipticity (K) of minor cross-sections, poloidal beta, β_p ($\equiv \beta_\theta$ of equation 3) and central electron temperature, measured by electron cyclotron emission (shot 3052) courtesy of JET Joint Undertaking).

additional heating without degradation of confinement will be needed. Extensive auxiliary heating is now being installed on JET.

Clearly the experiments are reaching a most interesting stage where the twin problems connected with the density are being tackled; if the density is too high, the current channel appears to disrupt; if the density is too low the thermal insulation is adversely affected. Both problems are hopefully going to be understood and overcome. Experiments with additional heating will also throw light on these problems.

CONCLUDING REMARKS

If we return to the early observation of Professor Pollock, we can see that the electromagnetic pinch is now a well-established phenomenon, as indeed would be expected eighty years after a pioneer paper. It is perhaps noteworthy that so far only relatively modest extension of currents has been obtained: that is the initial observation used a lightning stroke of say 200kA. Currents of up to 4MA are now achieved. A limitation of the simple pinch as a source of hot very dense matter - which otherwise it would seem to promise, is the instabilities found, which grow on a timescale of the radius of the pinch divided by the velocity of sound, which is very short. Sustained large currents of up to the 4MA of the Joint European Torus are achieved as a result of the stabilization by longitudinal magnetic field established in the sixties. In the highly stabilized discharges of the tokamak type the need for externally generated fields of greater strength limits the degree of compression that can be obtained, but they show good promise of yielding the high temperatures and thermal insulation needed for power-producing thermonuclear reactions. Whether or not the simple pinch can be made stable by relativistic or degeneracy effects remains very uncertain; but if instability could be overcome, then perhaps the more extreme conditions speculated about in equation (5) and (6) might bring highly compressed matter into the laboratory.

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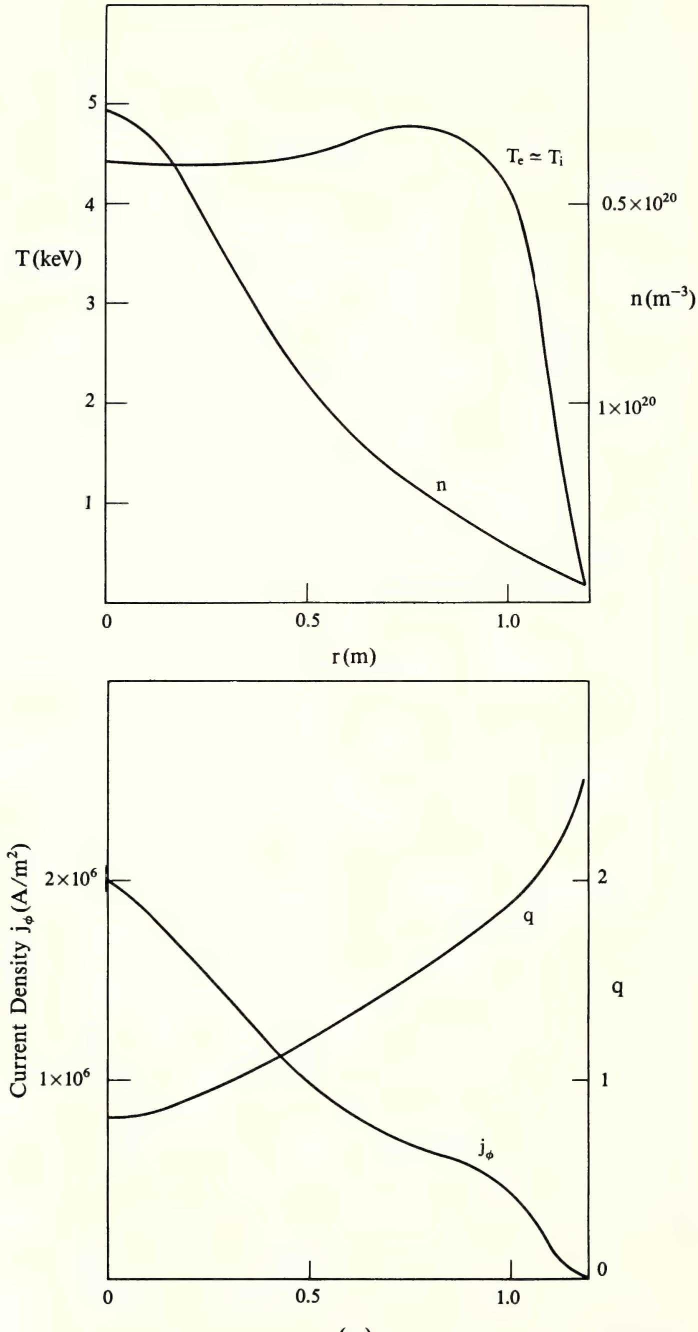

Figure 8. Calculated values of (a) the density and temperature and (b) the current density and safety factor q as a factor of minor radius in JET at a current 3 MA 10sec. after current start (M.H. Hughes private communication, and Burton et al, 1962). The corresponding calculated energy confinement time is 15.6s., and the mean density is $3x10^{19}m^{-3}$. The gas is pure hydrogen.

TABLE 2

Thermal Insulation of Large Tokamaks

OBSERVED and NEO-CLASSICAL CALCULATION of $\bar{n}_e\tau_e$

Experiment	GAS	I (MA)	T keV	Z_{eff}	$\bar{n}\tau_e \times 10^{-13}$ obs	$\bar{n}\tau_e \times 10^{-13}$ Calc ^{cm}
JET						
Shot 2029 [1]	H	3.1	2.5	2.8	0.5	12(4)
Shot 2471 [1]	H	3.0	2.5	2.7	1.1	17(4)
Shot 2523 [1]	D	3.2	2.9	4.0	1.9	9(4)
Shot 3055 [2]	D	3.0	3.2	2.5	2.7	15(4)
Hughes [3]	Calc.	(3.0)	4.4	(1.0)	-	47(3)
TFTR [1]	H	1.0	2.0	3.0	0.8	1.2(4)

1. Data from ref (35)
2. Data hand calculated from raw data
3. Hermes code calculation with I, Z_{eff} assumed; and mean density
4. Corrected from Hughes' calculations by

$$\bar{n}\tau_e \sim I^2 \sqrt{T/Z_{eff}} \quad M_i \ (R/r)^{1/2}$$

$\bar{n}$ is line of sight average number density of electrons.
τ_e is the energy confinement time, defined in refs.
Z_{eff} is the effective ionic charge, obtained from comparison between observed and calculated resistivity, allowing roughly for the trapped particle correction.

for a visiting professorship which has made it possible to be in Australia; and to Professor Hora who has provided the encouragement and facilities to prepare the lecture. In so large a subject as this, the work of a great many people is involved but I am especially indebted to Dr. H-O Wuster, Director of the JET Joint Undertaking for some of the data used in the analysis and figures, and to Dr. M.H. Hughes for his calculations and Figure 8.

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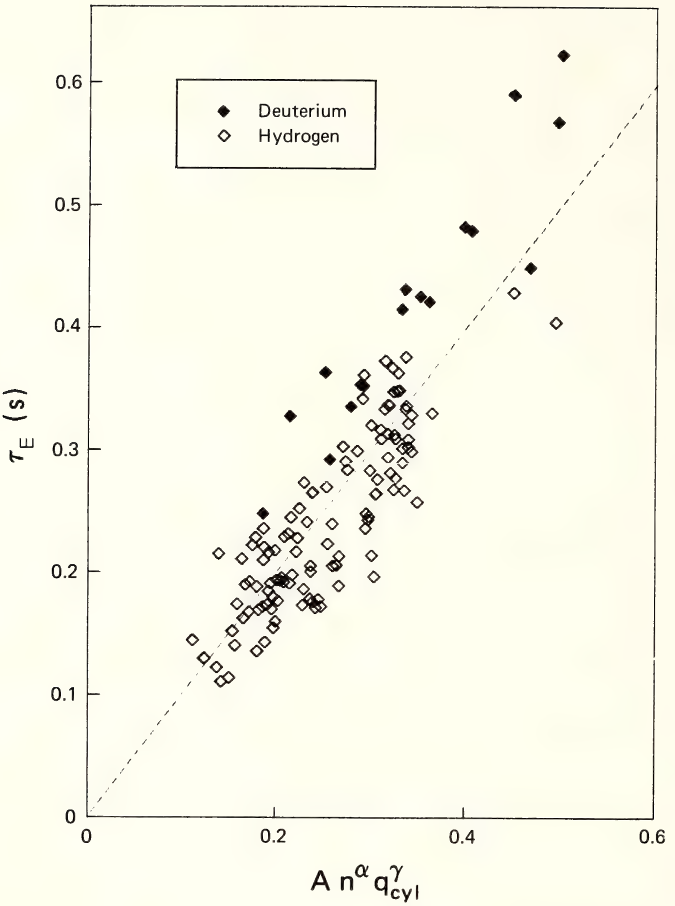

Figure 9. Energy confinement time in JET as a function of mean electron density n and safety factor q. The best regression fit gives $\alpha = 1.15$, $\gamma = 0.9 \pm 0.1$. The values of n are in the range 6×10^{18} to $3 \times 10^{19} m^{-3}$; q from about 2 to about 8. (Cordey et al in Rebut et al, 1985).

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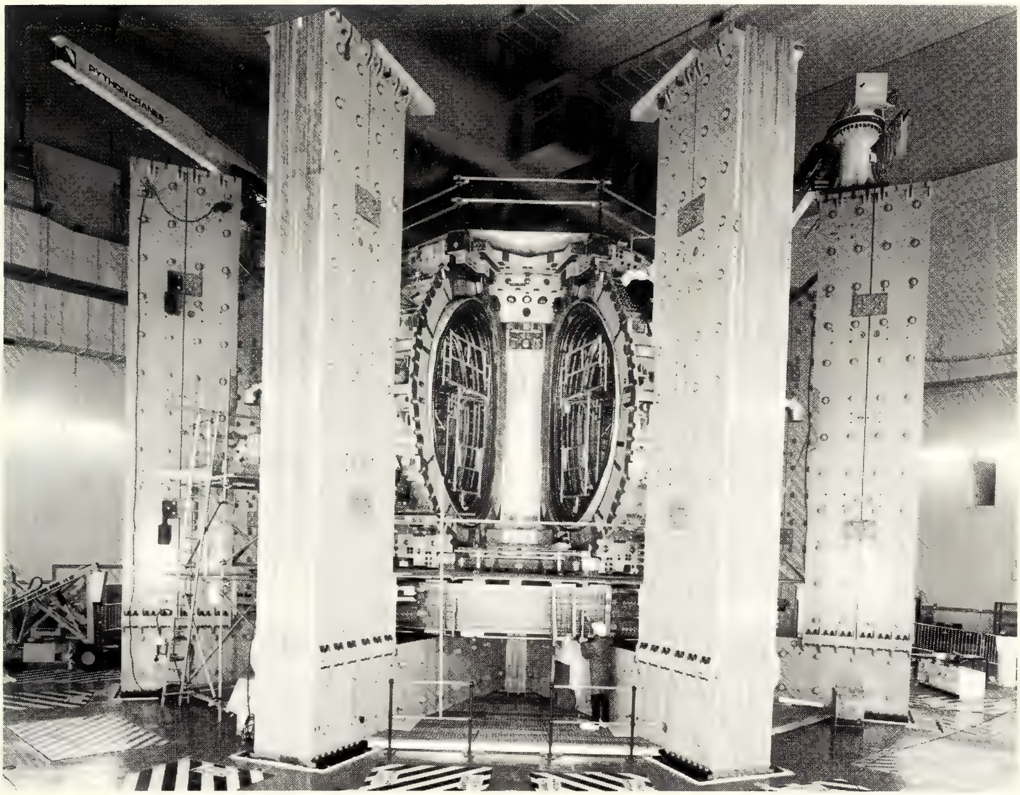

Figure 10. The Joint European Torus, JET, during construction. Four of the eight outer limbs of the transformer core await the top horizontals connecting them to the central core. The D-shaped minor cross-section of the toroidal vacuum vessel is seen in the gap left for the final octant, still to be fitted and welded into place.

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Why Bother About Science?*

R. HANBURY BROWN

Your Excellency, Mr. President, Ladies and Gentlemen,

First of all let me thank you for inviting me here tonight. After-dinner speeches are usually given to a captive audience who are partially anaesthetised by food and drink and so are less critical than when fully awake. But, as anyone who has talked to a scientific society knows, you can't take full advantage of that fact because your address may be printed in the Proceedings and read in the cold light of day by people who have not just had their dinner. That is why, of course, these speeches are apt to be "essays standing on their hind legs".

It was while rummaging about among the essays in the back numbers of your Proceedings, that I came across an address of awe-inspiring erudition and length by Professor Elkin on the occasion of your centenary in 1966. He made the point that science had now become so specialised that it was no longer practical to present original scientific work at the general meetings of your society. I feel sure that must have been true for quite a long time; in fact if we go back to the first meeting in 1867, the first paper presented to this society was entitled "Non-linear Coresolvents" and was delivered by none other than the Chief Justice of Queensland. What "non-linear Coresolvents" are I have no idea; what interests me is that a scientific paper should have been delivered by a member of the legal profession and, moreover, by one from Queensland!

Professor Elkin urged this Society to pay more attention to the History and Philosophy of Science and so - taking my cue from him - I have chosen as the text of my sermon, "Why should we bother about Science?" Let's ask this question first of the general public, then of the community of scientists and, if there is time, of me.

As a start - why do the man and woman in the street think that we should bother about science?

The New Scientist recently conducted a poll in the U.K. in which they asked a lot of people questions about science. They found, I am glad to say, that 45% of those people thought that science does more good than harm and that only 11% thought it does more harm than good; to be fair I should disclose that 38% of people thought that the good and the harm are about equal.

I am sorry to say that the New Scientist did not ask these people why we should bother about science, and so we must guess the answer, and for that reason I shall be very brief. I guess that in the minds of most people there would be no distinction between Science and Technology and that, with the exception of things which make very good TV such as the exploration of the Moon or the profile of Carl Sagan, they would see the principal value of modern science to be the good things which it has brought - better health, faster and easier travel and a wide variety of goods, services and entertainment such as ordinary people have never had before in history.

We may laugh at so-called primitive societies who build symbolic landing grounds in the hope that they will attract aircraft to appear out of the sky and bring goodies as they did in the last war; but we have our own Cargo Cult, Science, and the average member of the public knows about as much about the ideas behind science as the primitive, or should I say the "developing", people know about the ideas behind the aircraft which brought them goodies in the war. Like Francis Bacon in the early 17th century, most people equate the value of science with its practical benefits; they value it for new things not for new ideas.

Now let's ask our question "Why bother about Science?" of the scientific community and see if we get a better answer. We do need a better answer and it must depend on a better understanding of what science is all about. Let us hope that we shall find that better understanding among scientists, although I can't forget what Gerald Holton, Professor of the History of Science at Harvard, wrote in his article on the "Mainsprings of Scientific Discovery". He wrote:

> *"what seems to me to be most sensitive, the most fragile part of the total intellectual ecology of science is the understanding, on the part of scientists themselves, of the nature of the scientific enterprise".*

The scientific enterprise depends on three things; "the desire to know", "the initiative to find out" and "the awareness to apply". Your last speaker, the Hon. Barry Jones told us why we should encourage "the awareness to apply" and I am not going to say anything more about the importance of applied science; Mr. Jones has said it all. Lyndon Johnson once said of someone, "It's far better to have him inside the tent and pissing out, than outside the tent and pissing in". We can, I feel sure, extend those generous sentiments to the Hon. Barry Jones.

On second thoughts I might remark that, according to that sacred oracle the OECD, the amount spent on R and D by Australian industry fell from 0.48% of the Gross National Product in 1968 to 0.21% in 1981. This is low by international standards and is going down. I do hope that Mr. Jones, although now bereft of Technology, can, unlike King Canute, turn the tide.

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* Speech by Emeritus Professor R. Hanbury Brown, FRS, at the Annual Dinner of the Royal Society of New South Wales, held at the Sydney Hilton on Tuesday, 19th March, 1985.

It is only too obvious that we need to encourage the "awareness to apply", but why should we bother to encourage "the desire to know" and "the initiative to find out"? These qualities are, of course, as vital to applied science as they are to basic science but, not wishing to compete with Mr. Jones, I am going to talk about them, together, only in so far as they are the driving force behind basic science, or if you prefer that ridiculous phrase, "pure" scientific research.

As a member of the Australian Grants Committee for several years, I well remember how some journalists enjoyed themselves poking fun at some of the projects which we supported. They took it to be self-evident that research which cannot be seen to be useful, by someone who knows nothing about science, is a waste of public money. How should we have answered them? Why should we encourage and support basic research much of which, to the layman, looks to be useless?

The answers to this old question can be found in innumerable reports. They can be divided conveniently into two categories, the arguments that science is ornamental and the arguments that it is useful. The ornamental arguments picture the pursuit of pure science as a feature of a "high civilisation", like music and painting. To quote from the fairly recent Report of the Royal Commission on Australian Government Administration (1976):

> *"Like the arts Science is one of the graces of life, and its presence as an aspect of a particular society is seen as a mark of civilisation commanding respect from other societies. Since this activity can no longer be performed by the wealthy amateur, a civilised community will, it is argued, properly support it".*

This high-minded paragraph, I am glad to say, was not written by the scientists advising that Commission. It reminds me of the reasons which have sometimes been given for supporting a useless and decadent aristocracy. I can hardly imagine a worse argument for the support of science by a government which contains very few people who seem to be even interested in science.

It is one of the paradoxes of our time that we should be governed by a bunch of people most of whom seem to know very little about science - and yet modern science is, I would say, the major achievement of the present century and one of the principal forces shaping our world. In fact I know of only one legislative body in the world where you could hear a well-informed discussion of a scientific question and that is the House of Lords. By-the-way I am not complaining that our parliamentarians don't know a lot of science; I am complaining that they know very little about science - which is quite a different thing.

This brings us to a much more robust reason for supporting basic research - that in the long term it is useful. In its classic form this argument tells us that the only difference between basic and applied research is one of time scale. All knowledge, however apparently useless, eventually results in a better mouse trap. This is an argument of which we astronomers make considerable use when we are looking for money. The study of the Sun, so we say, has led to an understanding of the process of nuclear fusion, and in the long run will give us a new source of unlimited power for our power stations.

On a more local level my own Department of Astronomy has proved its usefulness to Australia Post, an organisation which surely needs all the help it can get. Only the other day one of our lecturers was hailed by the postman who was changing the little notice on the post-box which tells us when the next collection will be, "Hi, mate!", he called across the road, "can you tell me which is A.M. and which is P.M.? I can never remember". I am proud to say that our lecturer knew the answer.

The classical and most solid argument for basic research is that it is the seed corn of the future practical benefits which will flow from applied science, and it is our responsibility as scientists to put this argument over to the public and to the Government as clearly as possible - much more clearly than we do at present. To do that we need to trace the connections between basic scientific research and the progress of applied science.

Painful and paradoxical as it may seem to the tidy-minded people who would like to plan basic research, the popular, often self-righteous demand that all research should be relevant to our social needs is one of the greatest dangers to the advance of science, and hence to the long term satisfaction of those social needs. Indeed it is a hard historical fact that the future needs of our society have been well served in the past by research which was not constrained by those very needs. To insist on relevance in research is like insisting on naturalism in art; you end up with something comfortably familiar but not new.

Appoint a committee of eminent and concerned persons who know all about what society needs - not difficult to find - and very little about science - also not difficult to find - and give them the job of dispensing funds for basic research - and what will they do? The most likely thing - as happened quite recently in Australia - is that they will put the funds largely into medical research. What could be more important to the community than to seek a cure for cancer? In giving money to medical research - with the best possible intentions - they can be sure that they are "doing the right thing" - as we are told to do by the slogan on our rubbish bins.

What is wrong with that? Quite simply that history of science suggests that if you want to make a significant advance in medical science you should give the money to someone who is <u>not</u> working on medical science. In actual fact many of the major advances in medical science in the last 100 years, such as the discovery of X-rays and our understanding of cellular and molecular biology, have come from people who were not concerned with medical problems at all. Advances in molecular biology, which have greatly advanced our understanding of cancer, were made by physicists who in turn depended on advances in computing and applied mathematics. An understanding of the cause and cure of cancer may well come from some study which is apparently remote from medicine.

What the scientists are telling us is that the progress of applied science depends upon the progress of basic science and that, although relevance may be a good guide to applied research, it is a poor guide to basic research. Science advances on a side frontier and we can only make very limited advances on a narrow part of that frontier. When we bother about the health of basic science we must bother about the health of all science.

As this is a gathering of scientists and not a grant-giving agency may I end my talk by giving very briefly some of the answers which I, personally, would give to the question - why bother about science? As far as I am concerned the answers which we have given so far are the practical answers which we must use to enlist support for science; I believe that they are only a part of the whole story.

May I first add to the reasons for not constraining basic research by its relevance to our immediate social needs. My argument is unlikely to impress anyone who is actually responsible for funding basic research, but it impresses me. Copernicus removed the Earth from the centre of our picture of the Universe, but he didn't remove us; we still see ourselves firmly in the centre of the stage and nature as being there to serve our needs. One of the dangers of taking that view is that in planning our attempts to understand the Universe we are apt to forget that it may not have been designed with our welfare in mind, and that it is advisable to investigate it on its own terms and not in terms of its relevance to our own very limited concern with the practical application of what we discover. We must be guided more by the internal logic of science than by the external logic of our own needs.

Modern physics has show us that nature is very, very queer, even alien, and modern astronomy has shown us that the Universe is so vast that it is difficult to believe that it can all have been made for our benefit. I saw Mr. Universe on the telly the other night, Mr. Universe mark you, not Mr. Solar System; let us hope that on some other planet God has done better. As J.B.S. Haldane once said:

> *"The Universe may not be only queerer than we suppose, it may be queerer than we can suppose".*

We cannot, I believe, limit our interest in nature, nor the questions which we ask of her, to things which we expect to be useful.

I should also like to enlarge the argument that science is useful, because I do not believe that what is arguably the dominant influence on modern culture should be valued mainly for its utility. As I see it, the principal function of science in our society is not to produce goodies, but to show us the world *as it is* and not as we would prefer or imagine it to be. Science is our essential link with reality and if we fail to maintain that link then there is no longer any "nature's truth", there is only "your truth" and "my truth", and we are in danger of losing the distinction between fact and fiction, and between science and magic.

Francis Bacon said, "God forbid that we should give out a dream of our imagination for a pattern of the world". History shows us how true that is. We have only to look at the ideas of the Aztecs about the need of the Sun to be sustained by human sacrifice, or, nearer our time at the racial theories of the Nazis, to see what can happen if our conjectures about the nature of the world lose touch with reality.

The point which I want to make is that it is the new understanding, the new perspectives, the new ideas and the distinctive values which science brings to our society which are really of much greater value to us than many of the practical applications for which most of us value science. Our modern understanding of the genetic code and the mechanism of inheritance is a profound advance in our understanding of living creatures; our modern vision of the world seen in the completely new perspective of time given us by the theory of evolution, and in the completely new perspective of space given us by the discovery of the realm of the galaxies, is of profound importance to our ideas about our place in the Universe and so to our ideas about the meaning and purpose of life. To be reminded, by the sheer scale of the Universe, that man is not the measure of all things is, I suggest, a good thing, especially in a largely irreligious society.

The ideas of modern physics, for example the new ideas about causality and about uncertainty brought to us by the quantum theory, are of the greatest interest to philosophy, a subject which could well do with some fresh ideas. The values of science, which are encouraged by the actual practice of scientific research, such as intellectual honesty and international collaboration, are also important to society. For instance, let me remind you that one of the major dangers to any civilisation is that it should become credulous, and that the antidote to credulity is scepticism. Organised scepticism, so Robert Merton tells us in his classic study of the sociology of science, is one of the major values of science. Indeed the first commandment of science is to value the "truth of fact".

To sum up, a knowledge of what actually *is* in this mysterious world is central to the discussion and solution of all our major problems, and is essential to any wise vision of a better world. To seek that knowledge, and in so doing to cultivate the pursuit of the "truth of fact", is in my view the most cogent reason for bothering about science.

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Emeritus Professor R. Hanbury Brown, FRS,
37 Beatty St.,
Balgowlah, N.S.W., 2093, Australia.

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Biology at the Frontier*

CHARLES BIRCH

Alexander the Great after all his conquests is said to have sighed because there were no more worlds to conquer. Yet as he stood on the banks of the Jaxartes he could hardly have failed to see the vast stretches of untamed territory lying before him, even if he had no inkling of the unknown new world across the seas. An Alexander the Great of Victorian Science Lord Kelvin took his stand on a metaphysical Indus and boasted that within a short time the whole world of nature would be subjugated. There would be no frontiers left to science. Seldom was complacency so quickly deflated. In three successive years the discovery of X-rays, of radioactivity and of the electron shattered the Victorian concept of the world as a vast three dimensional billiard table with sharp boundaries on which scientists played a mammoth game of snooker.

More recently the molecular biologist Gunther Stent wrote a book called "The Coming of the Golden Age", the theme of which was the end of the sciences. Science will have delivered the goods, hence the Golden Age. He elaborated his thesis further in "Paradoxes of Progress", 1978, and spoke to us about that in this very lecture theatre a few years ago. There would soon be no frontiers left for science. When I was completing my undergraduate career in the University of Melbourne and was thinking of the possibilities of a research career, I remember bemoaning with a fellow student that all the major discoveries had been made and certainly the easy ones. I have learnt a lot since then.

Indeed one of the things I have learnt I would put into the principle - while knowledge increases arithmetically, ignorance increases geometrically. In other words, every advance in knowledge creates more problems than it solves. There is never likely to come a time when there will be no more intellectual worlds to conquer. The contemporary scientist finds himself more in sympathy with Sir Isaac Newton's view of his own achievements: the great ocean of truth still lies all undiscovered before us. The scientist is like someone perpetually cleaning out a cluttered basement. No sooner is the basement outline seen when someone finds a cleverly hidden trapdoor leading to a vast sub-basement. In scientific research there is one general law - the unexpected always happens. So we can never really predict but we can anticipate new frontiers at each step of really probing research.

The biological sciences are undergoing a transformation which has only just begun but which promises to be so revolutionary in opening up new frontiers that professors of biology who will take their place in this University in future decades will bear little resemblance to those of past decades, that is unless the University of Sydney gets stuck in a rut which heaven forbid. Even I bore little resemblance to any of my forebears in the Challis Chair of Biology. The contrast will be even greater in the future. One transformation is well under way. The other has hardly begun. The first I shall call the concept of levels of biology. The second is the holistic concept of the unity of all life.

Biological Levels

When I first came to this University, biology was subdivided into botany, zoology, genetics, microbiology and so on. It was a simple taxonomic breakdown. And it didn't work, at least from the time when biologists came to realize that the same processes went on in animals as in plants as in microorganisms. Their biochemistry is extraordinarily similar. So is their genetics. Genetics started with plants, continued with fruit flies and is now largely a study of microorganisms. It became pretty clear that both research and teaching in biology was not served adequately any more by persisting with the old subdivisions of biology. The alternative was to think of biology in terms of levels of organisation. There are four: molecular, cellular, the whole organism and the population. Some might want to add a fifth they would call ecosystems, i.e. the organism and its environment. I reject that as totally artificial because at every level the entity, be it molecule or cell or tree, can only properly be conceived as having an environment. So I say no more about it.

The transformation of biology in terms of levels has primarily taken place at the two ends, the molecular end and the population end, and that is where the growing points are. The greatest conceptual advance at the molecular level is, I venture to say, the concept of molecular ecology, a phrase invented by Paul Weiss, a developmental biologist. Just as the Newtonian concept of the universe as a billiard ball universe has given way to a much more ecological view of matter, so too the transformation is taking place in molecular biology. No longer are genes particles on chromosomes. No longer do we talk about particulate inheritance. What we call genes are what they are by virtue of their relation to their environment. What a gene can do depends upon neighbouring genes on the same and other chromosomes, and on the other environments of the cell in which the genes find themselves. This is an ecological concept.

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* Address to the Faculty of Science Centenary Celebrations at the University of Sydney on April 17th, 1985.

You cannot simply say that gene x does this. What it is and what it does depends upon the environment. The part of the DNA that is expressed at any time depends on the environment, and that same environment keeps other parts of the DNA suppressed. Nothing is completely determined. Indeed the concept of degrees of freedom becomes quite relevant at this level of biology. If I were to go on with this story, as it can now be told in some detail, at least for some bacteria, I think I could persuade you that we might well talk about the life of the DNA molecule. Things go on at that level which we never dreamed of, and which we thought belonged only to cells and higher organisms. The American biologist, Lewis Thomas wrote a book, "The Lives of the Cell"; someone might well write a book entitled "The Lives of the DNA Molecules". Biology has advanced at the molecular level mainly so far by studying microorganisms, and particularly one, *Escherichia coli*. But as we seek to understand and control other organisms besides microorganisms, future thrusts are moving fast in that direction. And when you come to think of genetic engineering in which genes from one organism are planted into another one as far apart as a human and a bacterium, then you realize that it too is at centre ecological. We are giving genes new homes, and as yet we are uncertain as to how the new environment will influence all their activities, and for that matter the activities of the genes in the recipient organism.

I said that the transformation of biology is taking place primarily at the level of the molecule and secondly of the population. Population in this context means populations of organisms such as mosquitoes or people. The principle of population ecology is that organisms can only be properly understood as we study them in relation to the numbers and kinds of their own species around and the numbers and kinds of other species in their environment. Most of the advances in population ecology have come from studies of organisms that we want to make rarer because they are pests, or that we want to make more common because of their usefulness to humans. And more recently there is the added component of threat to all life of changes in the habitats of the earth due to human activity and changes in the atmosphere surrounding the earth. A proper study of human ecology would include all those organisms on which human life is dependent, not only those that serve as food but those that maintain what we call the life-support systems. These are the organisms that contribute to the stability of the composition of the atmosphere and the degradation of wastes on land and sea. Every year sees the disappearance of more and more species from this planet and yet we cannot predict the consequences of the extinction of a given species any more than an airline passenger can assess the possible consequences of the loss of a single rivet from the wing of a plane in which he or she is travelling. So concerned is Paul Ehrlich that in his book, "Extinction" he argues for the saving of all species from henceforth as part of the life-saving of human life. So I see the studies of population ecology extending to many more species than we have at present studied, and this is a vast programme. The European rabbit in Australia is one of the best studied animals from an ecological point of view. It involved the research of many full time biologists for close on two decades and still there is more to do. We can expect to know little about the population ecology of any species unless the study covers at least a decade and involves many workers. To date these sorts of resources have only been available for work on species of well established economic importance such as the rabbit in Australia. A major problem in this work is that both the time needed and the resources needed are more readily available in research institutes than in universities. Yet the universities need to become much stronger in this field of work. The quality of the teaching of a university is related to the quality of the research in that same institution, so it is important that both go together. I cannot see that happening in Australia to the extent needed unless more research institutes are established in the universities themselves. There are already some good examples. The Waite Agricultural Research Institute of the University of Adelaide is outstanding in this respect as is the Walter and Eliza Hall Institute associated with the University of Melbourne. To set up biological research institutes with adequate staff and buildings is now an expensive operation far beyond the normal resources of the University of Sydney. The University of California at Berkeley has recently been allocated $41 million of state funds for a building alone for the Life Sciences. A new Institute for Basic Research in the Life Sciences has recently been established in Odawara City some 80 km from Tokyo. It has an initial outlay of $21 million with 50 scientists and 50 support staff. It is not in a university and belongs to Meija Milk Products. In 1984 alone six new research institutes in the Life Sciences have been opened in Japan all of them outside universities. Barriers between universities and industry are said to be just too high for effective research cooperation in Japan, so there has been no accompanying boom in research in Japanese universities. A major problem for the future of the life science in Australia will be whether the universities will continue to fall behind Commonwealth Government Research institutes in facilities for research. It is these rather than private industry which do much of our basic research. I find it sad that cooperative research between universities and CSIRO within the University campus is less now than it used to be. In the past that was one way of sharing the wealth available for research.

A New Holistic Emphasis in Biology

Here we come to the most difficult frontier of all to explore. The physicist, Paul Davies, in his book "Superforce: the search for a grand unified theory of Nature" (1984), said, "Like many compelling images it may turn out be a mirage, but for the first time in the history of science we can form a conception of what a complete scientific theory of the world will look like". In this exploration the physicists are far ahead of the biologists and the danger is that the new complete scientific theory of the world will have no contribution from biology. If that is so, it cannot be a complete theory of the world. The time is ripe for biologists to adventure into quite uncertain seas of unknown depths to seek a unified theory of nature. It needs tremendous imagination. I remember the biologist C.H. Waddington saying to me some years ago that in quantum physics you are not

likely to make a contribution unless you have outrageous ideas, to think thoughts that no one has thought before. Waddington was one of the few biologists of his generation who attempted to do this in biology. Another great biologist of our time J.B.S. Haldane said, "Now, my suspicion is that the universe is not only queerer than we suppose, but queerer than we can suppose". (J.B.S. Haldane, 1927, p.286). His distinguished father, J.S. Haldane, also a biologist went further in his Donnellan Lectures, "The Philosophical Basis of Biology" when he concluded "Personality is the great central fact of the universe. This world, with all that lives in it, is a spiritual world." And that is why he made this seminal prediction about the future of science, "That a meeting-place between biology and physical science may at some time be found there is no reason to doubting. But we may confidently predict that if that meeting-place be found, and one of the two sciences is swallowed up, that one will not be biology." (A. Hardy, 1965). And again his son J.B.S. Haldane said, "And though today the theoretical physicist is and must be the principal type of world builder, the biologist will one day come into his own in this respect." (J.B.S. Haldane, 1927, p.281). The similarity between father and son was a source of some confusion. When J.B.S. Haldane was asked on one occasion was he related to J.S. Haldane, he replied, "that depends upon whether identity is a relationship."

Who are the biologists in our time who have attempted exploration in that direction? There are few of whom the following are outstanding: C.H. Waddington, Bernard Rensch, Theodosius Dobzhansky, Sewall Wright and W.H. Thorpe, and I am happy to be able to include one Australian, indeed my first teacher, Professor W.E. Agar, who wrote a little known but important book, "A Contribution to the Theory of the Living Organism" (1943). I believe that all of them would have agreed with J.S. Haldane that personality is the great central fact of the universe. Life, as I have argued, can be understood at different levels, but the most elusive level is the phenomenon of mind and consciousness that we ourselves are aware of and which we assume is characteristic of at least some of our non-human colleagues. But it is developed in us in a paramount way. This is the most incredible aspect of the living organism, that it is a subject and not just an object; that is to say a being that experiences and has feelings. You could hardly call a human who was without any feelings at all alive in any real sense. To live is to be responsive in a feeling way. Now you may say give the brain physiologists time enough and they will come up with an answer. But, oddly enough very few of them are interested in the problem of mind and consciousness. There are exceptions, such as John Eccles and Roger Sperry, both of whom insist that consciousness is a reality and itself a causal agent. The search for light in this area is doomed as long as biologists suppose that classical physics can tell them what the real world is like and that life and consciousness are properties that emerge like secretions from bits and pieces that are completely without any elements of life or mind. That is to believe in miracles. That model falls apart especially now that physicists such as David Bohm claim that there are no fundamental particles. The image was all a huge mistake stemming from Democritus and thence from the interpretation of Newton in terms of a billiard ball universe.

Physics is moving onto the new frontier away from its old fundamental particles to a more unified theory that is much less mechanical. As one of the architects of the new physics David Bohm remarks, "the question of whether the basic laws of physics are in fact mechanical or not is of the utmost importance to biology .... it does seem odd therefore that just when physics is moving away from mechanism, biology and psychology are moving closer to it. If this trend continues, it may well be that scientists will be regarding living and intelligent beings as mechanical, while they suppose that inanimate matter is too complex and subtle to fit into the limited categories of mechanism." (Bohm, 1969). The point is that the present categories of mechanism within which the biologist constructs models of life are too constraining. They will not enable us to move into the new frontier in the way in which physics is moving. This is not to say that mechanical models are invalid. On the contrary, they are basic to biology, but they have their limitations which we should now recognize instead of being blind to the constraints they impose on our thinking.

We say this is an age of science. This is a misleading half-truth. Ours is a transition period of an incomplete, unbalanced science lacking basic clarity because it has tended to ignore internal factors and causes, and has included only external causes or relations. We are fundamental ignoramuses. There is an enormous gap between what we experience and our understanding of that experience. Experience which involves internal relations cannot be understood in terms of a science that deals only with external relations. People are not just objects that are pushed around by external relations. We have internal relations that transform life. And why is this not true of other living organisms and for that matter of all entities.

I am arguing for a much more adventurous and exciting assault on life. We have become too restrictive. The brain functions as a sort of reducing valve that shuts out the universe so that the individual can do what is immediately in front of him. The million signals a second must be reduced to a few. But the creative intuition and imagination can be trained to remain open to the possible worlds yet to be discovered by a more holistic science. We have to lift our gaze from the immediate to the wider horizons. But that will demand a scientific revolution as large as that of the 16th century in which we see in T.S. Kuhn's terms a new paradigm or a new gestalt. To see a new gestalt is not to analyse things into pieces but to have a vision.

Scientific rationality has gained much for the world but it is also letting us down. In so far as it leads to a mechanistic model of life and the universe, it is contributing to the current world malaise of meaninglessness and despair. It is no mere coincidence that the search for meaning through way-out sects and fundamentalist religions is growing fastest in the new centres of technology such as silicon valley in California. The technological view of the world is life threatening. We need a life enhancing view of the world. It won't come from the simplistic and fundamentally false doctrines of the sects. It is emerging from a new awareness amongst some young people who have not turned their backs on science but who seek a

transformation of science because of the depth of their human experience. This is happening simultaneously in a number of places like flowers in spring, beneath the ugly wreckage of a past civilization. I think in particular of a group of students in Amsterdam who provide a course which they call "The transformation of Science". In their discontent is an expression of a widespread determination to reconstruct a view of life and a way of living in the light of a rich experience and a rich conception of what it is to be human. In that great endeavour biology has a critical role to play.

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5a/73 Yarranabbe Road,
Darling Point, N.S.W., 2027,
Australia.

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Science v Law: The Next Century*

THE HON. JUSTICE M. D. KIRBY, CMG

100 YEARS ON

It is extraordinary to think that the proposal to establish a Science Faculty within this University attracted vehement opposition, at the time, by the Principal, the Reverend Charles Badham (Footnote (1)) The first lecture in the University had been given by Professor John Smith in 1852. He chose the subject of chemistry and was no doubt condemned as a "trendy" for his pains. Yet the establishment of a Faculty, to provide coherence to the study of science, engendered passionate opposition. It signalled the move of this University from classical studies to the modern era. It is not a coincidence that the admission of women and the establishment within a year of the Faculties of Medicine and Science occurred with harmonious contemporaneity. The Challis Bequest and the social ferment of the 1870s and 1880s provided a ripe field for change and reform.(2) It is not without irony that the Physics Department was housed for a time in the present Badham building. The Reverend Charles, clinging to the old ways and the old beliefs, is doubtless glowering down upon us, even now as we meet to celebrate and to remember his error.

The century between those vigorous debates and this celebratory week has been marked by human disaster and triumph. The empires which were reaching their apogee a hundred years ago have faded away. The British fleet which swept the oceans and protected far flung colonies, such as Australia, has sailed home. On the international stage, blood and destruction have been the watch words of the century. And to them science has contributed notably and imaginatively. As Robyn Williams insistently reminds us, as often he can, half of the scientists that ever existed are alive today; and half of them are working in connection with military science.

But triumphs there have been, as well. Nuclear fission, though pregnant with possibilities of disaster, is by any account an amazing scientific development of potential utility as well. The micro-chip and informatics are in the process of revolutionising our society, its organization, its work and its leisure patterns. Biotechnology presents stunning possibilities, from in vitro fertilisation through genetic engineering to human cloning and manipulation of DNA. In my many hours of "leisure", since returning to judicial duties, I have found the time to read in the field of science. Just to prove that there is some value in book launching, I have to tell you that recently, as a reward for a particularly provocative book launch, I was given John Gribbin's startling book *"In Search of Schrödinger's Cat"* (3). This little book unveiled for my ignorant eyes the pioneering work of Schrödinger and the other physicsts of the 1920s who developed the theory of quantum physics. Amongst other things, this theory led on to the atomic bomb, lasers, the micro-chip and the understanding of DNA. The book provides an accessible account of what is probably the most important discovery of the past 100 years of science. It bears out Niels Bohr's stricture:

> "Anyone who is not shocked by Quantum Theory has not understood it". (4)

The world of Newton and even Rutherford was a world of laws, of order and of basic rules. "Rules of Nature" if you like, rules of God for the believers. After quantum theory, it is suggested that there is no such thing as certainty - our world being simply the product of many processes. It is little wonder that the world of new science is such an uncomfortable phenomenon for the lawyer. Trained to understand, and believe in, order, rules, authority and certainty, the lawyer finds the modern world of science and its distaff offsping, technology a puzzling and uncomfortable place.

In part this is because of educational streaming. Lawyers tend to be those who, at school, were noted for their skills in English composition and fascination with the history of battles long ago. Scientists tend to be those who liked a mathematical problem or puzzled over algebra, chemistry and physics. The result is an early separation. Even in universities, where the universe of disciplines should come together, it is rare indeed to see a science/law graduate. Last week, amidst 1,000 handshakes at Macquarie University, I was privileged to greet but one law student who had graduated with a science and law degree. There are few of them. Interestingly one of them, a distinguished graduate of this Faculty, is Justice Lionel Murphy of the High Court of Australia. He is one of the few science/law graduates I know and the only such graduate in the judiciary. Perhaps his original approach to legal concepts derives from his impatience with lawyerly beliefs in the certainty of rules, the unambiguity of the meaning of words and the objectivity of the judicial process.

However that may all be, I have tardly laid my ground. The law loves certainty, order and authority. The problem for today's lawyer (unlike his counterpart outside the university in 1885) is

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* Address given to the Faculty of Science Centenary Celebrations at the Great Hall, The University of Sydney, on Wednesday, 15th May, 1985.

The Hon. Justice M.D. Kirby, CMG, is the President of the Court of Appeal, Supreme Court, Sydney; Chancellor, Macquarie University, Sydney; formerly Chairman of the Australian Law Reform Commission. The views stated are personal.

that the world today is engined by science and technology. It is a world of rapid change, of shattered rules, of developments incomprehensible even to the highly intelligent and educated. It is a world of few certainties where science fiction and science merrily exchange places.

A CHANGING WORLD

So far I have been a little critical of my own profession. But it is not entirely the fault of lawyers. As Barry jones pointed out in January of this year:

"The sheer complexity of 20th century science is a major factor inhibiting community understanding and support." (5)

According to Barry Jones, there has been a long standing anti-intellectual tradition in our country and it is still "alive and well today." He quoted the well-known limerick of the 1920s, possibly anti-Semitic and certainly anti-intellectual:

"I don't like the family Stein
There is Gert, there is Ep and there is Ein.
Gert's writings are bunk,
Ep's statues are junk,
And nobody understands Ein."

According to our erudite Science Minister, Gertrude Stein's reputation is safe. So are Jacob Epstein's statues. And Albert Einstein has been elevated to the pantheon of demigods.

"Einstein remains the only 20th century scientist whose name and image are generally recognised by literate people, followed at some distance by Marie Curie. There is Freud, of course, but his status as a scientist would be hotly contested. Who runs third? There might be a scattered recognition of Rutherford, Bohr, Fleming (though hardly at all for Florey who was more important, except for the fortunate holders of Australia's $50 notes), Watson and Crick. Jeans, Eddington and Halldane were well known until the 1940s but forgotten now. But Planck, Heisenberg, Dirac, Godel, Schrödinger .... Medawar - they retain their awesome reputations amongst the initiated without ever having made any impact on popular consciousness." (6)

The point that Barry Jones makes is that until the Second World War, science was a subject of intense popularisation. At the time of the foundation of this Faculty and right up to the Second World War, the exposition of science and its impact on life was highly visible and could be understood by the intelligent layman, who put his mind to it. Today "this honourable tradition has largely disappeared ... scientists are now ultraspecialised, hermetically sealed off not only from the community, but often to other practitioners in related disciplines." (7)

Nowadays, because of its mystery, the popular image of science is Dr. Strangelove. A weakness of science is its psychological remoteness - an audience no longer relates to it. Whereas at the time of the foundation of this Faculty, popular science "revealed" mysteries, science today, and in the next century, appears esoteric: constantly saying that the structure of the universe is infinitely mysterious - "the more we explore the less confident we can be about our capacity to provide meaning".(8) Technology and science are linked to the frightening worlds of the atomic bomb, the work destroying micro-chip and weird and manipulative biotech. Dangers are seen in it for man's work and life-style, for man's nature, indeed for man's very existence and survival as a species. And that, if I can say so, is at once the opportunity and risk of the law in its relationship to science in the century ahead.

One of the prime objectives of this celebration should be to rekindle concern about the relationship between modern science and the community. Barry Jones in his second Whitlam Lecture in 1984 called for recognition of science as an element in our political (and I should therefore say, legal) culture:

"There is a major communication gap in scientific areas which I can illustrate by referring to two close friends in the ministry. When Michael Duffy talks to colleagues about the triumph of Essendon .... or John Brown extolls the virtues of the Parramatta "Eels", they establish an immediate bond with their listeners: they do not have to establish their fram of reference. I envy them this facility. .... When I want to share some new intellectual excitement, for example,

Ilya Prigogine's fundamental questioning of the second law of thermo dynamics .... I run the risk of sounding pretentious, a prig, a bore, or - even worse - an intellectual. Australia must be one of the few countries in the West where "intellectual" and "academic" are still terms of abuse and yet is it unreasonable to argue that the implications of Prigogine's work .... are potentially more exciting and moving even than Essendon's recovery in the final quarter?"(9)

So you can imagine how delighted I was to read of the proposed establishment of a new institution, the Centre for Human Aspects of Science and Technology as a body which will look at human problems and possibilities arising from the scientific and technological revolution. I would want to give my enthusiastic support to this proposed venture. I congratulate the Faculty of Science for adding a social and human dimension to its centenary celebrations. Indeed, one of my disappointments in the program for the centenary of the Faculty of Medicine of this University was its neglectful inattention to the social implications of medicine. It is timely and it appropriate that the Faculty of Science should contemplate this initiative. One has only to reflect upon how strange such a proposal would have seemed to the Reverend Badham and the founders of the Faculty to pose the question: What will it all be like in 100 years from now? Will this Great Hall still be here? Will humanity have survived? Will our human form have changed, adapted by genetic engineering and implanted micro computers to enhance the capacity of the science graduate elite of 2085? Comtemplating these things makes it important to state at the outset that any review, in the tolerably available time, of the law

and science in the next century must be problematical and superficial in the extreme. Futurology is a necessary but partly discredited activity. It is a dangerous activity in the context of the changes of science and technology which must be regarded as now in contemplation - let alone those which will come from the fertile mind of man in 100 years.

SCIENCE AND LAW

Talking about the relationship of science and law has come to be in vogue in recent years. In May 1982, delivering the inaugural presidential address to the Law Section of the Australian & New Zealand Association for the Advancement of Science (ANZAAS), Professor Douglas Whalan of the Australian National University asked, relevant to the science/law relationship: "Are lawyers really necessary?" I will not give too many guesses about his answer. Yet it is notable that it took 52 Congresses before law was even admitted to ANZAAS. Of course, the scientists who kept law out were only equalled by the lawyers who wanted to have nothing to do with science. The concept of law as a set of positive rules, laid down by a sovereign able to impose sanctions for their breach, distinguished law from the work of scientists and technologists. But the growing realisation of the social importance and uncertain operation of the law and of science, together with an appreciation of the functions of the social sciences in understanding it, led to the final admission of the discipline to ANZAAS in 1982. It is now one of the more robust sections. Perhaps the scientists should be on their guard lest strident lawyers seek to take over the organisation!

Professor Whalan called attention to the formulation of laws on privacy (relevant to informatics) and transplantation (relevant to biotechnology). He stated that we were then starting to look at in vitro fertilisation and plant patents. He pointed out that we were beginning the debate on genetic engineering and the law and on whether life should be patentable. Perhaps of interest is the fact that the very first article in the 1982 volume of the *Australian Law Journal* is on "The Use of DNA and the Law in Australia" by Professor Douglas Fisher of New Zealand.(11)

Professor Whalan's thesis was simple. Lawyers, he contended, should anticipate rather than simply react to scientific developments. They should not monopolise the law reform and law development process. On the contrary, they should invite the participation of the scientist and technologist but also of the economist, the philosopher and many others as well.(12)

Unsurprisingly the balance of Professor Whalan's essay was directed at issues of biotechnology; foetal experiments, cloning of mammals, in vitro fertilisation and what he described as "the genetic engineering dilemma". I say 'unsurprisingly' because of the fact that Professor Whalan has been appointed to the DNA monitoring committee of the Federal Government. He is the lay-representative on that committee of scientists. But what will be the participation of scientists as the law increasingly addresses itself to public concerns about developments of science affecting society?

Professor Fisher is another law professor who has interested himself in the science/law relationship. In 1984, delivering the Hudson Lecture at the Wellington Branch of the Royal Society of New Zealand, he adopted as his topic the rather melancholy title "Science and law - an unbridgeable gap?". At least he posed this as a question and not as an assertion. His remarks are highly relevant to the topic I have been assigned. Like Professor Whalan, his area of knowledge and expertise has been very much in the field of environment law and biotechnology. Much of his lecture is addressed to DNA and the law, and the need for effective but not unduly intrusive or uneconomic regulation of those activities involving the manipulation of DNA that may pose an acceptable risk of injury to individuals and society.

In his opening remarks, Professor Fisher(13) suggests that science and law are "at the opposite ends of the fulcrum of human activity". Yet, although the two disciplines have different purposes, characteristics and methodologies they are, he claims, in several vital respects inextricably interrelated - each having an impact on the other.

Apparently unconcerned by quantum theory, Professor Fisher, a Professor of Law at the Victoria University of Wellington, New Zealand, asserts that science is totally self-contained. It is concerned with knowledge about substances with a view to describing their properties although susceptible to ambiguity and uncertainty of interpretation as any rule of law, the basic subject matter of the discipline has objective reality.

Law, on the other hand, is an artificial entity created by man for his convenience. According to Fisher, the law can prescribe how man may or may not interfere with processes by the use of science and technology. It is the formal mechanism through which society expresses its values and aspirations. It is an artificial rather than a natural phenomenon, "theoretically self contained and objectively identifiable". But with science, law shares the fact that difficulties arise because of the involvement of human beings. And at its core the law is prescriptive whilst the function of science is primarily descriptive. Fisher contends that it is this prescriptive function of the law which compliments the nature and role of descriptive science. He asserts that it is not only true that the legal system is conservative. It is inevitable. Science and technology provide the bases for new objectives, directions and values in society. The law provides the mechanisms for bringing these about or, he could have added, for frustrating their achievement.

The paramountcy of law as the reflection of the will of society may sometimes seem to be a fairy tale, like the old belief that the common law always existed and needed simply to be "found" in the bosum of the judges. It is romantic to think of the law as an accurate reflection of the wishes of the community. Often the laws administered by the courts bear little relevance to the conduct of society. We have only to think of laws which are honoured in the breach rather than the observance. Laws on gambling or the so called victimless crimes come immediately to mind.

In default of modernisation and reform of the law, it is not at all unusual for a statute passed in an earlier century to have an unexpected and wholly unintended impact on some practical development of science or technology today. Equally, the common law, found in judicial decisions handed down in different circumstances and different times even centuries before, can apply to fill the void of legislation. Thus the new problem of the revival and ushering into life of deformed and retarded neonates is determined by judges applying principles laid down in an earlier time before the advent of sophisticated technology which, in combination with heroic efforts, may save babies who in an earlier generation would have died in the course of nature. The law insists upon the best efforts to revive and sustain the impaired baby. However medical practice often varies from this standard and public opinion almost certainly supports that practice.(14)

Sometimes laws are developed specifically to address a new scientific achievement when that achievement is perceived as morally or socially unacceptable. The clearest recent example of this reaction is the legislation introduced in England to forbid surrogate birth arrangements(15), and the legislation recently introduced by Senator Harradine into the Australian Senate, designed to prohibit embryo experimentation. The advent of new scientific developments creates new potentials. If it is possible to "grow" a human embryo in vitro for the purpose of aiding infertile couples, should moral or positive law require the scientist to discard the embryos excess to use? The creation of multiple embryos seems to be an inevitable part of the chancy business of facilitating conception and the development of offspring genetically linked to the parents or one of them. But if human life begins at the moment of conception, as some claim, the destruction of the excess embryos is tantamount to murder - if not in law, then at least in moral principle.

On the other side of the coin are the scientific "utopians". They point to the potential benefits of these scientific developments. And not only for the couple immediately involved. For example, such commentators may claim that far from discarding excessive embryos, they should be put to good use, with experiments which may provide infinitely useful data because of the special value of human embryonic and foetal material. Why discard such material, they ask, where it could be so useful to humanity? A recent report in Britain urged that experiments with the human embryo should be permitted until the 14th day. That is just before the "primitive streak", which was thought to be the first relevant and undisputable signal of the change of the primitive embryo of cells to the beginnings of human form. The Warnock Report, by majority, recommended in favour of this permissible zone of scientific experimentation. But the opponents, including Senator Harradine, regard such experimentation with human life as totally unacceptable. For them there is no room for negotiation. And that is why they urge the introduction of legislation designed to stop such experimentation in its tracks. Rightly, they point out that in default of legislation such experiments will take place. Upon them, they claim, the law should not be silent. It should speak with a clear voice. It should speak for society. It should speak to the scientists and tell them that they should go no further for fear of the reduction of human life to the status of a thing - an object. The law defending human standard should speak out clearly. But what are those human standards?

Opponents of the legislation introduced in Britain to prohibit experiments with embryos point to numerous objections: the impossibility of regulating such matters on a local or even national basis only; the difficulties of enforcing such laws; the double standards and artificialities alleged to arise; and the unacceptability of the enforcement of the morality of *some* upon the future of *all*. Furthermore, some commentators have suggested that such laws made by politicians and lawyers fall heavily, not upon those making them, but upon those who are deprived of the advantages which scientific experimentation may bring: the infertile couples, the couples deprived of early detection and prevention of genetic defects in offspring, the sick and dying deprived of the benefits that flow from scientific experiments.

The difficulty of using the law as a means of regulating scientific advance is called to attention in many instances. In the issue of *Science* for 15 March, 1985, a leading article deals with the benefits and risks of vaccines. Provocatively, the writer asserts that whereas in earlier centuries, the hero was the explorer who blazed trails through hostile terrain to discover new worlds and wealth, nowadays he is the victim who blazes trails through hostile lower courts to establish a new precedent for law suits and wealth.(16) The point being made is the inevitable risk of some scientific procedure. It seems that, even today, the risk of vaccination is 1 in 100,000. Of course, for the one involved and the family concerned that risk is intolerable. In the courts it may sometimes sound in substantial damages. But the point made in the commentary in *Science* is the apparently cold hearted and clinical assertion that scientific advances, made in human, economic and social terms, benefit overwhelming numbers but at some cost. Is the law by direct regulation to prohibit such activity, upon some perception by the community of moral standards? Or is the law to stand back, countenancing the built in risks because of the greater benefits. The editorial in *Science* concludes:

> "At some point the judicial system will have to face the most inexonerable of all laws, the law of probability. Risks of disease and harmful side effects from vaccines is steadily being reduced, but they will never be absolutely zero. ...with drugs and vaccines some national compensation system in which medical

> costs, lost pay and so are calculated on appropriate statistical basis will need to be inacted ...[W]e may be able to introduce into Government the concept of a statistical morality as the foundation of a more rational approach toward all compensation situations. The next hero may be the statistics advocate who has the courage to say: "the healthy can afford to help the sick, but we do not live in a risk free world"."

THE MIGHTY MICRO

But if these problems of biotechnology are severe and controversial, the pervasiveness of the mighty micro presents difficulties just as acute for our society and its laws. I learnt something of these issues in the time I spent as Chairman of an OECD expert group studying the phenomenon of transborder data flows and the protection of privacy.(17) The challenges to the law by reason of the international developments of data traffic are enormous. They derive from the universal, pervasive and international character of this new technology. The questions for the lawyer are many. They include the impact of this new technology on:

- the vulnerability of society - when vital data may be stored in a few tapes, vulnerable to accident, terrorism, natural disasters and criminal conduct.
- the criminal law, which has developed as a phenomenon of local sovereignty, and is normally strictly confined to its own jurisdiction, but may have to adapt because of the potential of informatics. A message may originate in country A, be transmitted through countries B, C and D, be switched in country E and ultimately do harm in countries X, Y and Z. Whose criminal law is to apply to such a case? Whose police forces are to investigate the anti-social conduct? Whose courts are to have jurisdiction? Whose laws of evidence and procedural laws are to govern such cases?
- the laws of copyright and intellectual property generally, the laws of contract, the rules by which conflicts of laws are reconciled: all of these will have to adapt to the international character of informatics. In a real sense the new technology will facilitate and demand the development of international law.

In the field of privacy, the OECD guidelines, recently acceded to by Australia, lay down certain rules which should be observed in the movement of personal data. The Australian Law Reform Commission conducted a major review of this subject. It has recommended(18) the introduction of Federal legislation for the better protection of privacy in Australia, including in computerised personal data systems. It is understood that legislation based upon the Commission's report may be expected to be introduced in the Parliament some time in 1985. Of course, there are those who contend that it is impossible to protect privacy, given the pervasive and overwhelming character of the new technology and the speed with which it is being adapted, developed and accepted. But those features of the technology seemed to the Law Reform Commission (as they seem to me) to be argument for the development of flexible laws not reasons for abandoning altogether the endeavour to state basic principles and, where necessary, to enforce them.

One does not have to peer into the distant recesses of the 21st century to see a practical case concerning the tension between new technology and old legal principles. One can see it in a contemporary instance which illustrates my point. I refer to the suggestion that Australia should adopt a system of universal identity cards: doubtless computerised, infallible and assigned to all of us at birth. This well meaning proposal has been advanced by a number of unlikely advocates, the most vocal of whom has been a spokesman for the Australian Taxpayer's Association. In the kindly and admirable concern to help the administration to beat tax avoidance, it has been suggested that all of us should have this universal identifier. By this means the tax cheat (and doubtless a few other anti-social persons) will be caught. No one will be able, so it is claimed, to take cash on the side. Somehow the problem of tax avoidance will be dealt a mortal blow, in a single stroke, by requiring everyone to have a foolproof universal identifier.

Public opinion polls conducted by radio stations apparently disclosed strong support for this idea. One Sydney poll reported to me showed an 87% vote in favour of the proposal for Australia-wide, universal ID cards. Mind you the sample was small (450) and not scientifically gathered. Furthermore, in an article in the *Daily Telegraph*, which has been conducting a single minded campaign in favour of the ID cards, Federal political writer, Ian Collier said this:

> "What is remarkable is not so much the support but the lack of opposition to the proposal. The Labor Party left wing has remained silent. The only opposition so far came in the speech by Justice Michael Kirby ... Justice Kirby said the ID cards smacked of a Big Brother ideology. He said the cards would provide the authorities with an excellent method to keep tabs on us all. But many people who once would have supported Justice Kirby's remarks now believe they are irrelevant."(19)

This essay comes under the heading "Time May be Ripe for ID Cards". Significantly, perhaps, the print media, including the *Daily Telegraph* did not see fit to reproduce my remarks, although they were available to them and did appear in some of the electronic media. It never ceases to surprise me that the Australian media - suggested (and frequently self-proclaimed) defenders of our freedoms - are often happy to support their diminution, save where they perceive an element of self-interest. If the prerogatives of the media are involved, they tend to be loud in their proclamations of freedom. For many media writers in Australia, the *only* freedom is freedom of the press.

There are, of course, other elements of freedom. They will be surely tested in the century ahead by the developments of science and technology. ID cards are simply a relevant, current, vivid illustration.

The arguments for such cards are arguments of efficiency. Certainly, we would catch a few tax cheats. Doubtless we would catch a few criminals as well. What then are the arguments against? This is

difficult for lawyers to explain. And yet it is important that the explanation go forth so that it can be considered in any decisions that are made.

The following is my short list of reasons for opposing this apparently seductive use of the new information technology:

1. I am not convinced that it would be especially effective. I recently heard Mr. John Howard, Deputy Leader of the Opposition, say that information available to him from the United States suggested that the cost of a universal identifier outweighed its potential benefit. This seems sensible to me, given that people who engage in tax avoidance will, in any case, frequently transact their activities in cash or kind, avoiding the record that would be picked up by an ID card.
2. But even assuming effectiveness, we do not make all decisions touching our liberty by reference to efficiency. Of course it would be more efficient to tap telephones without limit. Of course the criminal law would be more effective if we terrorised our people with barbaric punishments, detention without trial, spies in every street and the panoply of the police state. We do not do so. Significantly opinion polls show that about 75% of the population supports hanging. Yet I have not heard any responsible politician in the Federal Parliament urging the restoration of that form of punishment. Sometimes majorities can be wrong. Sometimes, out of ignorance, they can be too ready to throw away ancient freedoms or hard-won liberties and reforms. And sometimes they embrace ideas which they later reject with indignation, when the full enormity is brought home to them.
3. Efficiency is not all. This was vividly illustrated to me when I attended a conference on informatics held in Paris in 1980. When the spokesmen of the new information technology were urging the need for a computerised identity card in France a man, in alarm, rose from the audience. In vivid French, he reminded his listeners of the fate of the Jews in wartime Europe. Why was it, he asked, that 90% of the Jews of the Netherlands perished? Why was it that 60% of the Jews of France survived? At least part of the explanation he attributed to the fact that the Netherlands, with its efficiency, had produced an identity card which could not be forged. The French, though they had an identity card, produced it negligently. It could be forged. Many Jews and many heroes of the Resistance, Allied soldiers and others survived to see the re-establishment of a freedom in France because of this inefficiency in the identity card. Efficiency is not the only social virtue. Calls by, of all people, the Taxpayer's Association for efficiency in tax collection must be weighted against the respect for our civil liberties. It is forty years since the end of the Second World War. But for some people that is a time for speeches and military parades - not as I would assert, a time to reflect upon what all the dreadful sacrifice was about.
4. This brings me to the principal argument. Despite the advance of informatics (under which such universal identifiers may not even be necessary) there is always a risk that the assignment of an identity number and card to every individual will attract a change in the relationship between the individaul and the State. There is a risk that if people have identity cards they will next be required to carry them. If they carry them, they will next be required to produce them. If they have to produce them, they will have to produce them to the officials of the State. It will start with security. It will move to regular police. It will expand to customs officers. It will end up with many in vast array of officialdom: empowered (all in the name of efficiency and the due administration of the law) to stop citizens in their daily lives. To intervene in their lives. To intrude into their private zone - and this without the present safeguards which prohibits such intrusions, generally, unless the persons so authorized have reasonable cause to suspect the commission of an offence. If you say this is alarmist talk, consider telephonic interceptions. Once it was unthinkable that the privacy of the Royal Mail and the telephone should be invaded. And then came exceptions for national security. Next came cases of drug surveillance. Then there were exceptions for special circumstances. Last week another exception for cases of life and death reached Parliament. Who knows where this diminution in the confidence in telecommunications will end up? If you believe some editors of our newspapers, following the so-called *Age* tapes, it is very naughty to tap telephones; but it really does not matter very much - unless a journalist's phone is tapped. Privacy, it seems, is a value that is being eroded, but we should only be selectively concerned.
5. And fifthly there is the point of personal autonomy. In our kind of society individual human beings are people - not numbers. They are not to be reduced to a mere electronic number on a national and universal identifier. They may be in various systems by their own choice or by reason of particular laws. But there is, as yet, no universal system. Provide a universal identifier and you provide a universal data base which will collect increasing data about us all, producing the ultimate triumph of the authoritarian state which even Orwell and Kafka would not have dreamed possible. In inefficiency sometimes lies a defence to freedom. In a sense, that is one of the reasons for the Federal form of government, if you think about it. A few days ago I was reading an article by Gary T. Marx in *Dissent*. Marx drew attention to the hit song by The Police titled "Every Breath You Take".(20) It was, he said, a warning of the new surveillance. Listen to the words and to Marx' counter-point:

   "Every breath you take, [breath analyser]
   Every move you make, [motion detector]
   Every bond you break [polygraph-lie detector]
   Every step you take [electronic anklet]
   Every single day [continuous monitoring]
   Every word you say [bugs, wire taps, mikes]
   Every night you stay [light amplifier]
   Every vow you break [voice stress analysis]
   Every smile you fake [brain wave analysis]
   Every claim you stake [computer matching]
   I'll be watching you [video surveillance]".

It may be that there is little that we can do about the new surveillance. Some of it is thoroughly beneficial and highly desirable. Much of it takes place with the acquiescence or even the active participation and encouragement of fellow citizens. The zone of privacy diminishes every day. But that is not to say that we should simply wring our hands and abandon the proper role of the law and of our institutions in defending our hard won freedom and individuality.

This is not a melancholy lament for the return of the good old days. The advent of informatics brings in its train many consequences that will not go away. But lawyers have a part to play in reminding society that even in the age of informatics - even in the next century of science and technology - important values must be preserved. The role of the State must be contained. Indeed, it must be contained with special vigor, because the science and technology that is about us, arms the State and its agencies with seemingly limitless powers:

- to change or facilitate the change of our very nature and species.
- to intrude into, monitor and control our every day life even to its private moments and
- ultimately, to destroy us all, unless, as human beings, we are vigilant and assertive.

Let this great power be controlled.

WHAT IS TO BE DONE

The moral of this tale is not that we should favour foetal experiments. Nor is that we should mount the barricades over identity cards. Still less is it that we should join the peace marchers and protest against nuclear weapons. Those are matters that must be left to politicians and to individuals.

But the lessons which I would derive from the efforts of our society, since the Second World War, to adapt to the age of science and technology are these:

1. First, we must make greater efforts to improve our education system so that increasing numbers, including increasing numbers of women who tend to be neglected in science education, will be trained in the basics of science. It is no longer acceptable, for the educated person to be ignorant of Quantum Theory or oblivious to the basics of DNA. Bronowski warned that to ignore science and technology today was to walk, with eyes open, towards slavery. I agree with that warning. We should take heed of it in our educational system so that we produce new generations of Australians who are not illiterate in the basic moving force of the world they live in. (21)
2. Secondly, we must encourage greater communication on the part of scientists with the community whom they so profoundly affect. That is why the moves of this Faculty to promote community awareness and to institutionalise it, are to be welcomed. The dangers of failing to do so are a mindless advance of science, indifferent to the needs and wishes of the people or, equally unacceptable, the mindless interference by organised society to stop scientific advance out of fear of where it may lead us, rooted in ignorance.
3. Thirdly, consideration must be given to our institutions. How are our laws in an age of mature science and technology to be developed and adapted? How are the judges who make the common law to be sensitised to the values of scientific research and the needs to reconsider old precedents, adapting them to the potential benefits of technological advances? The opportunity costs that are involved in research forbidden by law may be enormous. The blunt weapon of legislation or curial pronouncements, may unacceptably intrude upon the onward advance of scientific progress.
4. But fourthly, society has the right, ultimately, to pronounce the circumstances in which science and technology shall take place. They exist to serve us. We are, and should remain, the masters, not the abject slaves of the technological engine. Of course, there are limitations. Science is a world wide phenomenon. It is difficult to stem the tide of its onward rush in one country, when it remains unregulated (and even possibly encouraged) in another. But the fact that others do not defend themselves is not a reason for surrender and capitulation. (22) Our legislatures have their own responsibilities to define the future shape and nature of our country. It is to be hoped they will do so, conscious themselves of what is at stake and reacting to a democracy that is itself literate in the field of science and technology.
5. Finally, our legislatures should, in my view, neither gnash their teeth and rent their clothes (as did the prophets of old) nor abandon their responsibilities to maintain the fundamentals of our society. They are our guardians. Our institutions will need to adapt. Above all they will need to develop laws affecting science with the full participation of scientists and of the community.

And that is the only optimistic not upon which I can finish this review. The involvement of scientists in law development has now begun. In the field of human tissue transplants, the Law Reform Commission involved many of the finest medical scientists throughout the country, together with theologians, philosophers and many others. The late Professor Sir John Loewenthal of this University was a leading consultant to the Commission. Similarly in the Law Reform Commission's work on privacy protection the scientists, the computerists, the police intelligence experts and others were all closely involved. Professor John Bennett of this University took a leading and constructive role in the work of the Law Reform Commission on these new laws. That is the way it will be in the future. Not lawyer, lost in his classics, disdaining the world of the scientist. Not the scientist, immersed in his equations and formulae ignoring the lawyer. But each communicating with the other in a language which the other understands.

When in a 100 years this Faculty meets to celebrate its second century of achievement, I do not doubt that the dialogue of science and the law - and the relationship of science to society - will be at the top of the agenda. I hope that, in the meantime, we have been able preserve our existence, our species and our precious human liberties.

FOOTNOTES

1. Sydney University, *The Gazette*, vol. 4, No. 11, March, 1985, 8.

2. D.F. Branagan and G. Holland, *Ever Reaping Something New*, History of the Science Faculty, University of Sydney, 1985.

3. J. Gribbin, *In Search of Schrodinger's Cat*, Wildwood House, London, 1984.

4. N. Bohr in Gribbin, 5.

5. B.O. Jones, Opening Address, 1985, The National Science Summer School, mimeo, Canberra CAE, 7 January, 1985, 1.

6. ibid, 2.

7. id, 3.

8. id, 4.

9. B.O. Jones, Second Whitlan Lecture, N.S.W. Fabian Society, November, 1984, mimeo.

10. (1982) 56 *Australian Law Journal*, 658.

11. D.E. Fisher, "The Use of DNA and the Law in Australia", (1982) 56 *Australian Law Journal*, 6.

12. (1982) 56 *Australian Law Journal*, 660.

13. D.E. Fisher, "Science & Law - An unbridgeable gap?" *NZ Science Review*, vol. 41, 1984, no. 5, 82.

14. M.D. Kirby, "Deformed and Retarded Neonates: Medical & Legal Responses", unpublished paper for the World Congress on Law and Medicine, New Delhi, India, 23 February, 1985. This paper reviews the recent common law decisions in a number of countries.

15. See M.D. Kirby, "Surrogacy, 85 - From Hagar to Baby Cotton", in *Australian & New Zealand Journal of Obstetrics & Gynaecology* forthcoming (Arthur Wilson Memorial Oration, 1985).

16. D.E. Koshland, "Benefits, Risks, Vaccines and the Courts", *Science*, vol. 227, no. 4692, 15 March, 1985.

17. Organisation for Economic Cooperation & Development, *Guidelines on the Protection of Privacy and Transborder Flows of Personal Data*, OECD, Paris, 1981.

18. The Law Reform Commission (Cth) *Privacy* (ALRC 22), AGPS 1984.

19. I. Collier, "Time May be Right for ID Cards", *Daily Telegraph*, 9 May 1985, 11.

20. Gary T. Marx, "I'll Be Watching You", in *Dissent*, Winter edition, 1985, 26.

21. See Report N.S.W. Education Department, cited "Science is No Use", reported, *Sunday Mail*, Brisbane, 5 May, 1985.

22. P. Sieghart, "A Corporate Conscience for the Scientific Community?" in *Nature*, vol. 239, 1 September, 1972.

(Manuscript received 7.7.1985)

Title: Synthesis of GABA Analogues for Neurochemical Studies

Author: Joyce Fong

Abstract:

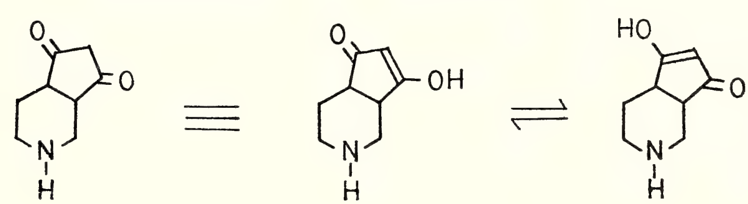
In my thesis, the synthesis is described of fifteen new GABA analogues. They are from three different series, namely β-diketone derivatives, unsaturated nipecotic acid analogues, and derivatives in the cyclopentane series. The GABA-mimetic activity of these compounds has been assayed by measuring the inhibition of [^{3}H]GABA binding, the potentiation of [^{3}H]diazepam binding, the inhibition of [^{3}H]GABA uptake, the inhibition of GABA-transaminase activity, and the substrate-activity of GABA-transaminase in rat brain membranes, as well as the transient contraction of the guinea-pig ileum.

The β-diketone derivatives, octahydro-1*H*-2-pyrindine-5,7-dione (1) and its diazo and 6-bromo derivatives, showed negligible or very weak activity as GABA agonists with respect to inhibition of [^{3}H]GABA binding, uptake and transamination.

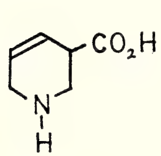
1,2,3,6-Tetrahydropyridine-3-carboxylic acid (neoguvacine) (2), the β,γ-unsaturated derivative of the GABA uptake inhibitor nipecotic acid, has been synthesized via the dilithium salt of the *N*-t-butyloxycarbonyl-protected intermediate. Substitution of the intermediate with alkylating agents or an aldehyde gave predominantly α-alkylated products as expected, but chlorination with *N*-chlorosuccinimide provided a route to the γ-substituted amino acids. Derivatives with bulky α- and γ-substituents are less active at inhibiting GABA uptake. However, a smaller α-methyl group and the more polar γ-hydroxyl group are better accommodated at the active site and these derivatives retain a relatively high potency. Structure-activity studies in this series included non-cyclic analogues of neoguvacine such as the β,γ-unsaturated *cis*, *trans* olefinic and acetylenic derivatives of 5-amino-pentanoic acid. These emphasized the preference of the active site for GABA uptake for folded conformations represented by *cis*-5-aminopent-3-enoic acid and neoguvacine.

In the cyclopentane series, *trans*-4-aminocyclopent-2-ene carboxylic acid (3) was synthesized via the thermal isomerization of the *cis* *N*-phthaloyl acid derivative and was resolved via crystalline diastereomeric 2,3-isopropylidene-*D*-ribonolactone and *D*-pantolactone esters. Both optical isomers of the β,γ-unsaturated and the saturated amino acid could be obtained from these esters. Although the unsaturated (+)-*trans*-(1*R*,4*R*)-4-aminocyclopent-2-ene-1-carboxylic acid was less active as a GABA agonist than its saturated counterpart, its potential to be converted into tritiated radioligand was demonstrated by its reduction to the saturated amino acid without isomerization. The conjugated (+)-(4*S*)-4-aminocyclopent-1-ene-1-carboxylic acid was found to be particularly selective and potent as a GABA receptor agonist while its (-)-(4*R*) isomer was conversely selective and potent at inhibiting [^{3}H]GABA uptake into rat brain tissue.

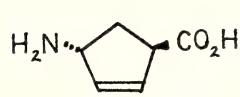
(1)



(2)



(3)



Type of Thesis: Thesis for the Degree of Doctor of Philosophy at The University of Sydney, N.S.W., 2006. (Department of Pharmacology)

Present Address of Author: 89 Ha Heung Road,
2nd/F Block B,
Yick Fu Building,
Tokwawan, Hong Kong.

Title: The Population Structure and Reproductive Biology of *Ceratonereis limnetica* (Polychaeta : Nereididae) at Lower Portland, Hawkesbury River, N.S.W.

Author: C. Glasby

Abstract:

The reproduction and population structure of *Ceratonereis limnetica* occurring at Lower Portland, a near fresh water site, was investigated over a 19 month period. The species had a relatively short life span of 9 - 15 months. Animals spawned once, in spring and early summer, in the benthos. There was no heteronereid metamorphosis, epitoky presumably having been suppressed. Embryonic development was non-pelagic with larval stages brooded within the parental tube. Very few juveniles were detected at Lower Portland, but were abundant further downstream. It is speculated that the reproductive success of this species at Lower Portland was poor over the study period and that individuals were recruited from further downstream. The reproduction and life histories of other nereidids, especially fresh water species, were reviewed and compared with *Ceratonereis limnetica*.

Type of Thesis: Thesis for the Degree of Master of Science at the University of Sydney, N.S.W., 2006, Australia.

Present Address of Author: The Division of Marine Invertebrates,
The Australian Museum,
College Street, Sydney, N.S.W., 2000, Australia.

MEETINGS

Nine general monthly meetings and the annual meeting were held during the year. The average attendance was 28 members and visitors (range 16 to 36). Summaries of the addresses were published in the Newsletter and abstracts of the proceedings are appended to this report.

The Liversidge Research Lecture for 1984 was delivered by Associate Professor D.H. Napper on "The Birth and Growth of Latex Particles", on Wednesday, 17th October, 1984, at the University of New South Wales.

The Pollock Memorial Lecture for 1984 was held on Wednesday, 28th November, 1984, in the Stephen Roberts Lecture Theatre at the University of Sydney. Dr. R.S. Pease, FRS, Director of the U.K. Atomic Energy Authority spoke on "The Electromagnetic Pinch: from Pollock to the Joint European Torus".

The first of a series of seminars on "Scientific Sydney" was held on Saturday, 24th November, 1984, in conjunction with the Royal Australian Historical Society, at History House, Macquarie Street. The title was "Artisans and Managers: Exploring Technical Education in N.S.W., 1884 - 1984".

The Council held 11 meetings during the year at Macquarie University.

ANNUAL DINNER

The Annual Dinner was held in Ballroom A of the Hilton Hotel on the evening of Tuesday, 19th March, 1985. 85 members and guests were present. The Guest of Honour was His Excellency Air Marshal Sir James Rowland, Governor of New South Wales. An address was given by Emeritus Professor R. Hanbury Brown, FRS, entitled "Why Bother About Science?" The vote of thanks was moved by Professor T.W. Cole, Vice-President.

PUBLICATIONS

The Journal and Proceedings Volume 117, Parts 1 to 4, were published during the year, incorporating 12 papers and the 1983-84 Annual Report. Council is again grateful to the voluntary referees who assessed papers offered for publication.

Every effort is made to ensure the wide availability of the Journal in Australia and overseas. Details of the distribution were set out in the 1983-84 Annual Report (*J. & P.*, p.71).

The Journal is abstracted routinely in Chemical Abstracts, Physical Abstracts, Geological Abstracts, Geoabstracts, Mathematical Reviews, and when relevant, in Biological Abstracts. Copies are sent air mail to ensure that abstract entries are achieved as speedily as possible.

NEWSLETTER

Nine issues were published under the editorship of Associate Professor J.H. Loxton. Council is most grateful to the authors of short articles, which are much appreciated by members.

MEMBERSHIP

The membership of the Society at 31st March, 1985, was:-

Honorary Members	12
Company Members	1
Life Members	30
Ordinary Members	291
Absentee Members	12
Associate Members	30
Total	376

AWARDS

The following awards for 1984 were made:

Clarke Memorial Medal:	Associate Professor Michael Archer
Edgeworth David Medal:	Dr. Alan James Husband
James Cook Medal:	Professor Ronald Lawrie Huckstep
Society Medal:	Dr. Robert S. Vagg
Ollé Prize (shared):	Mr. R.A.L. Osborne, and Terence J. Goodwin, Robert S. Vagg and Peter A. Williams.

SUMMER SCHOOL

A most successful Summer School on the Physics of "Light" was held from 14th to 18th January, 1985, at Sydney University. It was attended by 156 school students about to enter Year 12. They came from 48 State and private schools. The Summer School was organised on the Society's behalf by Associate Professor Denis Winch. The Society's appreciation is extended to Professor Winch and to the twenty-two lecturers whose talks, many accompanied by excellent demonstrations, made the Summer School one of the most successful the Society has held. A visit was paid to the National Measurement Laboratory, CSIRO Division of Applied Physics, Lindfield.

LIBRARY

The new arrangements described in the last Annual Report, whereby most of the Library was transferred to the Dixson Library, University of New England, have proved a very satisfactory solution to a difficult situation. The Society's exchange partners now send their publications directly to the Dixson Library and the Society dispatches its Journal to those partners as heretobefore. Mr. Karl Schmude, University Librarian of the University of New England, supervised the transfer of the Society's library and recommencement of the inter-library loan services. He reports that book-plating and gold-stamping with the Society's crest is actively proceeding and the shelving of the collection in

its final location is progressing satisfactorily as labour allows.

OFFICE

Macquarie University Council kindly offered the use of half of Convocation House, Herring Road, North Ryde, to the Society for use as an office, as was reported in the last Annual Report. However, the books stored in the house were not removed until late in 1984. Subsequent painting and installation of additional lighting by the University were not completed until early in 1985. Postal arrangements and installation of a telephone service have further delayed setting up of our office. As a result the temporary arrangement whereby the Society's business was conducted from Dr. Day's house continued throughout the year of report. Any inconvenience to members resulting from this arrangement is regretted. The Day family's toleration of the unexpected long continuation of the Society's dependence on their hospitality is deeply appreciated. It is confidently expected the office will be established at North Ryde in the next two months. Meetings of Council have been held at Convocation House since the beginning of 1985.

FUTURE DIRECTIONS OF THE SOCIETY

As previously reported, Council in February, 1984, established a Working Party to examine the Society's operations and recommend future directions which the Society could realistically pursue. Under Dr. R.S. Vagg's chairmanship the working party met on February 24, April 18, May 16 and August 13, 1984, at Macquarie University, and on October 3, 1984, presented a lengthy report to the Council outlining its conclusions and recommendations. A digest of the report was published in the December 1984 Newsletter and members' comments were requested. A number of members wrote constructive submissions. The Council is in the process of considering the report together with the submissions from members prior to acting on the recommendations.

SCIENCE HOUSE PTY. LTD.

The liquidation of the Company has proven far longer drawn out than we expected. The liquidators plan to complete the liquidation early in April, 1985. They expect to be able to return to our Society about $20,000 of the $416,991 it originally invested.

FINANCE

The Society's financial year extends from January to December. In 1984 a surplus of income over expenditure of $4819 was achieved. This resulted from (a) a number of operating economies which were achieved, no one of which was large but the total was significant, (b) generous donations by two members to general funds, (c) donations towards the cost of the Journal, and (d) small surpluses achieved by the Summer School on "Sound" and the first Scientific Sydndy Seminar.

After deficits in the last two years totalling over $3100 it was essential to achieve a substantial surplus in order to correct the downward trend in the Society's finances.

No new investments were possible during the year, but every effort will be made to build as substantial a (secure) investment base as possible in order to keep the Society financially healthy.

Mr. A.M. Puttock's professional assistance in the maintenance of the Accounts is again acknowledged with gratitude.

NEW ENGLAND BRANCH

The New England Branch held several very well attended meetings during 1984. The year started with three very popular talks on nuclear power (listed in 1983/84 report). Two further meetings were held in 1984:

22 October	Professor F.W. Robertson, Professor of Genetics, University of Aberdeen: "Genes, Lipo-proteins and Coronary Disease".
15 October	Associate Professor G.A. Woolsey of the University of New England: "Light and Colour in the Sky"

ACKNOWLEDGEMENTS

In conclusion, the Council wishes to express its gratitude to the many people who assisted the Society in the past year, including: the speakers at the monthly meetings; the authors of articles for the Newsletter; Miss Helena Basden for valuable assistance in publication of the Journal and Proceedings; Associate Professor D.E. Winch and Mrs. E. Winch who organised the Summer School; Professor S.C. Haydon and Professor R.L. Stanton in the New England Branch; Mr. K. Schmude for continuing processing the Society's library transferred to the Dixson Library and operating the inter-library lending from it; and Mrs. Grace Proctor and Mrs. Judith Day for their assistance in the library and office.

THE ROYAL SOCIETY OF NEW SOUTH WALES

AUDITORS REPORT

In our opinion:

(a) the attached accounts, set out on pages 2 to 12 which have been prepared under the historical costs convention, are properly drawn up in accordance with the Rules of the Society and so as to give a true and fair view of the state of affairs of the Society at 31st December 1984 and of the results of the Society for the year ended on that date; and

(b) the accounting records and other records, and the registers required by the Rules to be kept by the Society have been properly kept in accordance with the provisions of those Rules.

WYLIE & PUTTOCK
Chartered Accountants.

By ALAN M. PUTTOCK
Registered under the Public Accountants Registration Act, 1945 as amended.

BALANCE SHEET as at 31/12/84

	RESERVES			
7310.57	Library Reserve (note 2(a))			7310.57
5017.05	LIBRARY FUND (note 2(c))			5405.97
19875.34	TRUST FUNDS (note 3)			21454.25
73322.93	ACCUMULATED FUNDS			78496.67
105525.89	TOTAL RESERVES AND FUNDS			112667.46
	Represented by:			
	CURRENT ASSETS			
336.56	Petty Cash Imprest		117.30	
2405.60	Debtors for Subscriptions	2849.00		
2405.60	Less Provision For Doubtful Debts	2849.00		
0.00			0.00	
7044.59	Other Debtors & Prepayments		3933.37	
9583.43	Interest Bearing Deposit		13911.11	
4508.53	Cash at Bank		7259.91	
21473.11				25221.69

BALANCE SHEET as at 31/12/84

	Less: CURRENT LIABILITIES		
13475.72	Sundry Creditors & Accruals	9967.10	
	Life Members		
27.37	Subscriptions - Current Portion	27.37	
43.59	Membership Subscriptions Paid in Advance	77.88	
1042.00	Subcriptions to Journal Paid in Advance	979.71	
14588.68			11052.06
6884.43	NET CURRENT ASSETS		14169.63
	Add: FIXED ASSETS		
1568.66	Furniture, Office Equipment, etc.- at cost less Depreciation	1397.66	
13600.00	Library - 1936 Valuation (note 4)	13600.00	
10.00	Pictures - at cost less Depreciation	10.00	
15178.66			15007.66
22063.09			29177.29
	Add: INVESTMENTS		
8700.00	Commonwealth Bonds & Inscribed Stock	8700.00	
60000.00	Loans on Mortgage	60000.00	
15000.00	Interest Bearing Deposits	15000.00	
83700.00			83700.00
105763.09			112877.29
	Less: NON-CURRENT LIABILITIES		
237.20	Life Members Subscriptions - Non-Current Portion		209.83
105525.89	NET ASSETS		112667.46

R.S. BHATHAL President

A.A. DAY Honorary Treasurer

STATEMENT OF ACCUMULATED FUNDS
For the Year Ended 31st December 1984

(2504.57)	OPERATING SURPLUS for the year		4819.24
416991.00	EXTRA-ORDINARY ITEM		0.00
(419495.57)	SURPLUS & EXTRA-ORDINARY ITEM		4819.24
382.67	Donations & Interest to Library Fund		388.92
430.00	Transfer from Library Fund		0.00
416991.00	Transfer from Resumption Reserve		0.00
0.00	Recouped on Disposal Painting		354.50
78520.50	Accumulated Funds - Beginning of Year		73322.93
76828.60	AVAILABLE FOR APPROPRIATION		78885.59
3505.67	Transfer to Library Fund	388.92	
3505.67			388.92
73322.93	ACCUMULATED FUNDS Current Year		78496.67

NOTES TO AND FORMING PART OF THE ACCOUNTS
For the Year Ended 31st December 1984

1. SUMMARY OF SIGNIFICANT ACCOUNTING POLICIES

Set out hereunder are the significant accounting policies adopted by the Society in the preparation of its accounts for the year ended 31st December, 1984. Unless otherwise stated, such accounting policies were also adopted in the preceding year

(a) Basis of Accounting

The accounts have been prepared on the basis of historical costs

(b) Depreciation

Depreciation is calculated on a written down value basis so as to allow for anticipated repair costs in later years.
The principal annual rates in use are:

Furniture	7.50%
Office Equipment	15.00%

2. MOVEMENTS IN PROVISIONS AND RESERVES

(a) Library Reserve

7310.57	Balance at 1st January		7310.57
7310.57			7310.57
0.00	Transfer to Accumulated Funds	0.00	
0.00			0.00
7310.57	Balance at 31st December		7310.57

2. MOVEMENTS IN PROVISIONS AND RESERVES

(b) Resumption Reserve

416991.00	Balance at 1st January		0.00
416991.00			0.00
416991.00	Transfer to Accumulated Funds	0.00	
416991.00			0.00
0.00	Balance at 31st December		0.00
0.00	Represented by: Shares in Associated Corporation		0.00
0.00			0.00

(c) Library Fund

3039.78	Balance at 1st January		5017.05
382.67	Add Donations and bank interest		388.92
3123.00	Proceeds disposal library fittings		0.00
6545.45			5405.97
430.00	Loss on disposal library fittings	0.00	
1098.40	Costs re gift to University of New England	0.00	
1528.40			0.00
5017.05	Balance at 31st December		5405.97

3. TRUST FUNDS

	Clarke Memorial Fund - Capital			
5000.00	Balance at 1st January	5000.00		
5000.00	Balance at 31st December		5000.00	
	Clarke Memorial Fund - Revenue			
794.06	Revenue Income for Period	894.00		
809.22	Less Expenditure for Period	110.19		
(15.22)		783.81		
349.59	Balance at 1st January	334.37		
334.37	Balance at 31st December		1118.18	
5334.37				6118.18
	Walter Burfitt Prize Fund - Capital			
3000.00	Balance at 1st January	3000.00		
3000.00	Balance at 31st December		3000.00	
	Walter Burfitt Prize Fund - Revenue			
477.00	Revenue Income for Period	536.00		
65.03	Less Expenditure for Period	586.45		
411.97		(50.45)		
1624.27	Balance at 1st January	2036.24		
2036.24	Balance at 31st December		1985.79	
5036.24				4985.79
	Liversidge Bequest Fund - Capital			
3000.00	Balance at 1st January	3000.00		
3000.00	Balance at 31st December		3000.00	
	Liversidge Bequest Fund - Revenue			
477.00	Revenue Income for Period	536.00		
0.00	Less Expenditure for Period	39.45		
477.00		496.55		
100.54	Balance at 1st January	577.54		
577.54	Balance at 31st December		1074.09	
3577.54				4074.09
	Olle Bequest Fund - Capital			
4000.00	Balance at 1st January	4000.00		
4000.00	Balance at 31st December		4000.00	
	Olle Bequest Fund - Revenue			
635.00	Revenue Income for Period	715.00		
0.00	Less Expenditure for Period	366.00		
635.00		349.00		
1292.19	Balance at 1st January	1927.19		
1927.19	Balance at 31st December		2276.19	
5927.19				6276.19
19875.34	Total Trust Funds			21454.25

4. LIBRARY

During the 1983 year the Society gifted the serials collection component of the library to the University of New England. The Society has retained that section of the library which is of historical significance. At the 31st December, 1984 a current valuation of the library had not yet been obtained.

INCOME AND EXPENDITURE ACCOUNT
For the Year Ended 31st December 1984

	INCOME			
7920.00	Membership Subscriptions - Ordinary			7540.80
19.37	Membership Subscriptions - Life Members			27.37
44.00	Application Fees			42.80
----------				----------
7983.37				7610.97
3631.12	Subscriptions and Contributions to Journal Publication Costs			4825.86
----------				----------
11614.49	Total Membership and Journal Income			12436.83
13209.21	Interest Received			13132.79
0.00	Sale of Reprints			194.30
453.00	Sale of Back Numbers			0.00
268.00	Sale of Other Publications			8.00
30.10	Donations - General			1200.00
150.54	Summer School Surplus			26.86
124.00	Other Income			411.23
----------				----------
25849.34				27410.01
	Less:EXPENSES			
1025.00	Accountancy Fees		1080.00	
322.87	Annual Dinner		63.65	
513.00	Audit Fees		540.00	
17.35	Bank Charges & Government Duties		27.34	
150.00	Branches of the Society		150.00	
176.00	Cleaning		0.00	
429.00	Depreciation		171.00	
503.66	Electric Light & Power		0.00	
493.81	Entertainment Expenses		146.07	
255.73	Insurance		203.22	
7130.00	Journal Publication Costs	6655.00		
1784.97	Wrapping & Postage	1622.20		
----------		----------		
8914.97			8277.20	
140.00	Library Accession, Cataloguing & Reader Services		0.00	
2657.29	Library and Office Relocation		1851.25	
57.67	Miscellaneous Expenses		76.50	
706.50	Monthly Meeting Expenses		0.00	
1328.22	Newsletter Printing & Distribution		1660.45	
290.39	Postage		280.70	
432.10	Printing & Stationery - General		740.43	
1000.48	Provision for Doubtful Debts		1373.40	
2781.24	Rent		927.08	
112.00	Repairs & Maintenance		117.60	
81.70	Reprints - Loss on Sale		0.00	
5625.17	Salaries		4824.12	
339.76	Telephone		80.76	
----------			----------	
28353.91				22590.77
----------				----------
(2504.57)	SURPLUS for the year			4819.24
==========	=======================			==========

STATEMENT OF SOURCE AND APPLICATION OF FUNDS
For the Year Ended 31st December 1984

	SOURCE OF FUNDS		
0.00	Operating surplus for the year	4819.24	
	Add:		
	Items not involving the outlay of funds in the current period:		
0.00	Depreciation of fixed assets	171.00	
0.00	Provision for doubtful debts	443.40	
----------		----------	
0.00	Funds derived from operations		5433.64
382.67	Donations and interest to library fund		388.92
2383.00	Trust fund income		2681.00
3123.00	Proceeds disposal library fittings		0.00
3004.61	Loan Science House Pty Limited repaid		0.00
120.00	Life Membership Subscriptions		0.00
0.00	Proceeds disposal painting		354.50
----------			----------
9013.28			8858.06
==========			==========
	APPLICATION OF FUNDS		
2504.57	Operating deficit for the year	0.00	
	Less:		
	Items not involving the outlay of funds in the current period:		
429.00	Depreciation of fixed assets	0.00	
1000.48	Provision for doubtful debts	0.00	
430.00	Loss on disposal library fittings	0.00	
----------		----------	
645.09	Funds applied to operations		0.00
27.37	Reclassification of life members subscriptions in advance		27.37
874.25	Trust fund expenses		1102.09
6368.17	Increase in working funds		7728.60
1098.40	Costs re gift library to University New England		0.00
----------			----------
9013.28			8858.06
==========			==========

CITATIONS

THE CLARKE MEDAL

The Society's Clarke Memorial Medal for 1984 is awarded to Associate Professor Michael Archer, of the School of Zoology, University of New South Wales.

Born in Sydney in 1945, Professor Archer received his BA in geology and biology in 1967 from Princeton University before returning to Australia as a Fulbright Scholar. He joined the Queensland Museum in 1972 as Curator of Mammals, obtained his PhD in zoology in 1976 from the University of Western Australia, and in 1978 was appointed a Lecturer in Zoology at the University of New South Wales.

Professor Archer's fields of research are vertebrate zoology, biogeography and palaeontology, to which he has made many major and internationally recognised contributions. He has personally organised many major field expeditions which have brought to light, and recovered, large quantities of fossil vertebrate material filling gaps in our knowledge of Australian prehistory. The latest area of his research, the Riversleigh deposit in northern Queensland, is already proving to be one of the richest, most diverse and most valuable fossil deposits ever discovered in Australia. Largely as a result of this work, interest in the palaeontology of Australian marsupials has seen a major rebirth.

Professor Archer has been very active in promoting his research field. He has initiated and organised a number of major symposia on Australian mammalian topics, has edited several important collections of papers and reviews, is editor of "Australian Mammalogy", and was co-editor and a major contributor to the recent definitive volume "Vertebrate Zoogeography and Evolution in Australasia". He has been author or co-author of more than seventy research papers, and in addition has made his findings available to the public in general in an interesting and easily understood manner.

Few Australian biologists would have had such a major impact on a research field at such an early stage in their professional careers. Professor Archer already has proven himself well worthy of the award of the Society's Clarke Memorial Medal.

THE JAMES COOK MEDAL

Professor Ronald Lawrie Huckstep was born in 1926 in Chefoo, China and educated at the Cathedral School, Shanghai and Queen's College, Cambridge. His undergraduate medical training was received at the Middlesex Hospital Medical School of the University of London. He took his medical degrees from the University of Cambridge, graduating MA, MB, B.Chir. in 1952 and MD (Cambridge) in 1957. Fellowships of the Royal College of Surgeons of Edinburgh and of England followed in 1957 and 1958. After considerable orthopaedic experience under illustrious teachers of the University of London Medical Schools, he was appointed a Lecturer in Orthopaedic Surgery at Makerere University College, Uganda in 1960. He was at this time the first orthopaedic surgeon in Uganda. An orthopaedic service for nine million people in Uganda was built up over the next seven years and he was appointed to the first personal Chair of Makerere University College. His responsibilities in this position, besides his personal teaching, research and service included running of orthopaedic workshops, physiotherapy department, orthopaedic assistance service and rehabilitation centres.

Unrest in Uganda in 1972 made Professor Huckstep decide to relocate, and he was appointed the first Hugh Smith Professor of Traumatic and Orthopaedic Surgery in the University of New South Wales.

Professor Huckstep has made numerous contributions to Orthopaedic Societies and special meetings throughout the world, particularly in the Third World. His own research has spanned the topics of rehabilitation of poliomyelitis victims, late effects of untreated fractures, congenital deformities, osteomyelitis and tuberculosis of bones and joints. He has personally designed a number of orthopaedic appliances and devices, widely in use at the present time including the Huckstep Intramedullary Compression Nail.

He was the Founder in 1973 of World Orthopaedic Concern and was made an Honorary Member of the organization in 1978.

He has been concerned with patients crippled by disease and inadequate treatment throughout his life and has developed by personal research and effort, appliances and operations to help such individuals. In addition he has founded organizations to implement such benefits. For these outstanding contributions to orthopaedic surgery and human welfare, Professor Huckstep is awarded the James Cook Medal for 1984.

CITATIONS

THE SOCIETY'S MEDAL

The Society's Medal for contributions to the progress of the Society and to Science is awarded to Dr. Robert Sylvester Vagg, M.Sc. (N.S.W.), Ph.D. (Macq.), FRACI.

Dr. Vagg joined the Royal Society of New South Wales in 1973. He has served on the Council since 1980, being President in 1983 and Vice-President in 1984. He was Convenor of the Working Party to review the Society's role and activities. The Working Party's report, presented to Council in 1984, is a wide-ranging review and its recommendations will be an invaluable guide to the Society's future. He has also been largely responsible for finding a new home for the Society at Macquarie University. He has contributed three papers to the Society's Journal, including his Presidential Address entitled, "Chiral Discriminations and Molecular Propellers".

Bob Vagg gained his Bachelor of Science from the University of New South Wales in 1965 and his Master of Science in 1967, and then moved to Macquarie University for his Doctorate. He spent two years at the University College, London, working with Professor Nyholm, and then returned to Macquarie University where he is now a Senior Lecturer in the School of Chemistry.

His research is on synthetic and structural problems in inorganic chemistry. His recent work deals with molecular chirality (that is, left- and right-handedness) and its effects on molecular interactions.

He is a keen cricketer, having played for both the University of New South Wales and Macquarie University, and at one time captained the Macquarie First Eleven.

For his contributions to chemistry and to the Royal Society Dr. Vagg is a very worthy recipient of the Society's Medal.

EDGEWORTH DAVID MEDAL

Dr. Alan James Husband is awarded the Edgeworth David Medal for 1984. Born in Sydney in 1949, he was educated at Fort Street Boys' High School and the University of Sydney, where he graduated with first class honours in the Faculty of Agriculture in 1972. He was author of a thesis entitled "Passive transfer of immunity and autogenous production of immunoglobulins in calves".

Dr. Husband is one of the outstanding young immunologists in Australia with planned and directed research work in mucosal immunity over the last 10 years. He obtained his Ph.D. in association with the University of Sydney and the CSIRO, working in mucosal immunity under the tutelage of Professor Alec Lascelles in 1975 with a thesis concerning "Perinatal immunity in ruminants". He pursued post-doctoral studies in the Sir William Dunn School of Pathology at Oxford, where he did some of the key work in establishing mechanisms whereby vaccines presented to mucosal surfaces stimulate an appropriate defensive response. This work was done with Professor Gowan who is known for his many years' research into the circulating patterns of lymphocytes.

Since that time Dr. Husband returned, initially to work with the CSIRO, and then moved to Newcastle as a senior lecturer in the Department of Pathology, University of Newcastle. Dr. Husband has an active research group and has successfully supervised Ph.D. students.

In recent times his work has broadened to include examinations of a number of exciting new areas linking the immune system with behaviour patterns as well as continuing work with vaccines, attempting to identify the best way of stimulating immunity against intestinal parasites and other infections.

Dr. Alan Husband is an active and productive research worker and group leader and has contributed to our knowledge of mucosal immunity. He is a very worthy recipient of the Edgeworth David Medal for 1984.

THE ARCHIBALD D. OLLÉ PRIZE

The Archibald D. Ollé Prize for excellence in an original scientific paper contributed by a member to the Society's Journal and Proceedings is shared in 1984.

The Prize is awarded to Mr. R.A.L. Osborne for his paper entitled, "Multiple Karstification in the Lachlan Fold Belt in New South Wales: Reconnaissance Evidence", and to Drs. Terence J. Goodwin, Robert S. Vagg and Peter A. Williams for their paper on "Chiral Metal Complexes. Part 15. Alanine and Proline Complexes of [N,N'-Di(2-picolyl)-1R-diaminocyclohexane] cobalt(III". Both papers were published in Volume 117, Parts 1/2 of the Journal and Proceedings.

POLLOCK MEMORIAL LECTURE

Dr. R.S. Pease delivered the Pollock Memorial Lecture for 1984 on 28th November, 1984, the title of the Lecture being, "The Electromagnetic Pinch: from Pollock to the Great European Torus".

Rendel Sebastian Pease was born in 1922, and was educated at Cambridge University where he gained an MA and ScD. He is now the U.K. Atomic Energy Authority's Programme Director for fusion, and is one of the most distinguished plasma physicists in the United Kingdom. His election to a Fellowship of the Royal Society of London in 1977 attests to this fact. Before taking up his present appointment, Dr. Pease was Director of the Culham Laboratory for Plasma Physics; he was appointed to that position in 1968. Dr. Pease has published extensively in the field of plasma physics; some of his early work was concerned with the pinch phenomenon. This gives him a very interesting connection with Professor J.A. Pollock, who published the first theoretical analysis of the pinch effect in a metallic conductor. During the period 17th October to 17th December, 1984, Dr. Pease is the Gordon Godfrey Visiting Professor in Theoretical Physics at the University of New South Wales.

OBITUARY

FLORRIE MABEL QUODLING

Dr. Florrie Mabel Quodling, a Life Member of the Society, died on 16th February, 1985 after a short illness, at the age of eighty-three.

Photograph courtesy of the Sun-Herald

Florrie Quodling was born in Petersham on 15th June, 1901, and was educated at the Sydney Girls' High School, which was then in Elizabeth Street, during World War I. She gained an Exhibition to the University of Sydney, and entered the Faculty of Science in 1920, graduating with First Class Honours in Geology and Mineralogy in 1924. She began teaching mineralogy and crystallography in the Geology Department in 1924, and these subjects remained her specialties through her teaching career which spanned 38 years. She retired as Senior Lecturer in 1962.

Flo's classes will always remain memorable to geology students of that period. She took a personal interest in all her students, and later in their careers and families.

Despite all that time and attention to teaching, Florrie Quodling enjoyed the challenge of original research work, at first in collaboration with Professor David Mellor on the properties of inorganic complexes, then in structural crystallography, and finally on the nature and origin of blue-schists. It was typical, however, that her most important contribution to scientific knowledge - the study of blue-schist mineralogy for which she gained a Ph.D. from the University of Sydney in 1966 - was completed only after the demands of teaching had passed. She carried out her research on a polarizing microscope purchased from the proceeds of her retirement testimonial. Donations to the testimonial fund in fact far exceeded the cost of the microscope, and at Flo's insistence, the unspent balance went to establish a student prize in crystallography and petrology in 1963 - the Quodling Testimonial Prize.

Florrie Quodling joined the Royal Society of New South Wales in 1935 and contributed six papers on crystallography and mineralogy to the Journal and Proceedings.

Her other two interests were gardening and travelling. Her garden at Epping, and later at Whale Beach was always a "picture". During 1951 she took part in the pioneering geophysical survey in outback Australia, and in the sixties, her work on blue-schists took her frequently to her beloved Port Macquarie. In 1956, she visited universities in America and Europe, and in 1967, accompanied by her niece Jill O'Sullivan, she went around the world stopping off in such places as India, Iran, Europe, North America and Fiji. She particularly loved the small villages in the mountains of Switzerland and North Italy, and at the age of 80 was travelling alone on a Eurail pass, staying at small hotels and taking daily trips on cable cars and lake steamers. Failing health forced Flo to leave her home at Whale Beach in 1984, and retire to the Cotswold Retirement Village at Turramurra.

J.A.D.

ABSTRACT OF PROCEEDINGS 1984-85

The Annual General Meeting and eight General Monthly Meetings were held at Macquarie University. One General Monthly Meeting was held at the University of Sydney. Abstracts of the proceedings of these meetings are given below.

In addition, the Liversidge Research Lecture was deliver on 17th October, 1984, by Associate Professor D.H. Napper, at the University of New South Wales. The title of the Lecture was "The Birth and Growth of Latex Particles". The Pollock Memorial Lecture was held on 28th November, 1984, in the Stephen Roberts Lecture Theatre, the University of Sydney. Dr. R.S. Pease, FRS, Director of the U.K. Atomic Energy Authority, spoke on "The Electromagnetic Pinch: from Pollack to the Joint European Torus". The first of a series of seminars on "Scientific Sydney" was held on Saturday, 24th November, 1984, in conjunction with the Royal Australian Histrocial Society, at History House Macquarie Street. The title was "Artisans and Managers: Exploring Technical Education in N.S.W., 1884-1984".

APRIL 4th

956th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Dr. R.S. Vagg, was in the Chair and 24 members and visitors were present. Albert Freedman was elected to membership.

117th Annual General Meeting. Followed the 956th General Monthly Meeting. The Annual Report of the Council and the Annual Statement of Accounts were adopted.

The following awards from 1983 were announced and presented (with the exception of the Walter Burfitt Prize): Walter Burfitt Prize and Medal to Dr. William Stephen Hancock; James Cook Medal to Dr. Struan Keith Sutherland; the Edgeworth David Medal to Dr. Denis Wakefield; and the Ollé Prize to Mr. David S. King and Dr. Nicholas R. Lomb.

Messrs. Wylie and Puttock, Chartered Accountants, were elected Auditors for 1984.

The following Office-Bearers were elected for 1984-85:

President:	Dr. R.S. Bhathal
Vice-Presidents:	Dr. R.S. Vagg
	Professor T.W. Cole
	Mr. W.H. Robertson
	Professor R.L. Stanton
	Professor B.A. Warren
Honorary Secretaries:	Mr. D.S. King
	Mrs. M. Krysko v Tryst
Honorary Librarian:	Dr. F.L. Sutherland
Honorary Treasurer:	Dr. A.A. Day
Members of Council:	Professor P.J. Derrick
	Mr. H.S. Hancock
	A/Professor J.H. Loxton
	Professor R.M. MacLeod
	Mr. R.H. Read
	Mr. M.A. Stubbs-Race
	Dr. W.J. Vagg
	A/Professor D.E. Winch
New England Representative:	Prof. S.C. Haydon

The Presidential Address, "Chiral Discriminations and Molecular Propellers" was delivered by Dr. R.S. Vagg.

The incoming President, Dr. R.S. Bhathal, was installed and introduced to members.

MAY 2nd

957th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The Vice-President, Dr. R.S. Vagg, was in the Chair and 35 members and visitors were present. Robert Armstrong Lee Osborne, Patricia Mary Callaghan, Peter Beaton and Eric Adam were elected to membership.

Papers read by title only: "Multiple Karstification in Limestones of the Lachlan Fold Belt in N.S.W." by R.A.L. Osborne; "Computation of Reaction Matrix Parameters by Perturbation Theory" by J.L. Cook, E.K. Rose and B.E. Clancy; and "Stratigraphic Revision of the Early Carboniferous Flagstaff Sandstone, Southern New South Wales" by I.D. Lindley.

An address entitled "Life, Computers and Everything" was delivered by Mr. Neville Holmes of IBM Australia Ltd.

JUNE 6th

958th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Dr. R.S. Bhathal, was in the Chair and 38 members and visitors were present.

Papers read by title only: "Chiral Metal Complexes, Part 15. Alanine and Proline Complexes of [N,N'-Di(2-picolyl)-1R,2R-diaminocyclo-hexane] cobalt(III)" by T.J. Goodwin, R.S. Vagg and P.A. Williams; "The Stratigraphic Palynology of the Murray Basin in N.S.W.: II The Murrumbidgee Area; III The Lachlan Area" by H.A. Martin; "Changing Employment Patterns and Truncated Development in Australia" by B.O. Jones (Annual Dinner Address, 21st March, 1984); and "A Biographical Register of Members of the Australian Philosophical Society (1850-55) and the Philosophical Society of N.S.W. (1856-66)" by A.A. Day and J.A. Day.

An address entitled "Tracing Your Ancestors in Britain and Ireland" was given by Commander William Swan of the Society of Australian Genealogists.

JULY 4th

959th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Dr. R.S. Bhathal, was in the Chair and 28 members and visitors were present. Barry O. Jones, MP, and Gregory Monaghan were elected to membership. The Clarke Memorial Medal for 1983 was presented to Dr. Keith A.W. Crook.

A paper entitled "Notes on Freshwater Zooplankton Found in Central Province, Papua New Guinea" by B. Vlaardingerbroek, was read by title only.

An address entitled "Speech Synthesis and Aids for the Handicapped" was delivered by Professor R.E.

Aitchison, Professor of Electronics at Macquarie University.

AUGUST 1st

960th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Dr. R.S. Bhathal, was in the Chair and 36 members and visitors were present.

An address entitled "The Construction of Violins" was delivered by Mr. John Godschall-Johnson.

SEPTEMBER 5th

961st General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Dr. R.S. Bhathal, was in the Chair, and 25 members and visitors were present.

A paper entitled, "Chiral Discriminations and Molecular Propellers" (Presidential Address) by Dr. R.S. Vagg, was read by title only.

Professor R. Schwarzenberger, Professor of Mathematics at the University of Warwick at Coventry, spoke on "Teaching Mathematics: the Importance of Mistakes".

OCTOBER 3rd

962nd General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Dr. R.S. Bhathal, was in the Chair, and 24 members and visitors were present. Owen Andrew Reddecliffe Reddecliffe and Edward Donald O'Keeffe were elected to membership.

A symposium on "Scientific Literacy" was held, the speakers being Dr. R.S. Bhathal of the Power House Museum; Mrs. G. Kuhn, Senior Science Mistress, Abbotsleigh School, Wahroonga; Professor R.M. MacLeod of the Department of History, University of Sydney; and Dr. Peter Pockley, Department of Public Affairs, University of New South Wales.

NOVEMBER 7th

963rd General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Dr. R.S. Bhathal, was in the Chair and 16 members and visitors were present. Dr. Denis Wakefield and Miss Susan Elizabeth Kidd were elected to membership.

A lecture entitled "Venoms of the Funnel-Web Spider" was delivered by Dr. D.D. Sheumack, Post-Graduate Fellow in the School of Chemistry, Macquarie University.

DECEMBER 5th

964th General Monthly Meeting. Location: Stephen Roberts Lecture Theatre, the University of Sydney. The President, Dr. R.S. Bhathal, was in the Chair, and 22 members and visitors were present.

Papers read by title only: "Interpretation of Macroscopic Fold Structures in the Willyama Supergroup of the Thackaringa Area, Broken Hill, N.S.W." by I.L. Willis; "Weber Transform of Certain Generalized Functions of Rapid Growth" by R.S. Pathak and H.K. Sahoo (communicated by W.E. Smith); and "Oligocene and Miocene Volcanic Rocks and Quartzose Sediments in the Southern Tablelands, N.S.W.: Definitions of Stratigraphic Unit" by P. Bishop (communicated by R.A.L. Osborne).

A lecture and demonstration was given by Mr. Carey Beebe of Hurstville on "Historic Stringed Keyboard Instruments".

NOTICE TO AUTHORS

A "Style Guide to Authors" is available from the Honorary Secretary, Royal Society of New South Wales, PO Box N112, Grosvenor Street, NSW 2000, and intending authors *must* read the guide before preparing their manuscript for review. The more important requirements are summarized below.

GENERAL

Manuscripts should be addressed to the Honorary Secretary (address given above).

Manuscripts submitted by a non-member must be communicated by a member of the Society.

Each manuscript will be scrutinised by the Publications Committee before being sent to an independent referee who will advise the Council of the Society on the acceptability of the paper. In the event of rejection, manuscripts may be sent to two other referees.

Papers, other than those specially invited by Council, will only be considered if the content is substantially new material which has not been published previously, has not been submitted concurrently elsewhere, nor is likely to be published substantially in the same form elsewhere. Well-known work and experimental procedure should be referred to only briefly, and extensive reviews and historical surveys should, as a rule, be avoided. Letters to the Editor and short notes may also be submitted for publication.

Original papers or illustrations published in the Journal and Proceedings of the Society may be reproduced only with the permission of the author and of the Council of the Society; the usual acknowledgements must be made.

PRESENTATION OF INITIAL MANUSCRIPT FOR REVIEW

Typescripts should be submitted on bond A4 paper. A second copy of both text and illustrations is required for office use. Manuscripts, including the abstract, captions for illustrations and tables, acknowledgments and references should be typed in double spacing on one side of the paper only.

Manuscripts should be arranged in the following order: title; name(s) of author(s); abstract; introduction; main text; conclusions and/or summary; acknowledgments; appendices; references; name of Institution/Organisation where work carried out/or private address as applicable. A table of contents should also accompany the paper for the guidance of the Editor.

Spelling follows "The Concise Oxford Dictionary".

The Systeme International d'Unites (SI) is to be used, with the abbreviations and symbols set out in Australian Standard AS1000.

All stratigraphic names must conform with the International Stratigraphic Guide and must first be cleared with the Central Register of Australian Stratigraphic Names, Bureau of Mineral Resources, Geology and Geophysics, Canberra.

Abstract. A brief but fully informative abstract must be provided.

Tables should be adjusted for size to fit the format paper of the final publication. Units of measurement should always be indicated in the headings of the columns or rows to which they apply. Tables should be numbered (serially) with Arabic numerals and must have a caption.

Illustrations. When submitting a paper for review all illustrations should be in the form and size intended for insertion in the master manuscript. If this is not readily possible then an indication of the required reduction (such as reduce to ½ size) must be clearly stated.

Note: There is a reduction of 30% from the master manuscript to the printed page in the journal.

Maps, diagrams and graphs should generally not be larger than a single page. However, larger figures can be printed across two opposite pages.

Drawings should be made in black Indian ink on white drawing paper, tracing cloth or light-blue lined graph paper. All lines and hatching or stripping should be even and sufficiently thick to allow appropriate reduction without loss of detail. The scale of maps or diagrams must be given in bar form.

Half-tone illustrations (photographs) should be included only when essential and should be presented on glossy paper.

Diagrams, graphs, maps and photographs must be numbered consecutively with Arabic numerals in a single sequence and each must have a caption.

References are to be cited in the text by giving the author's name and year of publication. References in the reference list should follow the preferred method of quoting references to books, periodicals, reports and theses, etc., and be listed alphabetically by author and then chronologically by date.

Abbreviations of titles of periodicals shall be in accordance with the International Standard Organization IS04 "International Code for the Abbreviation of Titles of Periodicals" and International Standard Organization IS0833 "International List of Periodical Title Word Abbreviations" and as amended.

MASTER MANUSCRIPT FOR PRINTING

The Journal is printed by offset using pre-typed pages. When a paper has been accepted for publication the author will be supplied with a set of special format paper. The text may either be typed by electric typewriter directly on to the format paper or a word-processor print-out assembled on it. Details of the requirements for text production will be supplied with the format paper.

Reprints. An author who is a member of the Society will receive a number of reprints of his paper free. An author who is not a member of the Society may purchase reprints.

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THE ROYAL SOCIETY OF NEW SOUTH WALES

Address:— Royal Society of New South Wales,
PO Box N112 Grosvenor Street,
NSW 2000,
Australia.

THE ROYAL SOCIETY OF NEW SOUTH WALES

The Society originated in the year 1821 as the Philosophical Society of Australasia. Its main function is the promotion of Science through the following activities: Publication of results of scientific investigation through its Journal and Proceedings; the Library; awards of Prizes and Medals; liaison with other Scientific Societies; Monthly Meetings; and Summer Schools for Senior Secondary School Students. Special Meetings are held for the Pollock Memorial Lecture in Physics and Mathematics, the Liversidge Research Lecture in Chemistry, and the Clarke Memorial Lecture in Geology.

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Tribute to Sir Frank Macfarlane Burnet

G. J. V. Nossal

Sir Frank Macfarlane Burnet was one of the Royal Society of N.S.W.'s most distinguished Honorary Members. He died on 31st August, 1985, in his 86th year. Unquestionably one of the fathers of Australian medical science and one of this century's greatest biologists, he was awarded the James Cook Medal by the Society in 1954 for his outstanding achievements. We are indebted to Professor Sir Gustav Nossal, Director of the Walter and Eliza Hall Institute of Medical Research, for permitting us to publish the following tribute to Sir Macfarlane, which was originally given on ABC Radio on 4th September, 1985.

As Sir Macfarlane Burnet is being royally farewelled in the City of Melbourne, where he did all his important work, it is worth pausing to consider what were the qualities of intellect and spirit that made him such an outstanding and substantially different kind of scientific thinker. To attempt this analysis, we must recall the circumstances that prevailed in Australian science when Burnet made his start. Over 60 years ago, when he first entered the laboratory as a humble pathology registrar at the Melbourne Hospital, biomedical science was hardly stirring in Australia. The universities were good trade schools, the art of medicine being passed on largely by part-time clinical teachers with Collins Street practices. Rivett and C.S.I.R. were years away; The Walter and Eliza Hall Institute had made a faltering beginning and had just appointed Charles Kellaway as its second Director; in short, no serious person would contemplate learning medical research in Melbourne. For that, you had to go to London! This news, however, had not reached our shy young beetle-collector. Schooled by nothing other than the scientific literature, he began to study the bacterial viruses which later became the vehicles for the birth of the new genetics. Burnet was soon publishing in international journals. He did indeed go to London, at about the same time as his classmate Roy Cameron and his South Australian contemporary Howard Florey, but, unlike them, he quickly came back and with the exception of a second two-year British stint in his early thirties, the whole of the rest of his career was back at home. He built the Hall Institute into a formidable international force in virology and immunology. He made Melbourne a Mecca of medical research. This entirely committed Australian deserves to be recognized as the father of Australian medical science.

How, then, was he so successful, right from the beginning of his research career? Burnet's was a contemplative, almost a solitary, kind of genius. The majority of his papers were single-author works, using simple, elegant techniques, frequently of his own devising and requiring little more than a Pasteur pipette, a test tube, a fertile hen's egg and a microscope. Yet his studies differed sharply from the then current image of Paul de Kruif-style microbe hunting. Burnet's work did not *end* with the isolation and characterization of some new virus, it *began* there. The virus, important though it might be in its own right, was just the beginning of a rounded journey into biology. A particular finding made in the classical reductionist mode of normal science was never left alone. It became a springboard for speculation on the nature of life processes, a tool in the synthesis of a peculiarly holistic view, that drew microbiology, genetics, epidemiology, immunology and ecology into an intricate web of interlocking concepts. For the forging of these constructs, Burnet needed quiet and isolation. Not for him the thrust and parry of a vigorous discussion with a peer or gifted student. How many times have I seen him struggle to accommodate a new finding, his or someone else's, into the constantly changing pattern of his speculative framework. He would observe or listen, eyes half-shut, brow puckered in concentration. Then out would come pen and paper, and a doodle of simple images marginally annotated in his small, neat handwriting would emerge. And then, so often, "I'll have to think about this!" In the quiet of his study, that night (never next week, never after the next committee meeting or trip) the problem would be attacked again, and the next morning, with quiet certainty: "Nossal, I've found the answer!"

For Burnet, there was never a failed experiment. Most of us, when the fruit of a week's work emerges from our complex array of instruments - the gamma counters, the spectrophotometers, the protein sequenators - cope badly when the results appear confusing. We grind our teeth, we curse, "The bally thing didn't work. I'll have to set it all up again in the morning!" Burnet believed totally that nature was always trying to tell him something. So he would take the unexpected, uninterpretable results and turn them this way and that, add and subtract figures in various simple ways, play with the data until they were forced into some kind of order. Somewhat mischievously, he would say: "Nossal, I never repeat an experiment". He didn't mean it literally, of course. What he meant was that each experiment, no matter how small, would suggest some extra step, an extra control, an extra slight experimental variation, making the confirmatory experiment always into an elaboration, a broadened learning experience. It *was* true that Burnet published very quickly, some felt prematurely. He wrote freely, correctly as he went. Many papers did not see a draft before being sent off. His critics accused him of sloppy work, yet in truth he was a gifted experimentalist. But he was never interested in dotting i's or crossing t's. When he was convinced that he was right, he would publish and move on to the next problem, leaving the details to be sorted out by someone else. This capacity to skim the cream off the top certainly did not endear him to his competitors, particularly in the U.S.A.

Sir Macfarlane Burnet at work.

Today, we live in an age of technology. The era of the big battalions has arrived in research. Yet science is primarily about ideas. Advanced techniques serve as essential tools, but the logical and imaginative constructs that human minds produce when the tools have done their work are the essence of science, and that which distinguishes it from technology. Burnet believed passionately in ideas. His extraordinary gift was to take apparently unconnected observations and fit them, almost force them, into whatever theoretical framework was his current obsession. Nowhere was this more evident than in his two lasting contributions to immunology, the definition of tolerance and the creation of the clonal selection hypothesis. He was fascinated by the need for the immune system to have some way of distinguishing self from not self, the simultaneous capacity to mount a vigorous antibody response to *any* foreign substance that enters the body, but to avoid a destructive attack on the body's own tissues. He was equally intrigued by the immense diversity of antibodies, the seemingly endless array of specificities that could be generated as infections or vaccines hit the body. The problems were on his mind for 15 years. To address them, he drew on knowledge accruing in virology, zoology, embryology, haematology, enzymology and molecular biology instead of just conventional immunology. The technical details of the theories need not concern us here. Suffice it to say that he puzzled out the correct solution to these two central issues of immunology. This represents his greatest and most lasting achievement, richly meriting his 1960 Nobel Prize for Medicine.

The originality was fed by wide and disciplined reading. His sheer industry in keeping up with the literature over a broad front was prodigious. He had zestful, ready willingness to accept the probable

truth of new findings or incompletely proven claims. Most scientists, when their comfortable preconceptions are challenged, are so ready to shout: "I don't believe it", or "I'll wait till it's confirmed". Not so Burnet. He embraced new data as just more grist to the mill, ready for integration into his scientific *Weltanschauung*. As his reputation grew, he was prepared to admit to a human weakness. He was interested in other people's data, not in their theories. The only theories that mattered were his own. As a young man, I suffered greatly because of my perception of this egocentricity. I admired and respected Burnet so much but could not understand this single flaw. But as I have matured, I recognize that *he* could have worked no other way. The shyness, the single-minded preoccupation with his field of work, the almost obsessional desire to generalize, the joyful devotion to scientific truth, all this added up to a unique blend. He *did* perceive the universe distilled through a curiously personal filter, and what went on in his mind had greater reality, clearer *Gestalt*, for him than did anything else. Though the recognition from all round the globe and his happy, secure family life mellowed him a great deal over the last 30 years, the self-absorption never quite disappeared. Rather, it gradually became an amiable, comfortable boundary condition for all dealings with him, almost irrelevant once recognized.

Unlike many pure scientists, Burnet was an able leader, commanding the loyalty of his staff at the Hall Institute which he directed for 21 years. He ensured that the Institute worked on a single main theme of his own choosing, but, that much being said, he left his colleagues a great deal of latitude and never sought to claim credit for one of their discoveries. He was always eminently accessible and ready for scientific discussion. For years, the first thought of most Hall Institute workers when an interesting result came through was: "I wonder what the boss will think of this?" And there simply was no way to gain his approval, so important to all of us, other than through first-rate, honest scientific work. His extraordinary ability to evoke respect and loyality owed much to his own uncompromising scientific honesty and commitment. The full measure of Burnet's gifts as a leader and teacher can be gauged by looking at the careers of his students and disciples. Such scrutiny will reveal his immense influence on Australian science.

Never afraid to speak out on public issues, Burnet devoted himself entirely to writing and lecturing after his retirement 20 years ago. A surprising harvest of 16 books, roughly one a year, came from this period. The blend of popular science, history, sociology and philosophy bore the complete stamp of the master. To this work, as to his science, Burnet brought the unique spectrum of his gifts: originality, imagination, intuition, naive honesty, conceptual breadth and daring, and, yes, wisdom of an almost spiritual kind. No one who has known him will lightly apply the adjective "great" to another. No one who loves Australian science will ever forget his example.

The Walter and Eliza Hall Institute of Medical Research,
P.O., Royal Melbourne Hospital, Vic., 3050, Australia.

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Stratiform Ores and Geological Processes

R. L. Stanton

Clarke Memorial Lecture, delivered to the Royal Society of New South Wales, September 30th, 1985, at the University of Sydney.

In his Clarke Memorial Lecture delivered just 36 years ago, W.R. Browne observed that the old Hebrew Prophet well knew the value of the inspiration and encouragement to be got from the contemplation of an illustrious past when he counselled his countrymen "Look ye unto the rock whence ye are hewn, and the hole of the pit whence ye are digged; look unto Abraham your father and to Sarah that bare you". Browne, whose quiet and unpretentious erudition some of us remember so well, went on to say that it was doubtless with the same thought in mind that the Royal Society of New South Wales had ordained that the memory of the Rev. W. B. Clarke, the father of Australian geology and an early stalwart of the Society, should be kept green by the periodical delivery of a lecture dedicated to his name.

As it happens Browne's Clarke Memorial Lecture of 1949, on "Metallogenic epochs and ore regions in the Commonwealth of Australia", was the first scientific meeting I attended, and from the intellectual point of view it began my life as an active scientist. It is therefore a pleasant coincidence that I, like Browne so many years ago, now come to acknowledge my debt of inheritance to Clarke, and the kindness the Society has done me in asking me to give the lecture this year.

William Branwhite Clarke was born in East Bergholt in Suffolk 187 years ago, on 2nd June 1798, just ten years after the first settlement of Sydney (Jervis, 1944; Grainger, 1982). He was to be the oldest of five children of William Clarke, a schoolmaster soon to go blind, and his wife, the former Sarah Branwhite.

In spite of the family's limited circumstances, Clarke received a first class education at Dedham Grammar School in Suffolk and Jesus College, Cambridge. At Cambridge he read Mathematics, English Literature, Greek and Latin under the tutorship of one William Hustler, and attended lectures in Geology given by the then recently appointed Professor Sedgwick of Trinity. It is said that Clarke's great triumph at Cambridge was his gaining second place to the great historian Macauley in the annual competition for the Chancellor's gold medal for English verse. During his undergraduate years he established himself as a minor poet and, in addition to publication in a number of highly reputable literary journals, he had five volumes of verse published by the time he was thirty years of age. Clarke became an active member of the literary world of the Regency and late Georgian periods and numbered among his acquaintances the famous English landscape painter John Constable, also a product of Dedham Grammar School. On graduation from Cambridge in 1821 - by coincidence the year of foundation of this Society in its first form - Clarke was made deacon of Norwich Cathedral, and in 1823 he was ordained. This extraordinarily versatile man then plunged into a life dedicated to the Church, to Science and to Literature. In 1828 he published his first scientific note - on a new leather construction for the handles of geological hammers. In 1832 he married Maria Stather. In that year he applied for the Chair of English Literature at King's College, London, but was unsuccessful; and in 1833 he accepted the curacy of Longfleet St Mary near Poole in Dorset, at a salary of 45 pounds a year. The next five years were devoted to his family, his ministry, and to increasingly enthusiastic investigations of the geology and coastline of Dorset.

Clarke had lived just half his life when in 1838 and at the age of forty he and his wife and children migrated to Australia. He had suffered increasingly from a rheumatic complaint and on medical advice sought what he thought would be the warmer, drier climate of New South Wales. After an arduous voyage by sailing ship they arrived in Sydney on 1st June 1839 and Clarke took up the curacy of Campbelltown. From that point on he dedicated his life to his family and parishioners, to the intellectual life of the colony, and to the study of natural science, especially the geology of New South Wales. Among many other contributions he served as Headmaster of the King's School at Parramatta, as first rector of old St Thomas's in North Sydney, as a member of the first Council of St Paul's College in the University of Sydney and as permanent Vice President of the Royal Society of New South Wales. He maintained a large correspondence with famous scientists all over the world - Sedgwick, Murchison, Lyell, Darwin, James Dwight Dana, Silliman, von Mueller, de Koninck and many others. He published extensively in the Quarterly Journal of the Geological Society of London, the Proceedings of this Society and in many other journals on subjects as diverse as palaeontology, climatology, seismology, meteoritics, oceanography, physiography and the geology of gold and coal. Long and arduous periods, on unpaid leave from the Church, were spent on geological fieldwork. Extensive journeys carrying out geological exploration of the goldfields of central and southern New

South Wales and northeastern Victoria, the south coast and Hunter Valley coalfields, and the northern New South Wales and southern Queensland tin fields, were made with horse and cart and no more than one or two assistants. With Strzelecki he was, in 1841, the first officially to find gold in New South Wales - some ten years earlier than the commercial discovery by Hargraves in 1851. Through his work on the stratigraphy and dating of the New South Wales coalfields, his investigations of gold and base metal deposits, his generous provision of comprehensive fossil collections to Cambridge and elsewhere, and through his papers and correspondence, it was Clarke who first brought Australian geology and mineral deposits to the attention of the world.

Clarke was elected to fellowship of the Royal Society in London in 1876, just two years before he died - elected in the first year of his nomination with Charles Darwin one of his supporters. He was active in the field until 1877, and died in the evening of the 15th June, 1878, just a few days after his 80th birthday.

Clarke was remarkable for his scientific vision and insight, his versatility and breadth of interest, his boundless energy and enthusiasm for geology and natural science in general, and for his unswerving sense of duty. He was indeed the father of Australian geology and, equally without doubt, Australia's first great economic geologist.

I shall try to speak this evening in the spirit of Clarke - as one with a consuming interest in geology as a pure science and as one also who derives great satisfaction from its application to the material betterment of mankind. In keeping with Clarke's broad vision and great diversity of interest I shall attempt to show how one of its more intriguing problems - the nature and formation of volcanic stratiform ores - may have implications ramifying through much of geological science.

NATURE AND SETTING OF VOLCANOGENIC STRATIFORM ORES

Orebodies of this kind constitute one of the world's principal sources of zinc, lead, copper, cadmium, silver and gold. Their outstanding and most characteristic feature is that they occur, and appear to have formed, as lenses parallel to the primary layering of the volcanic and volcanic-sedimentary rocks that enclose them (Fig.1). They consist of varying concentrations and proportions of the common sulphides pyrite, pyrrhotite, chalcopyrite, sphalerite and galena, together with minor quantities of other sulphides, sulphosalts and native metals such as bismuth, silver and gold. Associated with these ore minerals, all in stratiform arrangement, are a variety of normal rock-forming silicates and carbonates and, from one deposit to another, there may be less common or conspicuous components such as fluorite, barite, anhydrite, gypsum and tourmaline. Not infrequently such deposits display a chemical stratigraphy, with iron and copper sulphides most abundant at the base, grading upwards into zinc- and then lead-rich zones, the whole finally capped by a zone rich in barite and, in some cases, argentiferous galena (Fig. 2).

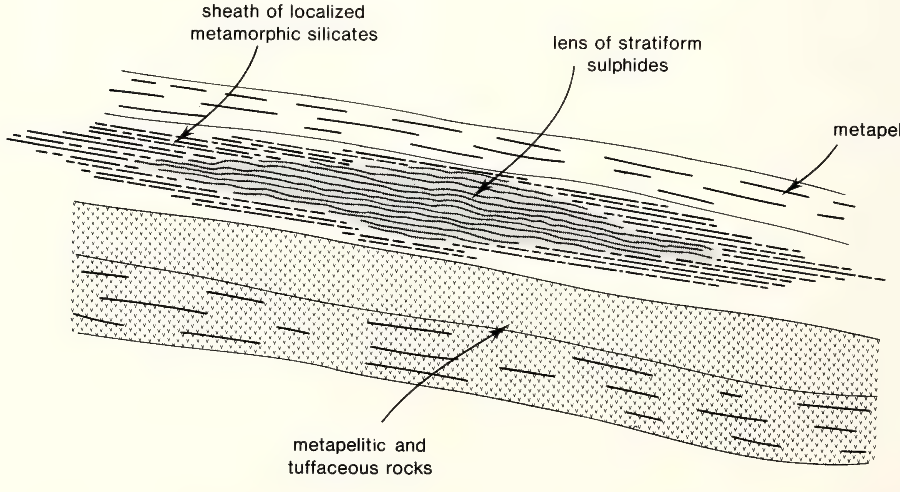


Figure 1. Idealized section through a volcanic stratiform orebody, showing its concordant, bedded nature, the lens-like outline of the aggregate of sulphide beds, the overall volcanic:pelitic environment and, where regional metamorphism has occurred, the close association of a distinctive suite of metamorphic minerals.

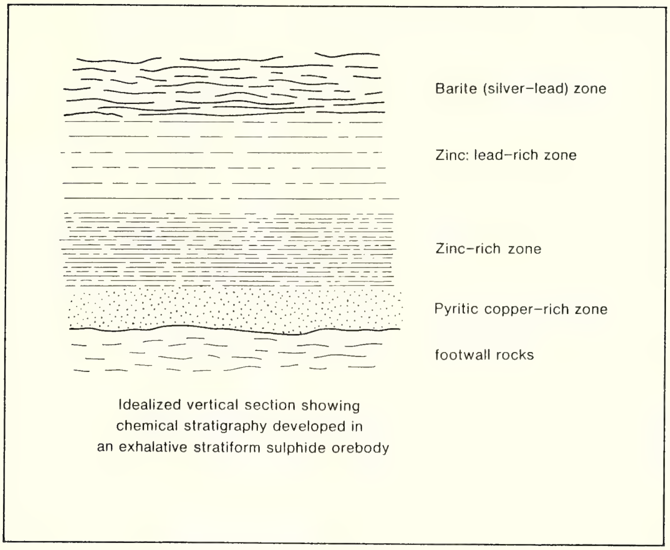


Figure 2. Idealized section through a volcanic stratiform orebody showing the general nature of the chemical/ mineralogical stratification commonly developed within them. The best developed and most complete stratigraphy of this kind is found in the deposits of felsic volcanic association.

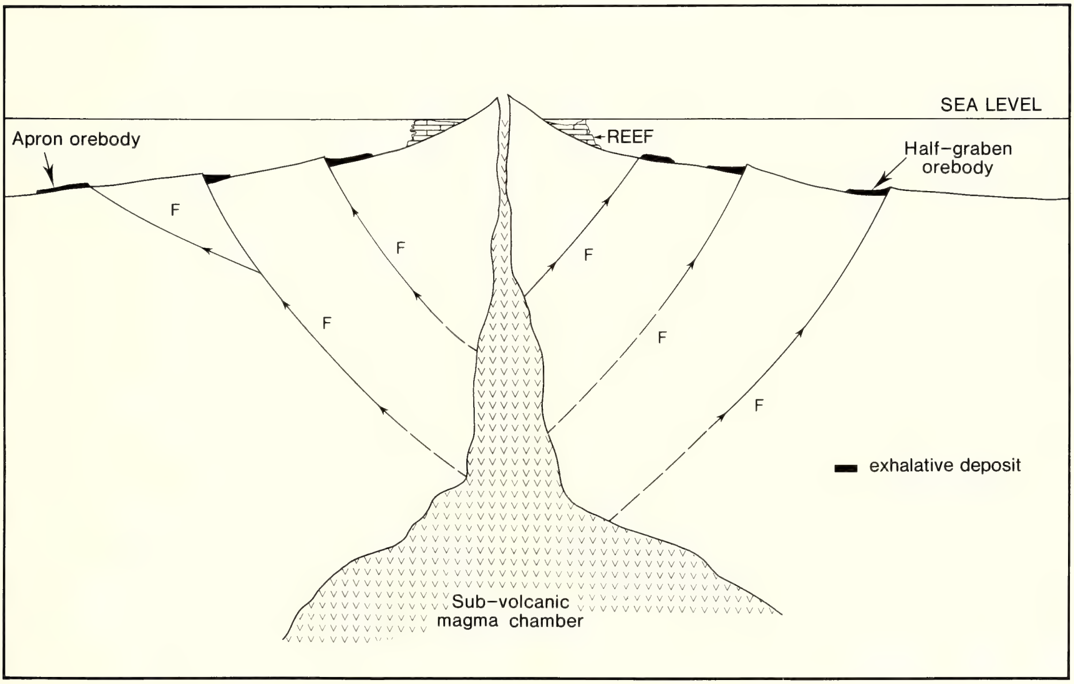


Figure 3. Diagram showing the occurrence of several volcanic stratiform ore bodies as exhalative aprons and half graben accumulations around the sub-sea flanks of a large volcanic centre.

With their close and very characteristic association with volcanic lavas and pyroclastic rocks it is not surprising that deposits of this kind tend to be grouped round old volcanic centres and along volcanic rifts and they show a marked tendency to occur close to just one, or perhaps two, horizons in the relevant volcanic-sedimentary stratigraphic succession.

The general nature of their setting may be portrayed, albeit in simplest terms, as shown in Fig. 3. Their components appear to have been supplied, in one way or another, from a volcanic source - from volcanic hot springs. They have been deposited predominantly as chemical sediments. In many cases they have undergone regional metamorphism. In terms of each of the three processes - volcanic rock formation, sedimentation and regional metamorphism - the orebodies constitute rather remarkable products. They represent a very effective fractionation and abstraction of the metals and a small group of associated elements from the igneous parent; the highly localised concentration of these elements within the chemical sedimentary regime; and the regional metamorphism of distinctive materials - the sulphides, sulphates, fluorides, phosphates and carbonates - whose original sedimentary compositions are both distinctive and known.

We may now consider some of the things that these ores may indicate concerning the three broad processes through which they have been derived and deposited, and by which they have been modified - volcanic crystallization, chemical sedimentation, and regional metamorphism.

VOLCANIC LAVA PETROGENESIS

That certain bedded sulphide ores might have derived from seafloor volcanic hot spring activity has been recognised for at least 140 years. In his Presidential Address to the Geological Society of France delivered on the 5th July, 1847 (just eight years after Clarke's arrival in Sydney), Elie de Beaumont referred to the seafloor precipitation of materials contributed by "submarine volcanic vapours" (de Beaumont, 1847). By 1955, 108 years later and after a long history of controversy, it had become apparent that many stratiform orebodies were indeed of volcanic derivation, that at least much of the volcanism associated with the ores was of island arc affiliation, and that the metal content of these stratiform ores might be systematically related to the composition of the volcanic rocks associated with them (Ehrenberg *et al.*, 1954; Kraume *et al.*, 1955; Stanton, 1955). By 1970 it had been recognized that there was a strong tendency for an association between the formation of pyritic copper ores (of Cyprus type) and deep-sea basaltic volcanism, and a somewhat contrasted association between pyritic lead-zinc ores (of Kuroko type) and island-arc andesite-dacite-rhyolite volcanism (Clark, 1970). By 1977 it was clearly recognized that the Cyprus-type orebodies were almost characteristically rather small and that the associated basaltic volcanism was effusive rather than pyroclastic, whereas the Kuroko- type orebodies ranged up to huge sizes (to over 200 Mt) and the associated andesitic to rhyolitic rocks were almost invariably pyroclastic rather than effusive. It had also become recognized that these two ore associations were simply end members of an extensive spectrum of ore types and related ore-volcanic rock associations.

The accumulation of large quantities of metallic sulphides and associated hydrothermal precipitates as beds and sinter mounds on the modern seafloor has now been spectactularly demonstrated in the Red Sea, on the Mid-Atlantic Ridge and the East Pacific Rise and elsewhere. The mode of formation of the exhalative stratiform ores has therefore not only been deduced from geological evidence, it has been established by direct observation.

For about ten years following general recognition of the association between exhalative ores and volcanic rocks (i.e. until about 1968) there existed a tacit assumption that the exhalations from which the sulphide precipitates were derived were products of a crystallizing, differentiating, volcanic melt: that is, the exhalations were assumed to be products of active sub-seafloor magmatic activity (Fig. 3). However during the period 1968-71 interpretation of sulphur, oxygen and hydrogen isotope data led increasingly to the view that the aqueous component of the exhalations was largely seawater (e.g. Sangster, 1968; Corliss, 1971). This quickly led to the conclusion that the ore solutions were not derived from magma chambers, but represented the re-emergence of seawater that had simply seeped into, and leached, the basalts of the seafloor; that the seawater had percolated down into long-solidified basalts, had become heated, leaching metals from the lavas. Then, as a result of the development of large- scale convection cells, the water re-emerged in restricted areas of the ocean floor there to deposit the metals as sulphide sinters (Fig. 4). It has therefore now become conventional wisdom that volcanism does not play any *active* role in the generation of ore solutions; it simply provides materials - the accumulated basaltic lavas of the oceanic floors - which, long after their emission, become *passive* providers of metals for leaching.

There is however considerable evidence to indicate that this currently fashionable view is at least not wholly true:

(1) The total volume of basalts in the marine environment is many orders of magnitude greater than that of marine andesites, dacites and rhyolites.

(2) On an approximate basis basalts contain about twice as much trace Cu + Zn + Pb (say 180 ppm) as dacites (say 90 ppm).

(3) The major part of the marine basalts is erupted onto the deep sea floor and remains on and beneath the ocean floor for geologically long periods of time. The andesites, dacites and rhyolites on the other hand generally do not appear until the volcanic edifice approaches the surface of the sea, and for the most part are erupted in

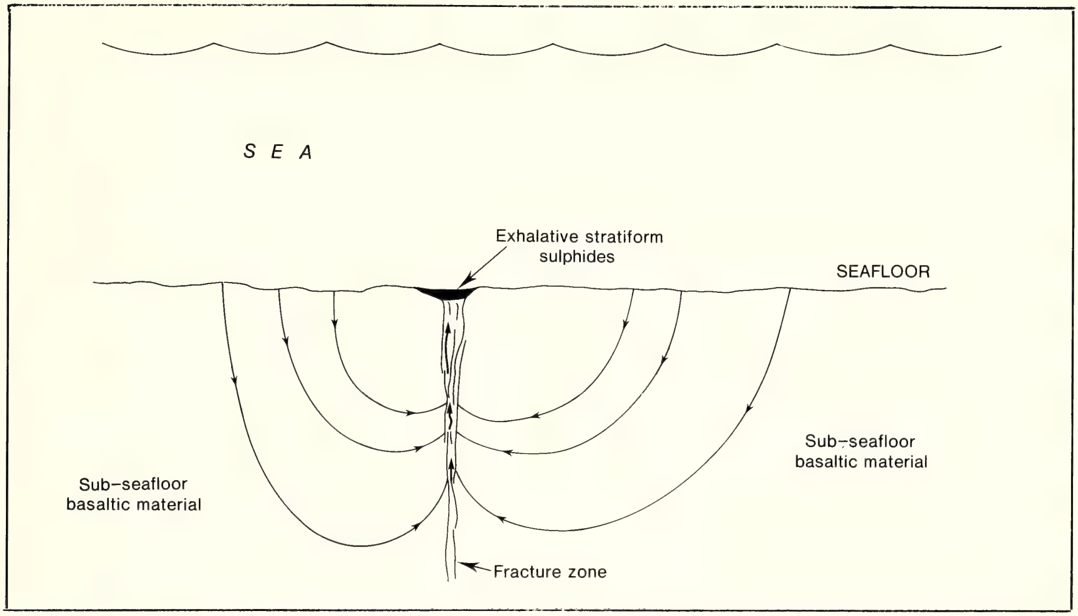


Figure 4. The currently favoured hypothesis: sub-seafloor convection, hydrothermal leaching of accumulated basaltic lavas, and the deposition of exhalative stratiform sulphides in areas of resultant hot spring discharge.

shallow water just prior to volcanism becoming subaerial.

The basalts thus bulk enormously larger, contain more base metal and are exposed to seafloor leaching to a far greater degree than materials of the andesite-rhyolite series. This would suggest that the exhalative orebodies associated with basalts would be far larger and more numerous than those associated with andesites, dacites and rhyolites. Exactly the opposite is the case. Apart from a very few possible exceptions in Japan, Cyprus and Norway, the basalt-associated deposits are small in size and relatively few in number. The world's great volcanogenic orebodies - and there are many of them - are almost invariably associated with rocks of the andesite-rhyolite series and their pyroclastic and volcaniclastic derivatives.

There are several other features of the ores and their occurrence that do not seem to fit the leaching hypothesis very well:

(1) *The association of particular ore types with particular volcanic rock types.* It has already been noted that the characteristic exhalative ore of the basaltic milieu is pyritic copper - ore referred to as "Cyprus type". Zinc is inconspicuous or minor and lead is essentially absent. On the other hand the exhalative ores of the andesite-rhyolite association are conspicuously rich in zinc, commonly possess abundant lead, and contain only relatively minor copper. However basalts contain as much if not more zinc than andesites, dacites and rhyolites and, what is more, contain it in an at least partly more leachable form, i.e. in olivine. It is not easy to see on the basis of the leaching hypothesis, why basalts should not yield zinc deposits just as vast as those associated with some calc-alkaline sequences. In addition basalt contains about one third as much lead as, say, dacite, and on the basis of a leaching mechanism it is not at all clear why the basalt-associated orebodies should be essentially devoid of lead. In this same general connection basalts contain Ni and Co in the same order of abundance as Cu and Zn (Fig. 5). The Ni and Co of basalts is located principally in olivine, the most readily decomposed component of these rocks, and Ni and Co form insoluble sulphides. On the basis of the leaching hypothesis it is therefore not easy to see why the basalt-associated "Cyprus type" deposits do not habitually contain nickel and cobalt sulphides as major constituents.

(2) *The incidence of groups of exhalative orebodies within single districts or in association with individual volcanic piles is, as already emphasised, not random.* Almost invariably the orebodies are confined to one, or at most two, horizons or restricted intervals within the relevant volcanic sequence. This is shown very clearly in such areas as Noranda and Bathurst in Canada (Spence and de Rosen-Spence, 1971; Holyk, 1956), the Rio Tinto district of Spain, the Bathurst area of New South Wales (Stanton, 1955), and the Kuroko province of Japan, and is the rule in virtually every volcanic-sedimentary stratiform ore province known.

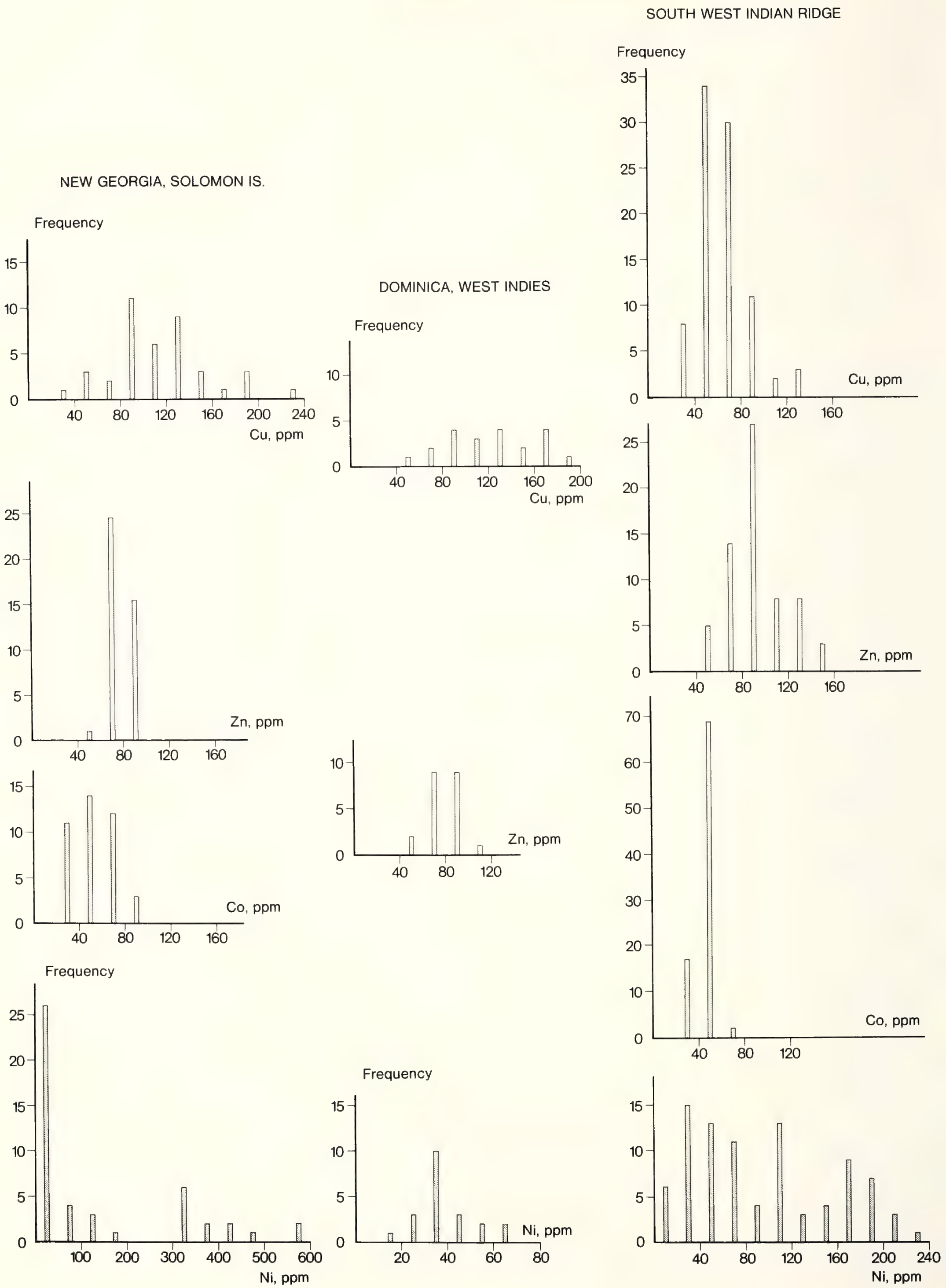


Figure 5. Abundance of Cu, Zn, Co and Ni in basaltic rocks of New Georgia, Dominica (in the Lesser Antilles of the Caribbean and the south-west Indian Ocean Ridge. The four elements all occur at essentially the same order of abundance. (Co determinations not available for Domincan lavas.)

Were the deposits the result of essentially continuous sub-seafloor convection and leaching it might be expected that there would be a relatively even time distribution through the pile, perhaps with an increased incidence near the top. This is clearly not the case - the deposits are always clustered in time - suggesting that exhalation results from some relatively brief, clearly defined event rather than from a long-continued process such as seafloor leaching.

There are thus a number of elementary geological features of the ore-volcanic rock association indicating that the metals were not derived by simple leaching. Some of the evidence, particularly that indicating restriction in time, indicates that exhalation has been triggered by some specific event as the attainment of a particular stage in crystallization and differentiation, a sudden decrease in confining pressure with consequent degassing, abrupt ingress of external water to a magma chamber, or perhaps a combination of two or more of these. The association of particular metal sulphide assemblages with particular volcanic rock types, and the evolving, chemically stratified nature of the ores themselves, suggests that the mechanism that produced the principal elements of the ores was related to progressive crystallization of a melt. This returns us to the earlier view that magmatism has played an *active* role in generation of at least the major part of the metals of the ore solutions and that magmatic differentiation has been the underlying process involved. It is possible that the process is in fact hybrid; a magmatic source yields concentrated ore solutions which move up and mix with copious quantities of seawater at higher levels. The sulphur, oxygen and hydrogen isotope characteristics of the relatively small volumes of magmatic solutions are, as a result, overwhelmed by those of the much larger volumes of seawater. For the purposes of the considerations to follow, we will assume this to be the case, and that the general process is essentially as depicted in Fig. 6.

This conclusion has led Dr B.W. Chappell of the Australian National University and me to undertake a major investigation of the behaviour of the principal chalcophile metals during the progressive crystallization of a modern island arc lava series - that of New Georgia (Fig. 7) in the Solomon Islands (Stanton and Bell, 1969). We have been concerned with the propensity of the various principal minerals to incorporate elements such as Cu, Zn, Ni, Co, Pb and Ba and with the effect of this on the concentration of these elements in the residual volcanic melt. Concentration in the final fractions of the melt is, clearly, of critical importance, as it is from here that the metals would enter the volatile phase, then be transported through the overlying rocks to the seafloor.

In the context of this principle the minerals we have examined are spinel (chiefly magnetite), olivine, clinopyroxene, hornblende and feldspar. Our investigations have shown beyond doubt that some of these minerals may incorporate quantities of the metals that are very significant in terms of their concentrations in an original parent melt, and that the different minerals incorporate the different metals in much more characteristic fashion than has been known to date. Thus for example a parent basaltic melt might contain 80 ppm Zn. In the Solomons lava series the magnetites contain an

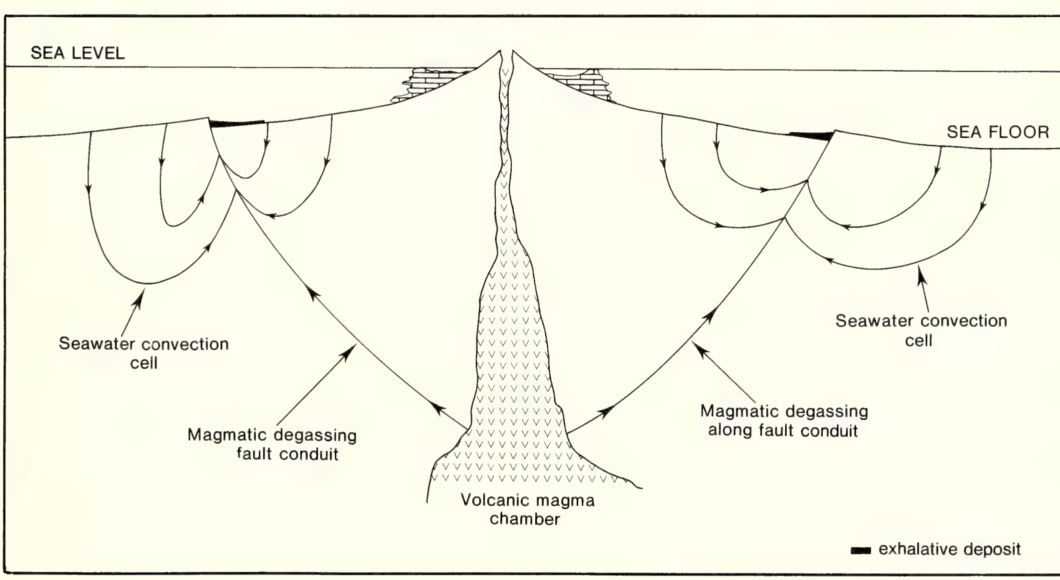


Figure 6. A suggested hybrid magmatic water/seawater process. The metals and part of the ore-forming solutions are derived from the volcanic melt, these encountering, and mixing with, copious quantities of sub-seafloor convecting seawater at higher levels.

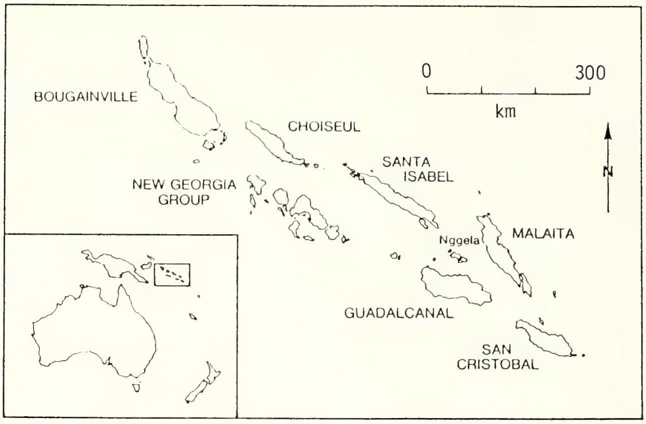


Figure 7. The central portion of the Solomon Islands volcanic archipelago.

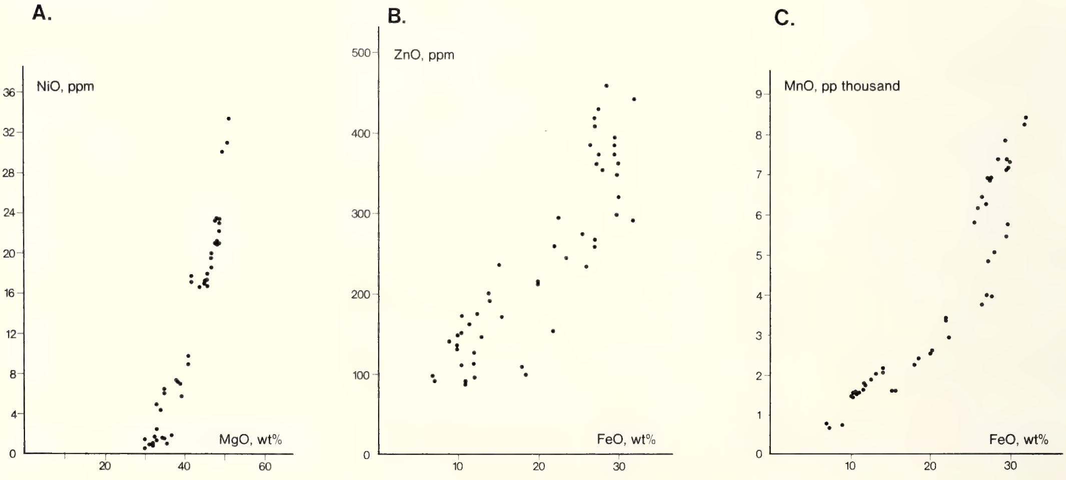


Figure 8. Relations between nickel and magnesium, zinc and iron, and manganese and iron in the olivine crystals of a group of New Georgia lavas. Each point represents an electron microprobe analysis (counting time 4-7 minutes) of single olivine crystal.

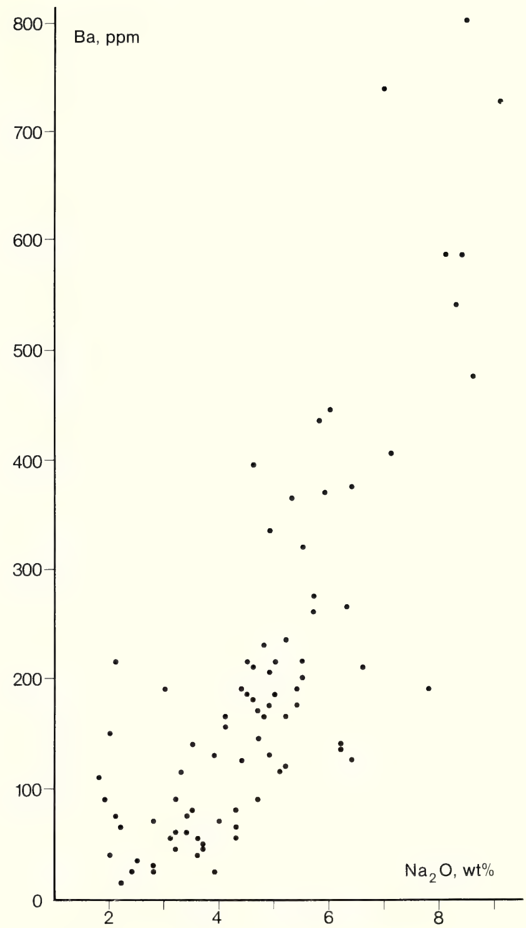


Figure 9. Relations between barium and sodium in plagioclases from the New Georgia lavas (x-ray fluorescence analyses of heavy-liquid separations).

average of 640 ppm Zn, olivines 300 ppm, amphiboles 120 ppm, pyroxenes 60 ppm and feldspars about 10 ppm. Also, as in the long-recognised direct relationship between Ni and Mg in olivines, the propensity for a given volcanic mineral to incorporate a given trace may be greatly influenced by variation in that crystal's major element constitution. For example, in addition to this nickel-magnesium relationship, incorporation of Zn, Cu and Mn in these same olivine crystals is directly related to their iron content (Fig. 8). The capacity of plagioclase feldspars to incorporate barium is related to their sodium content (Fig. 9). Lead, as has been recognised for many years, is always closely associated with potassium-bearing phases, and in calc-alkaline rocks is concentrated in the residual liquid (Fig. 10).

As a result of these influences of mineral chemistry, a number of the ore metals and related elements show well-defined patterns related to the evolution of the lava series. Stemming from the subtraction of nickel, in olivine, the melt is impoverished in this metal at a quite dramatic rate so that by the time differentiation has produced andesitic rocks - and particularly their groundmasses - there is virtually no nickel left in the melt (Fig. 11). This is even further accentuated in the later differentiates - dacites and rhyolites - in which Ni is virtually nil. Nickel is thus almost absent from the late stage, volatile-rich liquid, and there is none available to enter a volatile phase. Not surprisingly exhalative nickel deposits are uncommon. While there may be some deposits of this type in the Archaean, none appear to have formed in the subsequent 2,700 million years of Earth history, a period during which by far the major proportion of the Earth's exhalative orebodies has formed. While - as already noted - it is difficult to account for the paucity of nickel in exhalative ores on the basis of the leaching hypothesis, it is very easy to do so on the basis of magmatic differentiation.

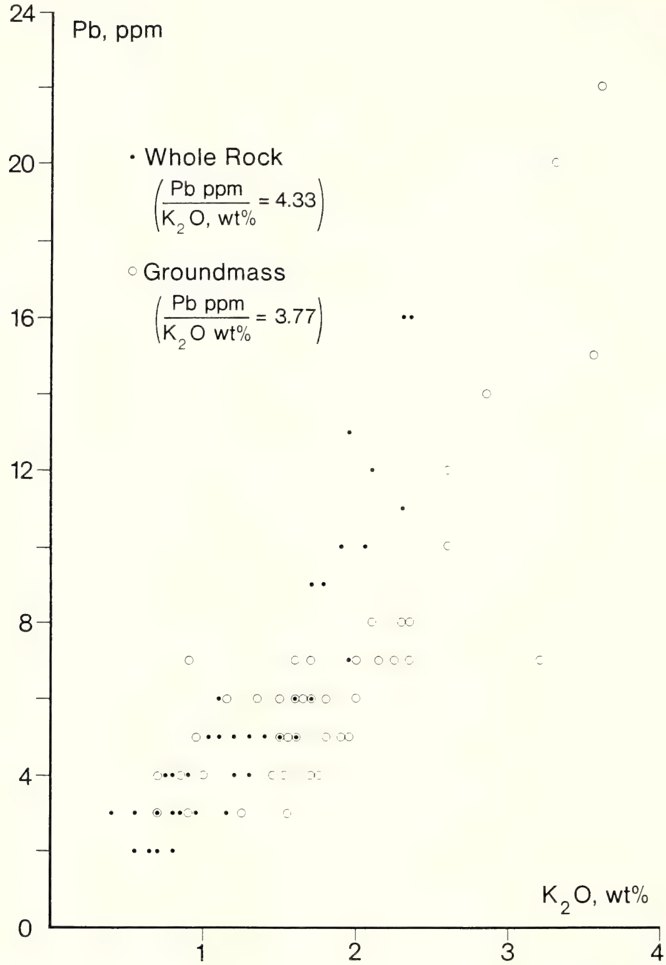


Figure 10.

Relations between lead and potassium in the Solomons lavas. The slightly higher Pb/K_2O ratios in the "whole rocks" vis à vis ground-mass reflects slight partitioning of lead into K-bearing feldspar with respect to the melt.

Lead, on the other hand, is not incorporated in the early-formed silicates and tends to concentrate in the melt (Fig. 12). Olivine, pyroxene and amphibole accept negligible lead and very little lead is incorporated into the plagioclase feldspars. The almost exclusive affinity of lead for potassium-bearing phases means that if K-feldspar does not crystallize, virtually all of the lead in a given volcanic melt is ultimately accumulated into the residual liquid, there to be available for incorporation in a volatile phase.

From all this it is easy to map out the sort of conditions that would favour the concentration of metals in the residual melt, there to be available for partitioning into a vapour phase should this form, with subsequent escape to the seafloor. Oxygen activity should be low, depressing the crystallization of magnetite with its high capacity for incorporating metals. Total Mg/Fe ratios in the melt should be high as among the ferromagnesian minerals olivine, pyroxene and hornblende the more magnesian members incorporate less Cu and Zn than the iron-rich ones. A relatively high silica activity favouring crystallization of pyroxene, with its rather low capacity for incorporating Cu, rather than olivine with its distinctly higher one, would contribute to an increasing concentration of this metal in the melt. Crystallization of calcium-rich rather than albitic plagioclase would concentrate Ba in the melt and crystallization of plagioclase rather than K-feldspar would concentrate Pb in the residual liquid.

All of this leads to the development of some remarkably systematic patterns in the incidence of the metals in the Solomons lava series. Fig. 13 compares the pattern of zinc occurrence with those of iron, calcium and manganese in the lava series as a whole. The plots exhibit a distinctive - and, of course, familiar - kinked configuration and are very similar, suggesting that the distributions of all four elements have been governed by the same or similar processes as the lava series has evolved. The patterns of occurrence of lead, strontium and aluminium on analagous triangular diagrams (Fig. 14) are again very similar among themselves - essentially straight lines - but quite different from those of Zn, Fe, Ca and Mn.

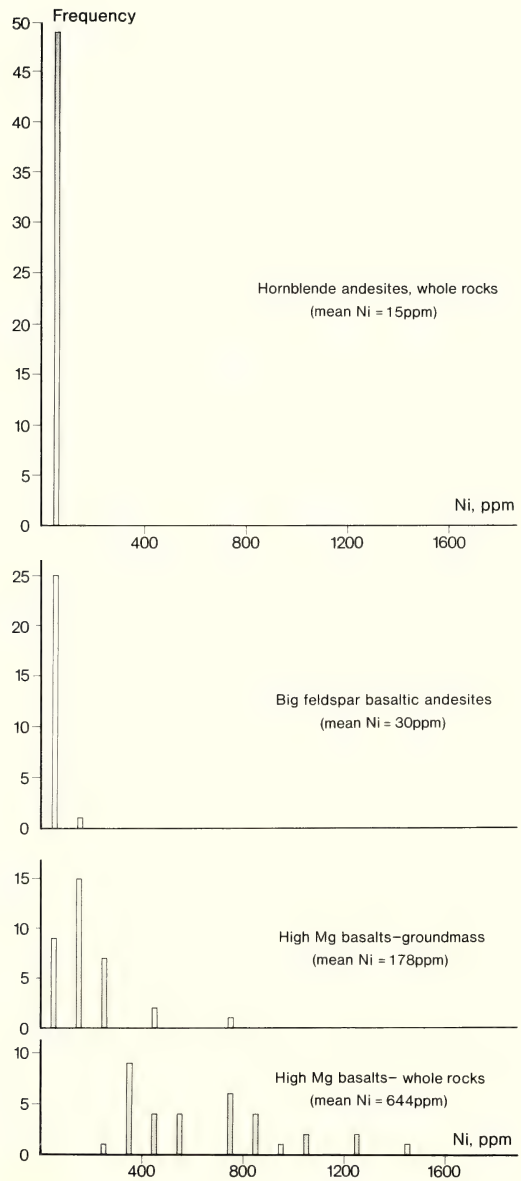


Figure 11. Abundance of nickel in the range of Solomons lavas, demonstrating the remarkable efficiency of magmatic differentiation, i.e. chiefly olivine crystal subtraction, in removing nickel from the melt. Mean Ni in the groundmasses of the andesitic lavas is 5.7 ppm and evolution to dacitic compositions involves the virtual elimination of Ni from the melt.

In this connection Fig. 15 illustrates two well-known petrological trends and their consequences in terms of fractional crystallization (Stanton, 1978). Having in mind the fact that it is a property - and an absolutely inescapable property - of these triangular diagrams that any pair of rocks formed by "closed system" differentiation of a parent must conform to a straight line relation with each other and that parent, the iron enrichment and other principal features of the tholeiites (trend D1) can be substantially accounted for by olivine:pyroxene crystallization subtraction (R) from a basaltic parent (P), and their genesis is now fairly well understood.

The calc-alkaline andesite-rhyolite trend D2 is however most definitely not understood. By analogy with the linear trend generated by the production of iron-rich tholeiite (D1) and complementary residual olivine basalt (R) from our parent basalt P, the production of a near-linear array of andesites, dacites and rhyolites from parent P should generate a complementary suite of rock types along the trend PX. Putting it another way, the development of the iron-enriched tholeiites requires the concurrent development of the complementary, relatively iron- impoverished, high-magnesium suite. In a completely analagous way the development of the iron-impoverished andesite-rhyolite suite must lead - if there have been no gains or losses to the system undergoing differentiation - to a relatively iron-enriched series of rocks falling along the trend PX. Such a suite is however unknown in the island arc volcanic milieu, and the gap is illustrated by Fig. 16. If the calc-alkaline suite has in fact been generated exclusively by crystal subtraction from parent P its inevitable complements have revealed themselves neither to geological nor geophysical observation. If rocks of the commonly observed olivine subtraction line PR are the solid complements of the calc-alkaline suite iron must have been lost from the latter as they formed. If this had not been the case the calc-alkaline rocks would have followed the tholeiitic trend.

This brings us again to the diagram for zinc, Fig. 13. Removal of the ore metals from the melt to the seafloor must occur in the volatile phase; in this process they are lost from the differentiating system of crystals and melt, and pass to another, separate, system. The amount of such metal in the residual melt is thus reduced. It is thus self-evident that the evolution and escape of the volatile metal compounds has a complementary effect on the composition of the remaining melt and hence on the composition of the volcanic rocks that crystallize from that point onwards. In the light of this it is not surprising that we find in Fig. 13 a pattern most readily accounted for by loss of zinc in other than the crystalline phase. A gap analagous to that of PX in Fig. 15 is prominent, and the overall pattern appears to fit very well with the hypothesis that it has been lost to the system in the volatile phase.

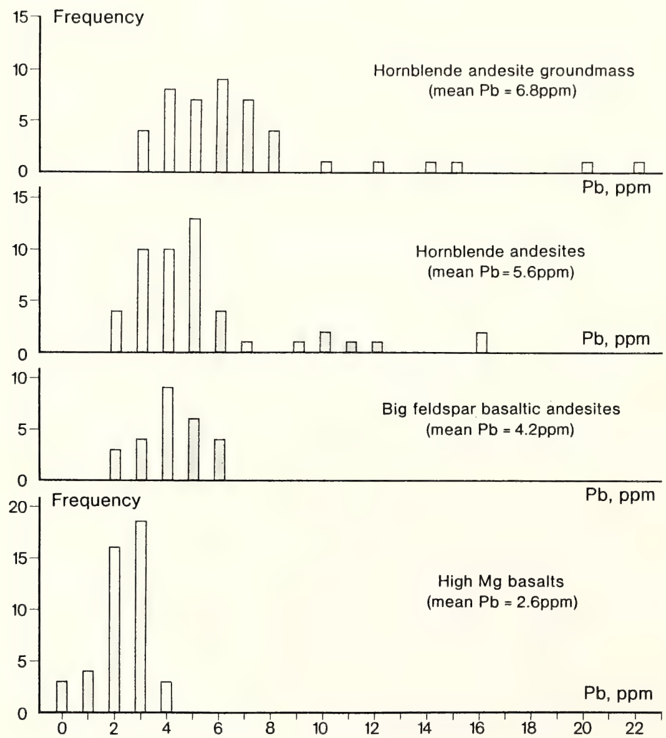


Figure 12.

Abundance of lead in the range of Solomons lavas, showing its progressive increase in the melt as the latter becomes more felsic. This results from the very low level of incorporation of Pb in crystals of ferromagnesian minerals and low-ootassium feldspars.

A simple calculation shows that in the Solomons lava series evolution of the melt from basalt ($SiO_2 \approx 49\%$) to a felsic andesite ($SiO_2 \approx 64\%$) involves a decrease in zinc from about 80 ppm to about 40 ppm - a reduction of 50%. A 50% loss of iron from a basaltic melt, bringing its FeO from 12% down to 6%, would certainly be seen to be a substantial modification, and would clearly change the character of the parent quite fundamentally. Indeed, with respect to iron, it would change the rock from a basalt to an andesite! We may now reflect that iron exhibits a "loss pattern" on the AFM diagram entirely analagous to that of zinc (Fig. 13), and, furthermore, that iron is normally the dominant, and in many cases the overwhelmingly dominant, metal of the exhalative ores and their environments. Not only may individual orebodies contain hundreds of millions of tonnes of iron in pyrite, pyrrhotite, iron-bearing sphalerite, chalcopyrite, carbonate and silicate, but the surrounding sedimentary or metasedimentary formations commonly carry at a lower but still elevated, level of concentration and in much higher total amount, very large quantities of iron in disseminated and bedded magnetite, iron-rich chlorites, sideritic carbonate and disseminated sulphide. Calcium exhibits analagous behaviour in the volcanic series, and as it happens calcium is the most characteristic non-sulphide element of stratiform ores. To the evolutionary stage reached by the Solomons series (felsic andesites to dacites) aluminium, strontium and lead preserve straight lines and appear to have remained within the closed system of crystals and melt.

Recognition of these facts immediately leads us to, or forces us to confront, the possibility that the conspicuous decrease in iron and calcium relative to alkalis in the andesite-rhyolite series (and its sub-volcanic counterparts) is at least largely a reflection of rapid exhalative loss in the aqueous vapour phase during the eruptive episode.

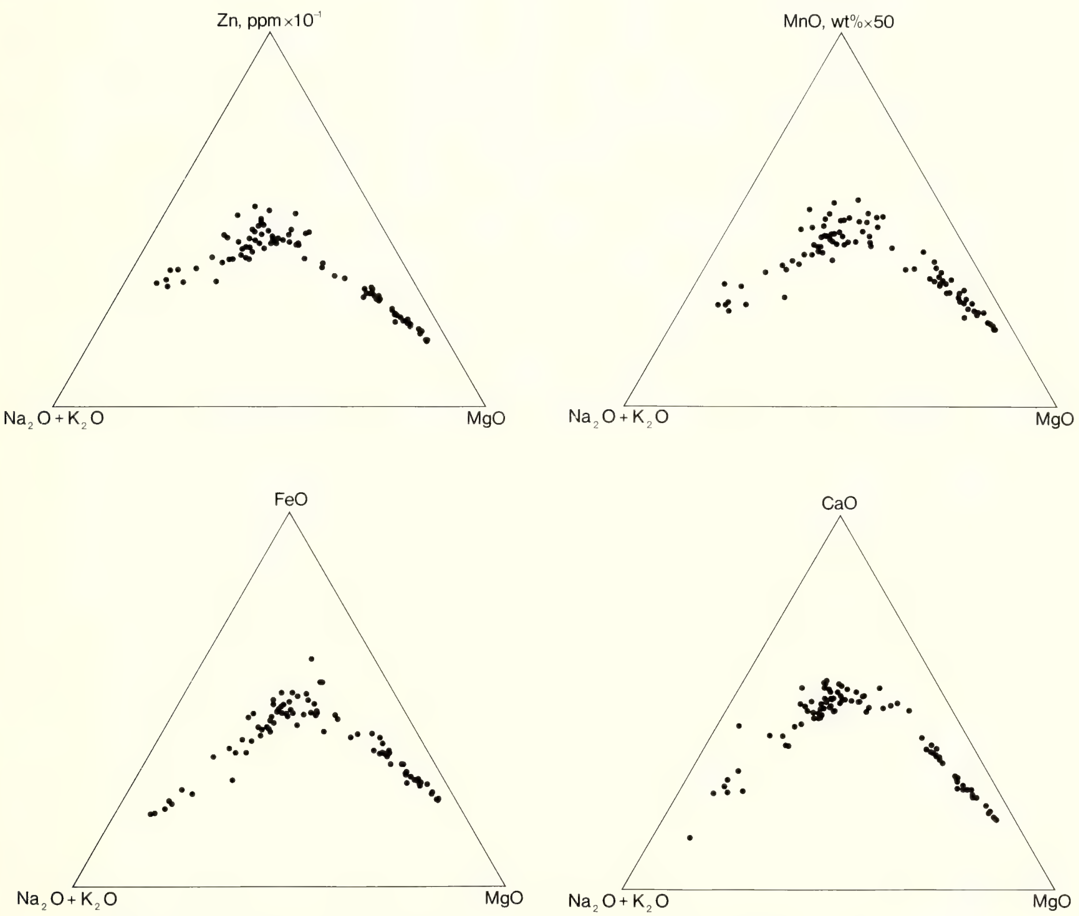


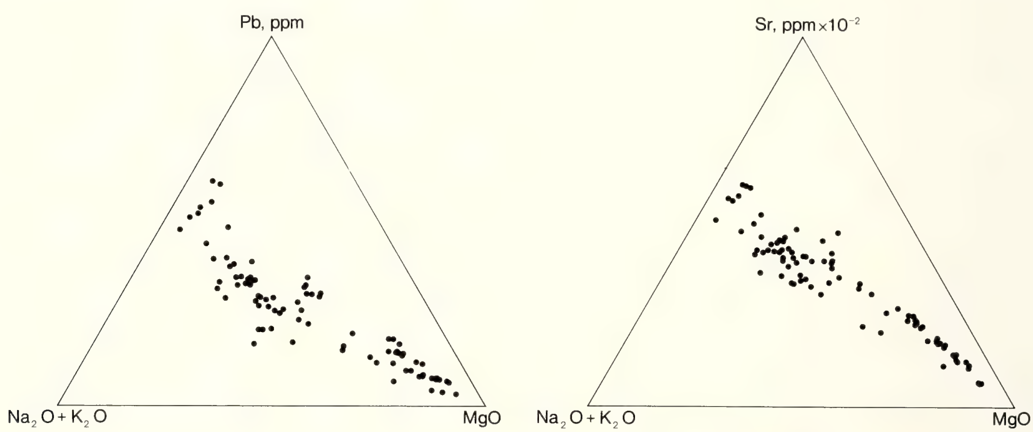
Figure 13. Ternary diagrams ("AFM" style) for Zn, MnO, total iron as FeO, and CaO in the Solomons lava suite. All exhibit the sharply kinked distribution well-known as a characteristic of (Na_2O + K_2O) - FeO - MgO relations in many mafic basalt:basalt:andesite:dacite:rhyolite assemblages.

This then brings us to the question: Are there particularly noteworthy quantities of gas given off during andesite:rhyolite volcanism? As we all know the emission of vast quantities of gas is dramatically characteristic of such eruptions. The volcanoes explode: Krakatoa, Tarawera, Santorini, Mount Lamington, Mount St Helens. The volume and probably mass of gas given off dwarfs the amount of solid, and the latter is almost entirely in the form of pyroclastic material rather than lava. It is now recognized that the world's atmosphere and oceans all derive from volcanic degassing. If our mantle basalts originally contained 1.5% water and they degassed this water completely, it would require a great deal of basalt to generate the present oceans - and those basalts would have undergone a change in composition of 100% with respect to water! A very clear corollary of this is that the mass-balance difficulties perennially encountered when trying to explain the generation of the basalt-andesite-dacite-rhyolite series on the basis of fractional crystallization alone might be obviated if the overall process involved a combination of fractional crystallization and volatile loss.

Such an idea is of course far from new; there have been many descriptions of differentiated lava sequences in which compositional variations could not be fully accounted for by partial melting or fractional crystallization, and some of the numerous investigators have suggested that gas transfer or volatile loss might be an additional factor. However the evidence now provided by volcanic stratiform ores and the apparently complementary behaviour of the ore metals in the associated volcanic rocks suggests that perhaps we should be looking at the hypothesis much more seriously. Perhaps we should be moving on from our current pre-occupation with relations between volcanic melts and their crystallizing *solids* to studies primarily directed at relations between those melts and the gases and vapours they evolve. The evidence of volcanogenic ores seems to suggest that we should.

EXHALATIVE (HYDROTHERMAL) CHEMICAL SEDIMENTATION IN THE STRATIFORM ORE-FORMING ENVIRONMENT

It is now just twenty years since the discovery of the modern metalliferous exhalative sediments of the median valley of the Red Sea (see, e.g., Hackett and Bischoff, 1973). Perhaps even more spectacular evidence of hydrothermal activity and sulphide deposition has since been discovered along the East Pacific Rise. The



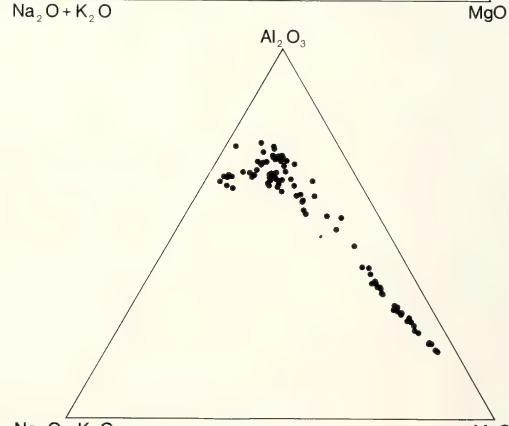


Figure 14. Ternary diagrams, analogous to those of Figure 13, for Pb, Sr and Al_2O_3. Apart from possible small inflections at the higher abundances of Pb and Al_2O_3 these distributions are, in contrast to those of Figure 13, essentially straight lines.

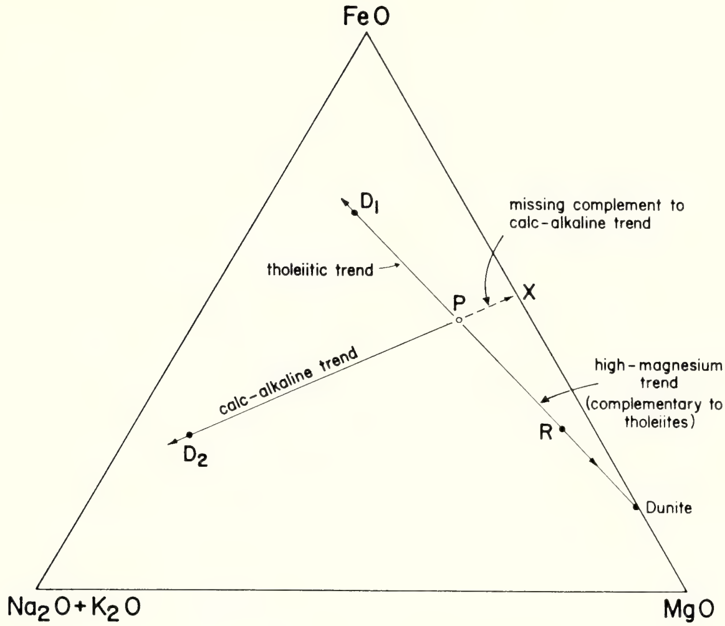


Figure 15. Numerical effects of crystal subtraction as these are manifested on standard ternary diagrams. Closed-system differentiation of parent P to differentiate D_1 and residuum R yields a linear relationship between the three. Open-system differentiation of parent P to differentiate D_2 and residuum R yields a non-linear relationship, in this case reflecting relative loss of FeO to another system. Closed system differentiation of P yielding differentiate D_2 would require the concomitant development of residua along the trend PX.

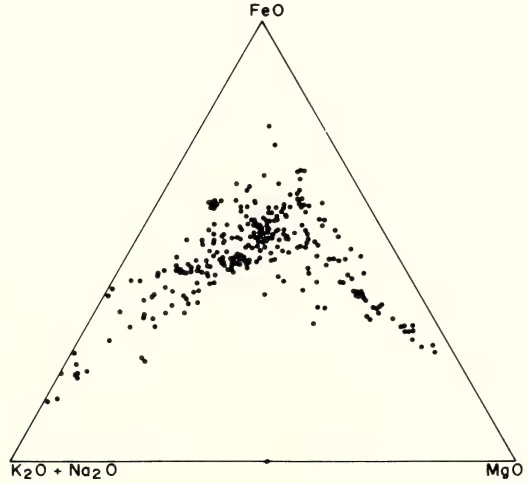


Figure 16. AFM diagram for a large sampling of modern island arc lavas, showing the conspicuous preservation of the gap corresponding to the interval PX of Fig. 15.

exhalative emission and sedimentary deposition of metal sulphide accumulations of ore grade and orebody size have now been established quite unequivocally by direct observation.

Not unnaturally these recent observations have concentrated on the sedimentary-diagenetic development of metallic sulphides, the vital components of the older stratiform ores. However what has also been observed in the hydrothermal aprons, but given little emphasis, is a variety of silicates which, like the sulphides, are products of the exhalative- sedimentary-early diagenetic process. A range of clay types, including smectites, illites and members of the kandite group, were noted from the very first observations of the Red Sea deeps and have been found in all of the seafloor hydrothermal fields discovered since. Chlorites are also ubiquitous. Observations of neoformed talc in hydrothermal aprons have been made by Spiess and his co-workers (Spiess *et al.*, 1980) in hydrothermal sediment of the East Pacific Rise and by Lonsdale and others (Lonsdale *et al.*, 1980) around seafloor vents in the Guaymas Basin in the Gulf of California. Hekinian and his co-investigators (Hekinian *et al.*, 1980) also sampling on the East Pacific Rise, found neoformed muscovite crystals up to 2.0 mm across in the hydrothermal oozes. These investigators emphasized that a chemical rather than a detrital origin was indicated by the angular shape and well-preserved outlines of the muscovite flakes, and by their inclusion within the newly formed sulphides of the vents. There appears to be no record so far of the neoformation of biotite in areas of seafloor hydrothermal activity though Bischoff (1973) specifically suggested that the iron-rich montmorillonite-nontronite of the Red Sea deposits might, with potassium uptake, transform to biotite. Authigenic biotite has however been found in the Eocene, oil- bearing, Green River Formation of western U.S.A. (Milton, 1971). Neoformed garnets have now been found in modern seafloor hydrothermal oozes of the southwest Indian Ocean, the Caribbean, and the Red Sea (Zierenberg and Shanks, 1983). The Indian Ocean garnets occur as a major constituent of a micrite, about 18 m thick, consisting of approximately 80% calcite, 20% garnet. The garnet is therefore not by any means a trace constituent - it is a major component of a substantial thickness of sediment. The authigenic development of amphibole and pyroxene has been established in the Green River Formation and in the Eocene petroleum reservoir rocks of the Spencer Formation of Oregon (Enlowes and Oles, 1966), and recently amphiboles and pyroxenes - as crystals up to 3.0 mm long - have been found as components of the chemically precipitated assemblages of the Red Sea hydrothermal sediments (Zierenberg and Shanks, 1983). Associated major minerals in these chemical oozes are euhedral albite, quartz, haematite, magnetite and anhydrite, and accessories are fine-grained smectite, talc, chlorite, sphene and andradite garnet. All appear to have grown together as products of chemical sedimentation and early authigenesis.

In this general connection it is interesting to recall that in a series of experiments carried out at Harvard in 1966, J. Ito and C. Frondel precipitated hydrogarnets by mixing $CaNO_3$, $FeCl_3$ and Na_2SiO_3 solutions in a beaker at atmospheric pressure. This was published in the *American Mineralogist* in 1967 (Ito and Frondel, 1967), but excited little or no interest. Currently my research assistant, Mrs W.P.H. Roberts, and I are in the process of extending these experiments.

All of this indicates that we may now be at a point in our observation and understanding of exhalative silicates where we were in our observation and understanding of exhalative sulphides some twenty years ago - i.e. at the time when we were just beginning to perceive the existence and significance of the Red Sea metalliferous brine deposits.

To this point in our current seafloor observations we have been largely pre-occupied with the exhalative sulphides, and of course it has been important for us to be so from the point of view of ore genesis theory. Perhaps, however, we should now be hastening to direct our observation to the exhalative silicates. This other, less conspicuous, component of the exhalative regime may have profound implications for petrology in a much wider sense.

REGIONAL METAMORPHIC PROCESSES: THE POSSIBLE SIGNIFICANCE OF STRATIFORM ORES IN THE INTERPRETATION OF REGIONAL METAMORPHIC MINERAL ASSEMBLAGES

These recent observations of the present-day formation of exhalative silicates to which I have just referred bring us naturally to a consideration of the metamorphic mineral assemblages that commonly accompany metamorphosed stratiform sulphides. Such assemblages may have important implications in exploration for stratiform ores in metamorphosed terrains and in the interpretation of regional metamorphic mineralogies generally.

Many regionally metamorphosed stratiform ores are characterised not only by the localised development of sulphides, and one or more of exhalative chert, carbonate, sulphates, fluoride, phosphate and borate but also by an almost equally localised development of metamorphic silicates (Fig. 1). These silicate assemblages, which may range from the simple abundant development of magnesian chlorite to highly complex associations of many metamorphic silicates, occur in close proximity to, and within, the sulphide lens. Like the associated sulphides, carbonates, barite and so on, the silicates are stratiform in their disposition and variations in silicate assemblages, as with the sulphide assemblages, are related to bedding (Fig. 17).

Recently, very detailed microscopical and electron microprobe studies of some of these metamorphic silicates of stratiform ores (e.g. Stanton and Williams, 1978; Vaughan, 1980; Stanton, 1982) have revealed several consistent and striking features:

(1) The composition of a single mineral species, e.g. garnet, may vary greatly from one grain to the next over distances of a small fraction of a millimetre. In most cases this is related to bedding and indicates metamorphic

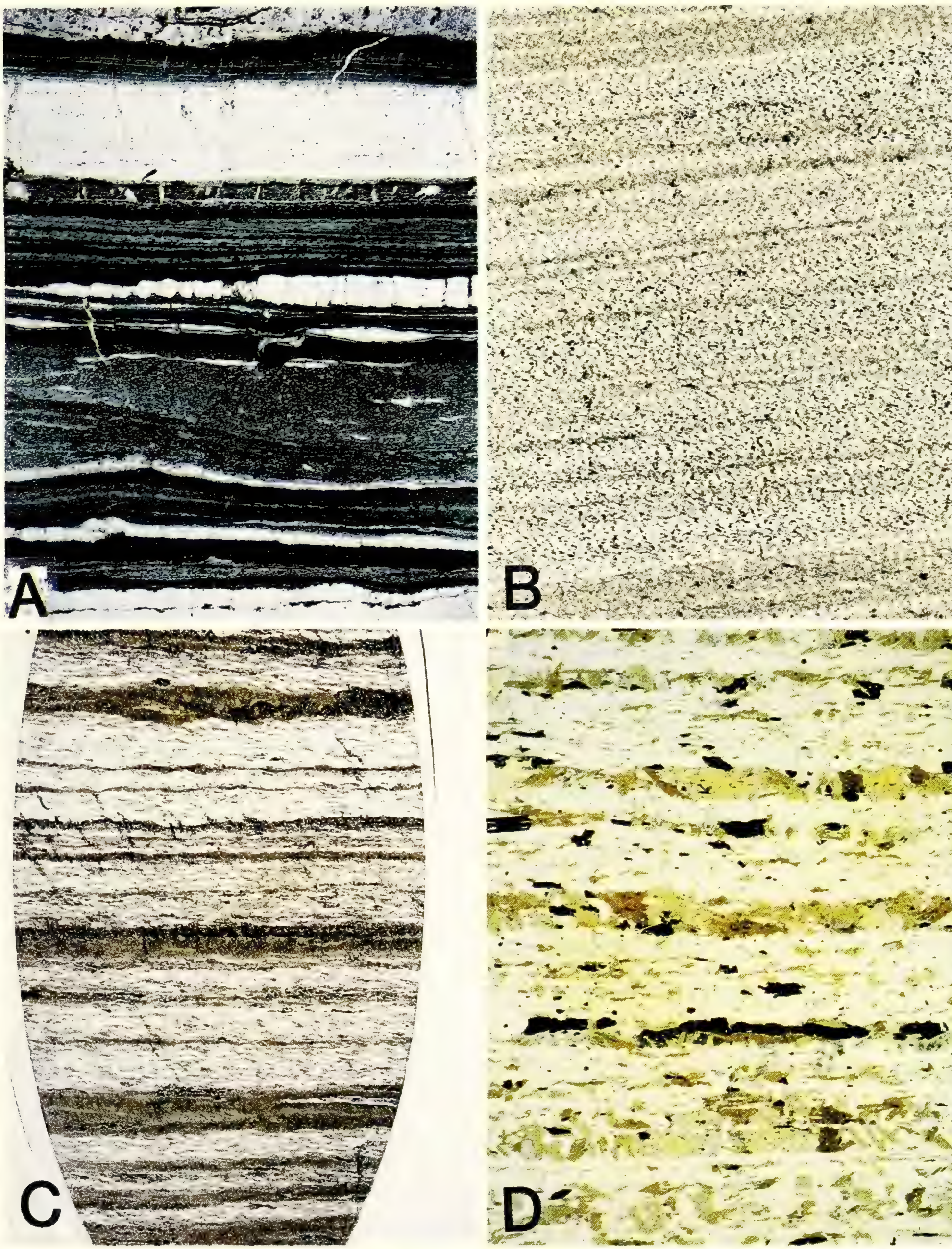


Fig. 17. A. Chlorite in bedded arrangement, Golden Grove stratiform Cu-Zn deposit, Archaean of Western Australia; B. bedding preserved in garnet-quartz assemblage of "garnet-quartzite", Main Broken Hill Pb-Zn lode, Lower Proterozoic, NSW; C. pyroxene-dominated assemblages (dark layers) interbedded with quartz-dominated assemblages, Oonagalabi stratiform Cu-Zn deposit, Lower Proterozoic, Northern Territory; D. Biotite-staurolite-garnet-assemblages interbedded with quartz-dominated assemblages, Gorob stratiform Cu-Zn deposit, South West Africa.

disequilibrium on a very fine scale (Stanton and Williams, 1978).

(2) Zoning patterns in minerals such as garnet may be quite different from one fine bed to the next - again on a scale of a fraction of a millimetre, and again indicating that metamorphic equilibrium has not been attained over these very small distances.

(3) Such compositional variations in silicates are commonly related to the incidence of sulphide or particular sulphides: chlorites associated with sulphide may be more magnesian, garnets more manganese and calcium-rich, than their counterparts outside the ore zone. Phyllosilicates associated with stratiform pyritic lead-zinc ores are usually iron-poor, whereas phyllosilicates occurring in adjacent, or interbedded, pyrrhotitic copper ores are usually iron-rich. Examples of prominent and systematic ties between silicate assemblages and compositions and associated sulphide assemblages and compositions are commonplace among metamorphosed stratiform ore deposits.

(4) Mineral assemblages are commonly complex on a fine scale and prograde chlorite, muscovite, biotite, almandine, staurolite and kyanite (or sillimanite) may be found together within a single bed and within a single microscope field. There is no evidence that such assemblages reflect a prograde - retrograde history: they appear to constitute, quite simply, particularly extensive prograde assemblages - groupings that might be attributed to disequilibrium in terms of current metamorphic theory.

(5) The metamorphic silicate of the ore and its environs is commonly quite out of keeping with that of the immediately surrounding terrain, indicating a larger-scale metamorphic disequilibrium. A number of stratiform pyritic gold deposits of the Archaean of Canada and Western Australia, for example, are enclosed in provinces of rocks of greenschist grade, but are themselves characterized by highly localised, concentrated assemblages of quartz, sulphides, prograde chlorite, muscovite, biotite, garnet, cordierite, K-feldspar, andalusite and sillimanite. The general phenomenon involves a wide variety of silicates, but is common in metamorphosed stratiform ore provinces all over the world. Each case represents the development of a minute volume of distinctive stratiform silicate isolated in a sea of quite different metamorphic silicates - just as the associated bedded sulphides represent the develop- ment of a relatively minute volume of ore isolated in a sea of rock relatively devoid of sulphides.

(6) The metamorphic silicate assemblages commonly change, in systematic fashion, in conformity with chemical sedimentary facies patterns developed by the oxide-carbonate-sulphide component of the orebody in question. Many examples - for instance the Aggenys-Gamsberg deposits of South Africa (Rozendaal, 1982; Ryan *et al.*, 1982), Geco (Friesen *et al.*, 1982) and Bathurst (McAllister, 1960) in Canada and Pegmont in Australia (Stanton and Vaughan, 1979) - range from oxide-rich banded iron formations through sulphide-silicate-carbonate to massive sulphide facies. In each case the metamorphic silicate assemblages change systematically in sympathy with the facies pattern developed in the ore body.

Taken altogether these features seem to indicate that the individual silicate compositions and assemblages are not manifestations of extensive equilibrium. Rather they seem to suggest that the silicates are essentially direct products of the ore-forming processes - that is, of sedimentary processes - down to a very fine scale, and that the resulting sedimentary features have remained essentially unmodified by subsequent metamorphism.

To say that such sedimentary features "have remained essentially unmodified by subsequent metamorphism" is a euphemistic way of stating that such metamorphism has not involved significant movement of the relevant elements; that, on a scale of a small fraction of a millimetre, it has been chemically static. If it has been static, the present composition of any given small domain must closely represent the composition of that domain prior to metamorphism. That is, the present metamorphic silicates - like the accompanying sulphides, carbonates, sulphates, fluorides and so on - must arise directly through the simple coarsening of sedimentary/diagenetic silicate of the same kind, or through the in situ solid:solid transformation of a sedimentary/diagenetic precursor material of similar composition.

Let us examine the problem in a more homely way...... There is now no question that the sulphides of these exhalative ores were deposited as sediments. There is also no question that they have been subjected to regional metamorphism and are now metamorphic minerals. Thus, for example, now-observed layers of metamorphic sphalerite are derived from pre-existing beds of fine exhalative zinc sulphide; the latter was probably precipitated as the low temperature hexagonal wurtzite form which with burial, heating and eventual metamorphism transformed *in situ* to the cubic dimorph sphalerite and then coarsened progressively to its present grainsize. No ore petrologist now doubts this simple, general sequence of events. Similarly the stratiform pyrite, barite, sideritic and manganoan carbonate, stratiform chert layers, fluorite, apatite and anhydrite as abundantly found as components of metamorphosed exhalative-sedimentary ores, are in their present form metamorphic minerals. However all are clearly recognized as having derived directly, by isochemical transformation and coarsening, from sedimentary materials of the same composition. Where the sphalerite, pyrite, carbonate and other minerals show subtle variation in composition from one bed to another, we recognise that this simply reflects variations in original sedimentary compositions and the isochemical nature of subsequent metamorphism. A critical point here is that we know the original sedimentary and present metamorphic compositions of these minerals and the essentially isochemical nature of their metamorphism is self-evident and unavoidable. If *these* minerals are the products of isochemical metamorphism, surely there must be a strong likelihood that the intimately associated silicate minerals are too.

We may look at the problem in yet another way. ... Two very well-recognised modes of ore formation are:

the one to which attention has been directed throughout this lecture - exhalative chemical sedimentation - and the other, contact metamorphism. In the case of contact metamorphic ore formation, already lithified rocks, usually carbonate-bearing, are intruded by a felsic melt which heats and modifies the rocks and transfers metallic sulphides to them. Intruded carbonates and associated sediments adjacent to the contact are heated, sintered and coarsened; impurities in the carbonate, and new compounds introduced from the intrusive, combine with the carbonate to form garnet, amphibole, pyroxene, forsteritic olivine and a wide variety of other minerals by metamorphic reactions, A variety of metallic sulphides, for the most part introduced from the de-gassing intrusion, are deposited by metamorphic reaction and metasomatic replacement.

Thus in the case of contact metamorphic ore formation sulphides and silicates (disregarding other species for the moment) are formed in a dynamic system of metamorphic reaction and metasomatic replacement.

In the case of the regionally metamorphosed exhalative ores, on the other hand, the sulphides have not been formed by metamorphic reaction and metasomatic replacement at all. The idea that they were deposited by replacement was cast aside some twenty five years ago and has been finally disposed of by the modern seafloor observations. It is now established that they were formed by earlier chemical sedimen- tation and, much later, simply passively involved in regional metamorphism. However, while changing our ideas on stratiform *sulphide* formation we continue to adhere to the earlier views on stratiform *silicate* formation and persist in extrapolating from contact metamorphic principles. Are the stratiform silicates quite different in their formation from the sulphides, and formed by metamorphic reactions, or are they, like the sulphides, sedimentary materials that have simply been passively involved in regional metamorphism? Once again it would be obtuse not to at least suspect the latter.

To sum up, the intimate association of the metamorphic silicates with the sulphides and other bedded chemical products such as barite, carbonate and chert, anhydrite, apatite and fluorite; the close relationships between sulphide assemblages and silicate assemblages; close relations between sulphide and silicate compositions; the identical or analagous nature of silicate and sulphide microstructures; and the common tie between silicate assemblages and compositions with bedding and small-scale sedimentary facies patterns, all suggest similar and parallel histories for sulphide and silicate. This indicates that the stratiform metamorphic silicates, like the accompanying stratiform metamorphic sulphides, have developed directly from sedimentary/diagenetic products - a deduction now supported by observation of these silicates in modern hydrothermal-sedimentary environments.

The mechanisms of development of the stratiform metamorphic silicates as we now observe them are therefore probably two: *firstly*, direct derivation by the coarsening of their fine sedimentary/diagenetic counterparts, as in the case of sulphides, and *secondly* by the isochemical transformation of sedimentary/diagenetic precursors, as for example in the case of anhydrite derived from gypsum and apatite derived from fine sedimentary phosphates. In the first case - simple coarsening of sedimentary/diagenetic minerals - we now know from direct observation of modern seafloor hydrothermal sediments that chlorite, talc, muscovite, garnet, amphibole and pyroxene can form by direct precipitation and early authigenesis in these environments. In the second case, among potential precursors the composition of a siliceous chamosite is virtually identical with that of almandine garnet (Figs. 18 and 19), With potassium uptake glauconites and iron-rich montmorillonites may well transform to biotite as suggested by Hackett and Bischoff in 1973. The aluminous clay-chlorite sudoite, a common hydrothermal alteration product of calc-alkaline volcanic rocks associated with the Kuroko ores of Japan, has a composition almost identical with that of cordierite (Stanton, 1984). I have recently found that a common alteration product of olivines in some basaltic andesites has compositions and computed structural formulae identical with those of Fe-Mg metamorphic amphiboles. There is now good evidence to suggest that commonly occurring materials such as mixed-layer kaolinite-gibbsite - in some cases derived from earlier partial degradation of detrital biotite incorporated in the original sediments - may constitute precursors to the alumino-silicate sillimanite (Figs. 19 and 20). These are just a few examples of potential precursors.

If the metamorphic silicates of exhalative environments do develop along these two paths - i.e. of simple coarsening and of precursor transformation - why is it that many of the assemblages are so complex?

A moment's reflection indicates that the hydrothermal- sedimentary environment is one in which the precipitation of a wide variety of silicates, especially the clays and phyllosilicates, would be expected.. It involves the interplay of two highly contrasted regimes - on the one hand the fluctuating, evolving, hydrothermal source contributing hot, concentrated acid solutions, and on the other the slow- moving cool, dilute and alkaline environment of sea or lake floor bottom waters (Fig. 21). Not simply an interplay, but a *fluctuating interplay* of two such contrasted regimes would clearly be one from which a wide variety of precipitation among sulphides, silicates, sulphates, fluorides, borates and other compounds would be expected. Indeed it is just this fluctuating nature that is responsible for the bed-to- bed variation in the amounts and proportions of the different sulphides found in all deposits of this kind. It would, presumably, lead to bed-to-bed variation in the precipitation of chlorites, talc, muscovite, garnets, amphiboles and pyroxenes such as those recently observed in modern seafloor hydrothermal aprons. It would also be conducive to the chemical precipitation of a wide variety of precursor substances - the cherts, clays, mixed-layer clays, clay-chlorites and clay-zeolites which, with the later imposition of regional metamorphism, might transform isochemically and *in situ* to a number of the metamorphic silicates such as cordierite, staurolite and the Al_2SiO_5 polymorphs.

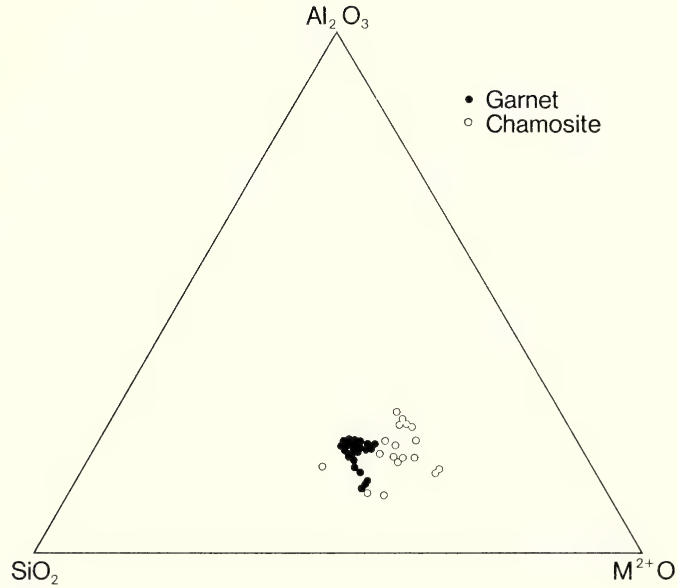


Figure 18. $MO-Al_2O_3-SiO_2$ relations in chamositic chlorites and almandine garnets from the Mount Misery stratiform Pb-Zn deposit, North Queensland. The diagram indicates that with a small addition of SiO_2, as in a "siliceous chamosite", garnet and chamosite would be essentially isochemical.

Finally, let us look at the phenomenon of zoning in regionally metamorphosed pelitic terrains.

I have already referred to the fact that many exhalative ore deposits exhibit lateral zoning of their sulphide minerals and that this lateral sulphide zoning is commonly accompanied by sympathetic lateral zoning in the associated silicates, carbonates and other non-sulphides. Such ordered, systematic lateral changes are presumably due at least largely to local, lateral changes in conditions of chemical sedimentation during ore deposition. A strikingly clear-cut instance of the related zonation of sulphides and silicates has recently been found in the Pegmont stratiform deposit of northwest Queensland (Fig. 22). This appears to have developed in a shallow basin or sea or lake floor depression and exhibits a metal sulphide zoning from lead-rich at the basin centre through zinc-rich in the intermediate facies to copper-rich at the basin edge (Stanton and Vaughan, 1979; Vaughan and Stanton, in prep.). In parallel with this the silicates are, for example, relatively Fe-rich and silica and alumina poor at the centre (abundant galena - olivine - clinopyroxene - garnet), becoming lower in Fe and higher in Al and Si in the intermediate facies (lesser sulphide - clinopyroxene - hornblende - garnet) this trend continuing to the basin edge (negligible sulphide - garnet - biotite - feldspar). This present pattern of mineralogy and whole-rock and mineral chemistry appears to reflect a faithfully isochemical metamorphism - a simple "baking in" of an original primary facies pattern of chemical sedimentation, the result in turn of a progressive change in sedimentary conditions from basin edge to basin centre.

This demonstrates what may be a very important principle in the petrogenesis of metapelitic rocks: changes in conditions of sedimentation may lead to progressive changes (zoning) in the incidence of precursor materials which, with later imposition of the physical conditions of regional metamorphism, may lead directly to systematic variation (again, zoning) in metamorphic mineral assemblages.

This shows up on a fine scale and in well-defined and rather spectacular fashion in the specialised environment of the exhalative locale. However it also appears, albeit in less spectacular and not-so-readily-seen form, in a number of larger-scale, "normal" geological environments.

It is now known that many marine shelves, inland seas such as the Caspian and Mediterranean, and saline lakes such as those of the Eocene of western U.S.A.,

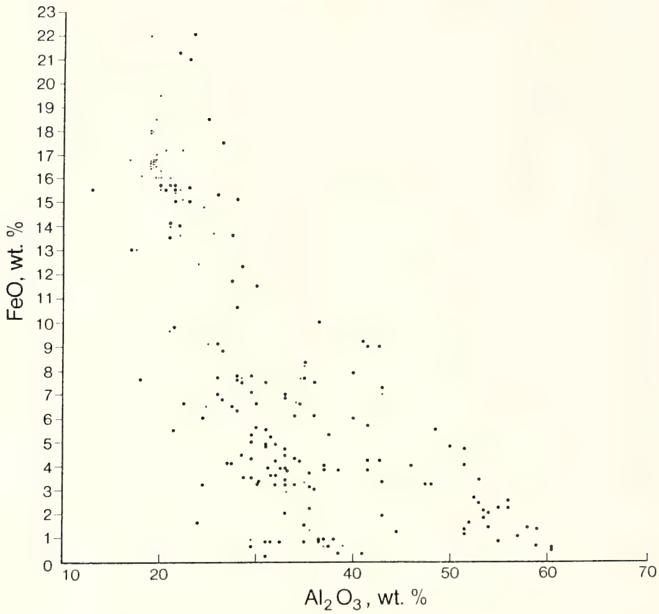


Figure 20. $FeO-Al_2O_3$ relations in electron microprobe analyses of the biotite-clay-sillimanite aggregates of Figures 19B and C. Analyses range from unaltered to increasinly degraded biotite, to low FeO- high Al_2O_3 clay-like material, finally to the fibrous material containing ca 62-63% Al_2O_3. (Circled dots indicate variably degraded, but still recognisable, biotite; plain dots indicate material from which all, or almost all, features of biotite have been lost).

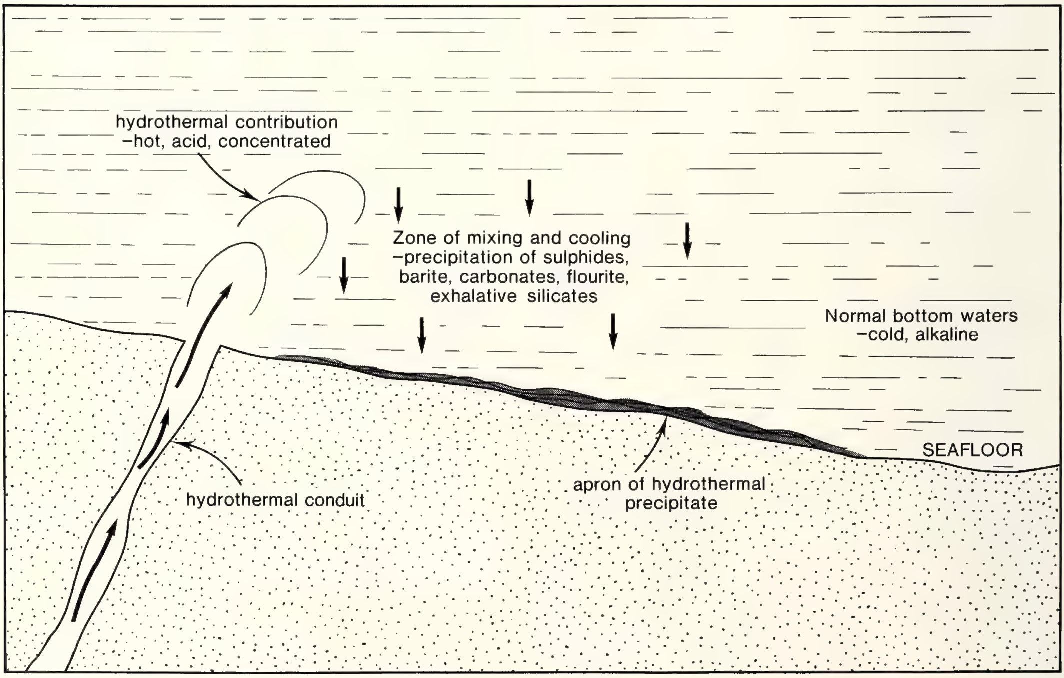


Figure 21. General nature of the highly fluctuating interplay between the hydrothermal and normal bottom water regimes occurring during hydrothermal sulphide/silicate depostion.

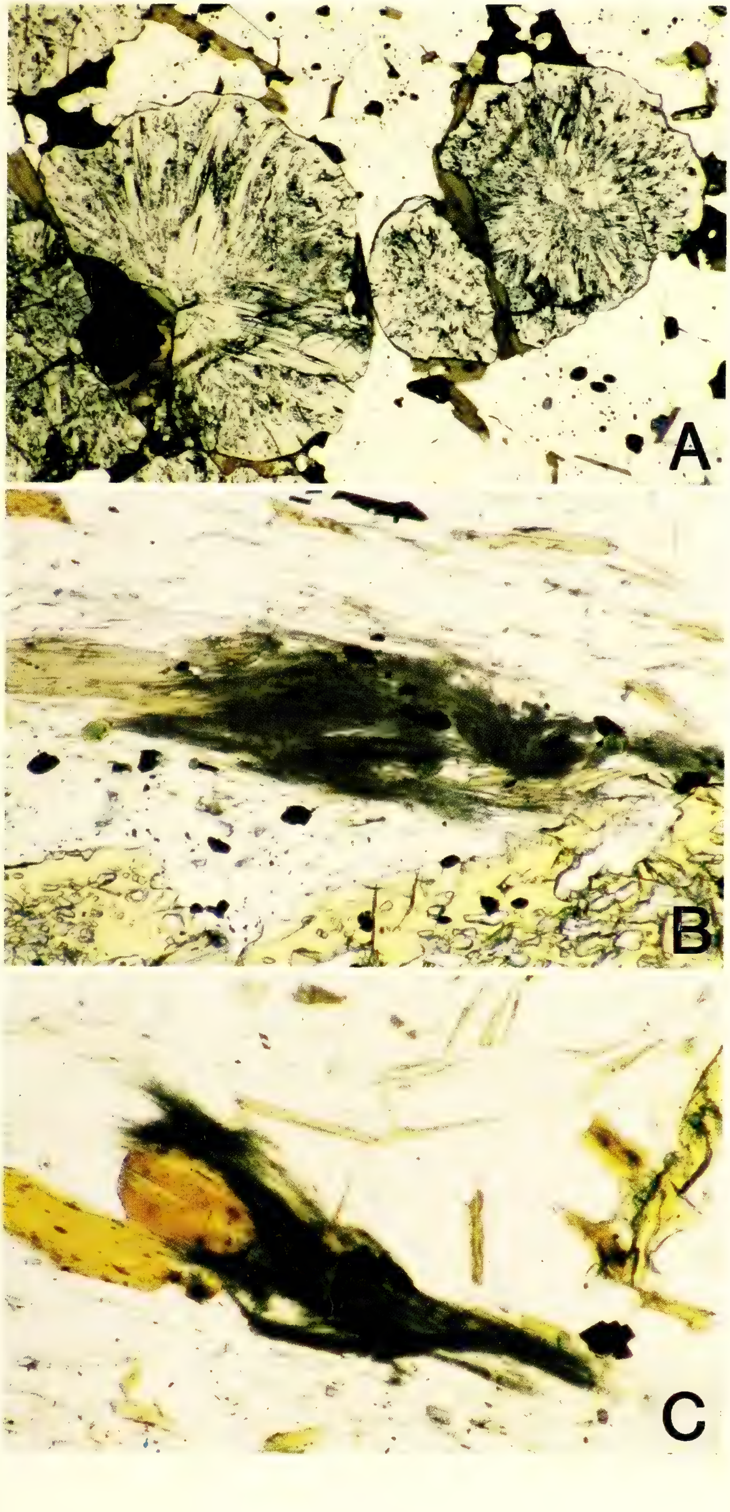


Fig. 19. A. Garnets from the Jolimont stratiform Pb-Zn deposit, Northwest Queensland, showing what appears to be relict phyllosilicate structure, preserved through the solid: solid transformation of chamositic chlorite to almandine garnet. Ordinary light, X40. These garnets were first noted at Jolimont by the author's former postgraduate student, Dr. J.P. Vaughan, and similar material has now been found elsewhere.

B. Quartz-K-feldspar-muscovite-biotite-almandine-staurolite-sillimanite metapelite from the Hemlo pyritic gold deposit of the Archaean of Lake Superior, Canada. The photograph shows a particle of biotite, partially degraded to highly aluminous (clay-like) dark material which grades out peripherally to fibrous material of sillimanite composition.

C. The same, showing more pronounced development of sillimanite fibres peripheral to the dark, highly aluminous matter.

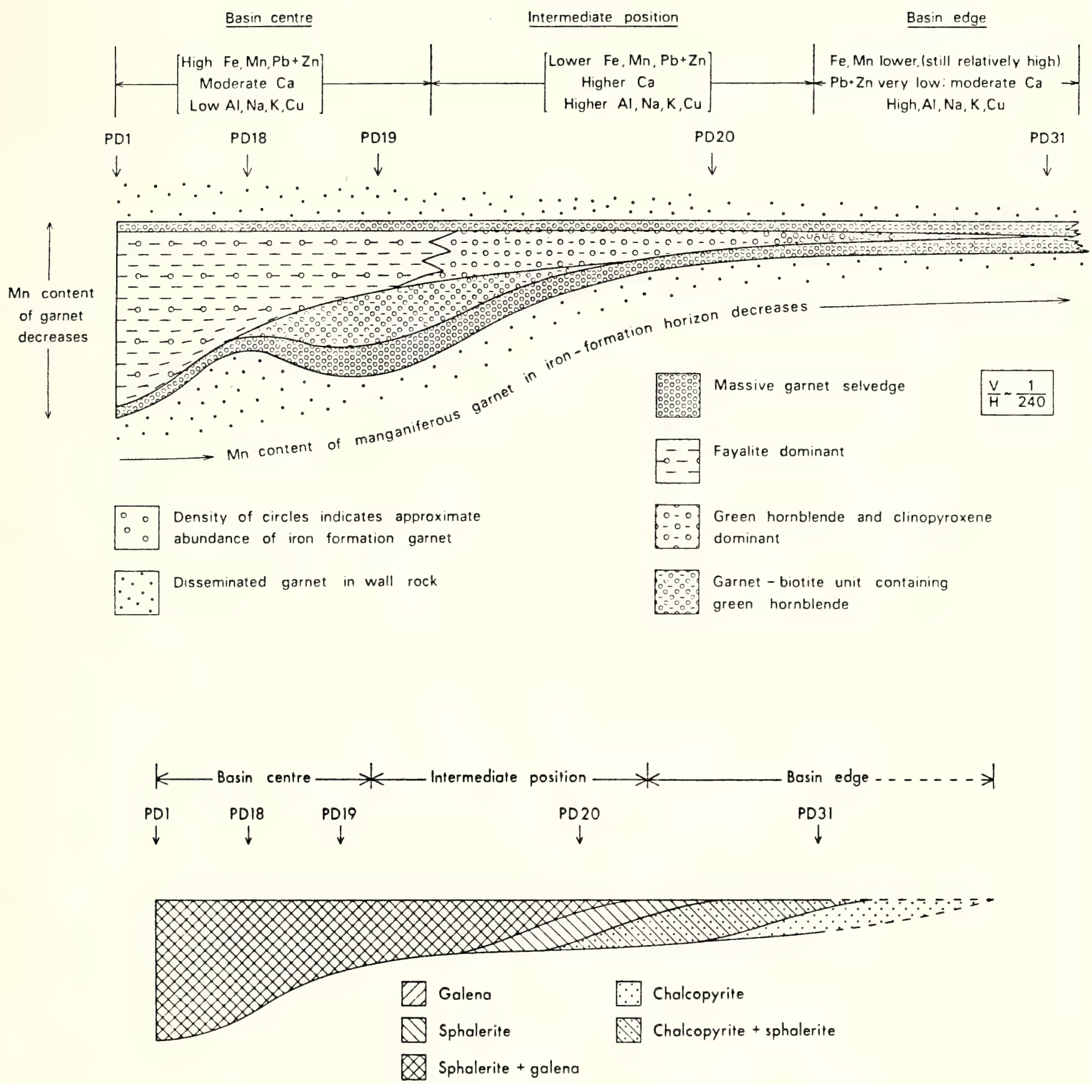


Figure 22. Silicate/sulphide zonation from basin centre to basin edge in the Pegmont stratiform Pb-Zn deposit, North-West Queensland (after Stanton and Vaughan, 1979; Vaughan, 1980).

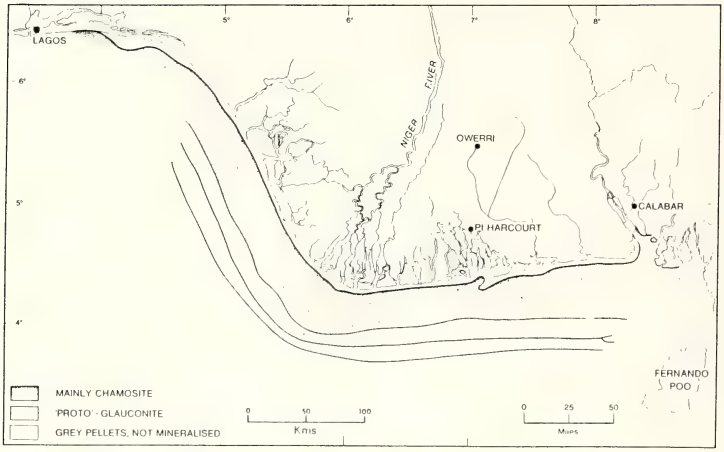


Figure 23. Sedimentary-diagenetic zoning of glauconite and chamosite (suggested precursors to biotite and garnet respectively) and associated materials in the modern sediments of the Niger delta (after Porrenga, 1967).

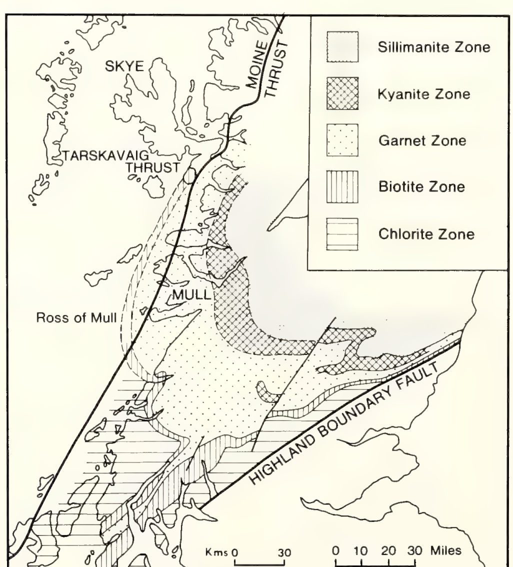


Figure 24. Zoning of metamorphic index minerals in the Dalradian of Scotland (after Kennedy, 1948). Note the general similarity of scale of these zones, and those in the modern sediments of Figure 23.

exhibit zonation of clays, mixed-layer clays, chamosites, glauconites and other phyllosilicates that seem likely to constitute specific precursor materials for the development of specific metamorphic minerals (Fig. 21). These sedimentary zonations are of regional extent, and of the same spatial scale as some of the well-known zones of regional metamorphism (Fig. 22). If we have a broad-scale zonation of such precursor assemblages, regional metamorphism must almost inevitably lead to an inherited zonation of metamorphic assemblages. Such a potential connection between sedimentary/diagenetic mineral zoning and inherited metamorphic mineral zoning is perhaps something that should be given very serious consideration when interpreting regional metamorphic mineral zonation.

The establishment of a causative tie between specific sedimentary/diagenetic mineralogies - as distinct from mere bulk chemistries - and specific regional metamorphic mineralogies such as I have suggested, might be expected to add considerably to our capacity to interpret metamorphic rocks. The clay and clay-like mineral component of shelves, lakes and inland seas reflects not only variations in local conditions of sedimentation, but also the broader configurations of shelf and lake floor, palaeogeography, climate, and the provenance of the fine detrital fraction. If, then, some of our regional metamorphic mineral assmblages are a direct reflection of earlier sedimentary/diagenetic precursor assemblages, it should be possible to use the metamorphic minerals to deduce such things as seafloor conditions, palaeoclimate and palaeogeography and sediment provenance. While current somewhat simple interpretations of metamorphic grade might have to be modified, this extended approach would add greatly to the information we could obtain from regionally metamorphosed rocks and add to our capacity to elucidate some of the more obscure, highly metamorphosed - and mineralized - Precambrian terrains. Perhaps, had George Barrow known about metamorphosed stratiform ores in 1893 (Barrow, 1893), our metamorphic textbooks may have been rather different from those we have today.

This lecture has covered a very wide field. The treatment of such a diversity of subject in a single lecture has been regrettably, but inevitably, less detailed and much less rigorous than I would have liked. It would have been much easier to have devoted the whole of the lecture to just one of the three aspects I have considered this evening. However as I said in my introduction I have tried to offer you a contribution in the spirit of Clarke. He was a great geologist in the widest sense, interested in all aspects of the science and a multiplicity of other fields pertaining to it. I have taken that part of geology that has constituted the central theme of my own scientific life - the stratiform ores of volcanic affiliation - and have attempted to show how this small aspect of the science may provide some intriguing, and perhaps eloquent, clues for the solution of some of our much greater, and more general, geological puzzles.

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Department of Geology and Geophysics,
The University of New England,
Armidale, N.S.W., 2351,
Australia.

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The Volatile Leaf Oils of Two Cultivars of *Callistemon viminalis*

J. J. Brophy, E. V. Lassak and R. F. Toia

ABSTRACT. A sum total of 52 components were detected by gas chromatographic/mass spectrometric analyses of the steam volatile leaf oils from two cultivars of *Callistemon viminalis*. Both oils were rich in monoterpenes (∿95%) with cineole as the major component (>50%) in each case. The key difference between the oils lies in the relative quantities of α-pinene, linalool and α-terpineol. Cultivar I contains a substantial amount of linalool (16%) with small quantities (∿1%) of α-pinene and α-terpineol whilst cultivar II contains significant amounts of the latter two components (18% and 12%, respectively) and only a trace amount (0.5%) of linalool.

INTRODUCTION

Many species of the Australian genus *Callistemon* (Myrtaceae), the so-called "bottlebrushes," are noted for their attractive red flowers and are thus in great demand as ornamental shrubs. Given the commercial importance of this family, it is perhaps surprising that the leaf oils have only received scant attention.

In an early report on the composition of the volatile oils from *C. viminalis*, detected *d*-α-pinene, limonene, dipentene, terpineol, phenols and sesquiterpenoids were detected along with major amounts of 1,8-cineole (Penfold, 1923). Similarly, the oil from *C. lanceolatus* was found to contain dipentene, limonene, α-terpineol, 1,8-cineole, phenols and sesquiterpenes (Penfold, 1923). In a later investigation on this latter species, the leaf oil was recognized to have fungistatic activity (Pandey *et al.*, 1982). *C. rigidus* has also received some attention, with 1,8-cineole being identified as the major component (Takemoto and Yahagi, 1955) whilst *C. speciosus* has been found to yield an oil, again with major amounts of 1,8-cineole (81.5%) but with α-pinene, limonene, myrcene, α-terpinene, α-terpineol and caryophyllene also being identified (Wasicky and Saito, 1972).

In this paper we present the results from a detailed examination of the steam-volatile leaf oils of two cultivars of *C. viminalis*.

EXPERIMENTAL

Collection of Plant Material and Isolation of Volatile Oils

Fresh foliage from two cultivars of *C. viminalis* (Sol.) Cheel growing at Randwick, N.S.W. was steam-distilled as previously described (Lassak, 1979) to yield pale yellow mobile oils. For cultivar I, the oil was obtained in 1.8% yield (n_D^{20}= 1.4648; α_D^{24} = + 0.894°) whilst for cultivar II "James Cook" a 1% yield (n_D^{20} = 1.4665; α_D^{24} = + 10.933°) was obtained.

Botanical voucher specimens have been lodged at the National Herbarium, Royal Botanic Gardens, Sydney.

Identification of Oil Components

Analytical GLC was conducted on a) a Perkin Elmer Sigma 2B chromatograph using a 50m x 0.2mm i.d. FFAP coated fused silica column and b) a Shimadzu GC 6 AMP chromatograph using a 70m x 0.5mm i.d. FFAP coated SCOT column with He as carrier gas in both cases. Individual runs were programmed a) from 80°C to 170°C at 6°/min following an initial holding period of 9 min at 80°C and b) from 80°C to 230°C at 3°/min. Individual components were tentatively identified by their retention times and by co-injection with authentic compounds. A Perkin Elmer Sigma 10B Chromatography Data Station was used to determine percentage compositions.

GLC-MS were determined using a Shimadzu chromatograph as described under b) above interfaced to an AEI MS12 mass spectrometer through an all-glass straight split with He as carrier gas. The gas chromatograph was programmed from 70 C to 230 C at 3 /min while the mass spectrometer was operated at 70 eV with the ion source at 180 C. Spectra were acquired every 6 seconds and processed by a VG Digispec Display data system which produced standard bar graphs for direct comparison with published spectra (Heller and Milne, 1978, 1980; Stenhagen *et al.*, 1974).

TABLE 1

% Composition of *Callistemon viminalis Oils*

Peak No.	Compound	Cultivar No. I	Cultivar No. II
1	α-pinene	0.7	18.0
2	camphene	tr	0.1
3	β-pinene	0.8	1.0
4	sabinene	0.5	-
5	myrcene	1.8	0.3
6	α-phellandrene	0.2	0.1
7	limonene	2.5	5.4
8	1,8-cineole	64.2	48.7
9	γ-terpinene	0.6	0.3
10	*p*-cymene	0.2	0.8
11	terpinolene	0.2	0.2
12	α-cubebene	tr	tr
13	$C_{10}H_{18}O$	tr	tr
14	linalool	16.0	0.5
15	pinocarvone	0.1	tr
16	terpinen-4-ol	1.6	0.9
17	caryophyllene	0.4	0.2
18	aromadendrene	-	0.1
19	β-terpineol	0.1	-
20	*iso*pinocarveol	0.1	0.3
21	δ-terpineol	0.1	0.4
22	humulene	tr	tr
23	α-terpineol	1.2	11.8
24	α-terpinyl acetate	0.2	0.3
25	$C_{15}H_{24}$	-	0.1
26	bicyclogermacrene	0.2	tr
27	$C_{10}H_{18}O$	tr	-
28	β-farnesene	0.4	0.3
29	un	0.1	-
30	$C_{15}H_{24}$ + $C_{10}H_{16}O$	-	tr
31	β-phenylethyl acetate	0.1	-
32	un	tr	-
33	$C_{15}H_{24}O$ (?)	tr	tr
34	geraniol	0.4	tr
35	$C_{15}H_{24}O$	tr	tr
36	$C_{15}H_{26}O$	tr	tr
37	caryophyllene oxide	0.1	0.3
38	$C_{15}H_{24}O$	tr	0.2
39	$C_{15}H_{24}O$	tr	0.1
40	$C_{15}H_{23}O(OH)$	-	1.0
41	$C_{15}H_{26}O$	0.1	0.4
42	$C_{15}H_{26}O$	tr	tr
43	globulol	0.3	0.9
44	$C_{15}H_{26}O$	0.2	0.6
45	$C_{15}H_{24}O$	tr	0.1
46	$C_{15}H_{24}O$	tr	0.1
47	α-eudesmol	tr	0.8
48	β-eudesmol	0.2	0.3
49	$C_{15}H_{24}O$	0.3	0.3
50	eugenol	0.3	tr
51	$C_{15}H_{24}O$	0.1	tr
52	$C_{15}H_{24}O$	0.3	tr

un = unknown tr = <0.1%

Oxidation of the Oil from Cultivar II

A sample of the oil (100 mg) in acetone was oxidised with Jones reagent (10 mins, RT). After conventional workup the oil was examined by GC under conditions described above.

Acetylation of the Oil from Cultivar II

A sample of the oil (100 mg) in benzene (1 ml) was acetylated using excess acetic anhydride and pyridine. After workup in the usual way the oil was analysed by GC under conditions described above.

RESULTS AND DISCUSSION

For both cultivars I and II (the latter being a dwarf variety of *C. viminalis* and referred to by horticulturalists as "James Cook") monoterpenes account for approximately 95% of the leaf oils, with the remainder being sesquiterpenes. In both cases, 1,8-cineole is the major component accounting for approximately 50% or more of each oil. Both oils also contain α-cubebene, δ-terpineol and β-farnesene; unusual components in that they are not normally found in myrtaceous oils. Of the total of 52 components detected, 43 were common to both oils, the remaining 9 generally being observed only in trace amounts.

Closer scrutiny of the composition of the oils shows that whilst they are qualitatively similar, there are also appreciable quantitative differences (Table I). More specifically, the other major components vary significantly between the two oils. For cultivar II, α-pinene, α-terpineol and linalool occur to the extent of 18%, 11.8% and 0.5% respectively whereas for cultivar I they are present at levels of 0.7%, 1.2% and 16% respectively.

Cultivar II aslo contains a significant amount of an unknown compound (peak No. 40), the MS of which shows ions at m/z (%): 236(25), 166(100), 151(20), 123(35), 70(15), 69(15), 43(30) and 41(32). It is well established that oxygen atoms in ether linkages do not significantly influence GLC retention times, (e.g. 1,8-cineole $C_{10}H_{18}O$, limonene $C_{10}H_{16}$and β-phellandrene $C_{10}H_{16}$ have very similar retention times on FFAP and Carbowax 20M coated columns (Shimizu, 1974; Brooker and Lassak, 1981); the same applies to linalool $C_{10}H_{18}O$ and the two furanoid *cis- and trans*-linalool oxides $C_{10}H_{18}O_2$) (Brooker and Lassak, 1981; Klein *et al.*, 1963). It is reasonable then to suggest that the unknown giving rise to peak 40, which has a retention time in the range of the mono-oxygenated sesquiterpenoids, is a hydroxy ether of formula $C_{15}H_{23}O(OH)$. Further, the hydroxyl functionality may be assigned as 3°, since this component was not oxidised on treatment of the oil with Jones reagent, nor was it readily acetylated.

The co-occurrence of 1,8-cineole with, at times, substantial amounts of linalool suggests a similarity with the leaf oils of certain *Melaleuca* species such as *M. quinquenervia* (Cav.) S.T. Blake (formerly referred to as *M. viridiflora* Gaertn.) (Hellyer and McKern, 1956) and *M. ericifolia* Sm. (Baker and Smith, 1922; Penfold and Morrison, 1936). *Callistemon* and *Melaleuca* are very close botanically (P.F. Lumley, personal communication) and it is noteworthy that Byrne suggested the placement of *C. viminalis* in *Melaleuca* (Byrne, 1984). It may also be significant that *trans*-nerolidol, characteristic of *M. quinquenervia* leaf oils (Hellyer and McKern, 1956), appears to be completely absent from both of the *C. viminalis* oil samples investigated by us.

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Department of Organic Chemistry,
University of New South Wales,
Kensington, N.S.W., 2033 Australia.

Joseph J. Brophy,
Robert F. Toia.

Phytochemical Services
P.O. Box 27
Berowra Heights 2082

Erick V. Lassak

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?Permian Palaeokarst at Billys Creek, New South Wales

R. A. L. OSBORNE AND D. F. BRANAGAN

ABSTRACT. Sandstone bodies within Silurian limestone at Billys Creek are interpreted as being palaeokarst deposits and are likely to be of Permian age.

INTRODUCTION

The character of the pre-Permian surface on which the Sydney Basin rocks were deposited has been a matter of interest to workers for many years and is discussed by Herbert and Helby (1980, pp.5, 21-24, 85-87, 519).

This surface, along the western margin of the Basin, remained largely exposed during Permo-Carboniferous times when glaciation occurred. Osborne (1984a) proposed that during this period of subaerial exposure karstification may have occurred, and that evidence for such karstification might be recognisable in limestones at Jenolan Caves, Bungonia Caves, Billys Creek, Brogans Creek, Church Creek and Portland, all of which lie at the edge of the Sydney Basin.

Massive Silurian limestone, probably attributable to the Cobra Formation (Scheibner, 1973), at Billys Creek, 7 km west of Yerranderie in Kanangra-Boyd National Park (Fig. 1), crops out at an elevation of approximately 600 m. The lowermost beds of the Sydney Basin Permian succession, the Snapper Point Formation, exposed less than one kilometre away, extend down to 690 m.

Relief on the base of the Permian along the southwest margin of the Sydney Basin is up to 600 m (Herbert, 1972) allowing the present surface of the limestone to be less than 90 m. from the Permo-Carboniferous landsurface. The limestone body at Colong Caves, only 4 km south of Billys Creek, is directly overlain by the Sydney Basin succession at its southern end at an elevation of approximately 750 m (Fig.1B) (Rose, 1961).

THE PALAEOKARST

McElroy and Rose (1961), when investigating the limestone in connection with proposed quarrying, noted the presence of small sandstone "inclusions" within the limestone towards the northern end of the outcrop north of Billys Creek (G.R. 355226 Yerranderie 8929-4-N), which is close to the point of highest elevation on the limestone.

Investigation of these "inclusions" has shown them to have a complex outcrop pattern indicative of palaeokarst. Osborne (1984b) figured one of these inclusions (Fig. 3) as an example of the shape of unconformities that exist between cave sediments and bedrock. In this case a small, roughly circular body of sandstone is completely enclosed within massive limestone. Another small sandstone body (Fig. 2) further demonstrates this relationship.

The largest sandstone body crops out over a length of 50 m striking at 160°, oblique to the elongation of the limestone body which has an average strike of 035°. Its boundaries with the limestone are

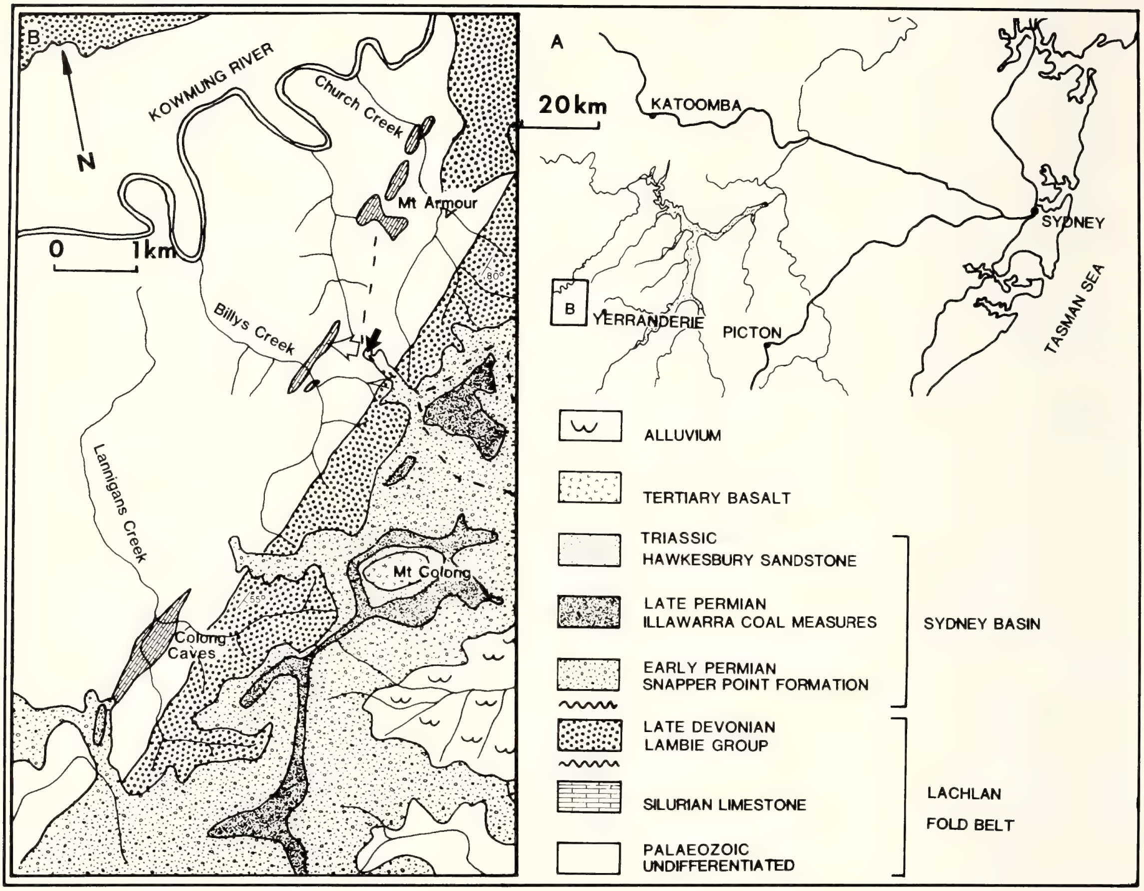

Figure 1. The Billys Creek area. Palaeokarst locality indicated by open arrow. Black arrow shows sample locality for sandstone illustrated in Figure 8. Geology partly after Rose (1961, plate 1), Upper Devonian after Powell and Edgecombe (1978, fig. 8).

irregular, but everywhere sharp. It develops a maximum width of 6 m, and it can be traced down an irregular sloping surface of limestone for a vertical distance of some 6 m to a well defined base. Towards the base grainsize increases and clasts within the sandstone are noticeably more angular. A number of smaller sandstone bodies parallel the large one (Fig. 4) forming a complex zone of infilling.

The outcrop pattern and sharp boundaries between these sandstone bodies and the limestone suggest that they are not coeval sand lenses within the Silurian succession, but rather are infillings in caves developed after the limestone had attained its present attitude.

Structural data support this interpretation. The largest sandstone body contains well-developed joints dipping at 12° to 095° and 72° to 040° which do not penetrate the adjacent limestone.

Regional cleavage is prominent in the presumably Silurian slates and sandstones immediately surrounding the limestone, with strike varying from 040° to 055°. Evidence of this cleavage is less apparent in the limestone but where visible strikes 165° to 170°.

With the exception of one locality the sandstone masses within the limestone are massive. The exception is at the basal edge of the largest sandstone body where a foliation is developed for several metres, striking 205° through a coarse gritty sandstone.

This zone of possible cleavage, at a distinct angle to the regional trend, grades upwards into sandstone which shows no sign of cleavage.

This restricted zone of contortion seems to be the result of local adjustment between the sandstone varieties and the surrounding limestone.

PETROGRAPHY

Whitworth (1961) described the sandstone collected by McElroy and Rose (1961) as being composed essentially of sub-rounded to angular grains of quartz and chert with a mainly carbonate cement, and noted that quartz grains had fused together.

Samples of the sandstone collected by the authors are similar to Whitworth's description. It is possible to recognise three sub-types: The first is a fine-grained, carbonate-cemented quartz-lithic sandstone (U.S.G.D. 62857) collected from the outcrop shown in Figure 2. This rock type is illustrated in Figure 5. The second variety (U.S.G.D. 62856) is carbonate-poor, coarse-grained quartz-lithic sandstone with sub-angular quartz and chert clasts and some ferruginous cement. This makes up the bulk of the main sandstone body (Figure 6). A third variety (U.S.G.D. 62858) from the base of the main sandstone body is quite coarse-grained and has a well developed foliation (Figure 7). In all three samples carbonate has replaced the matrix pervasively and many quartz grains have ragged edges due to this replacement. Attempts to extract spores from the sandstone have failed.

The palaeokarst sandstones are similar to basal Permian sandstones exposed less than 1 km away. This similarity can be seen by comparing Figures 6 and 7 with Figure 8 which shows a sandstone (U.S.G.D. 62859) from the base of the Snapper Point Formation on the Mt. Armour road.

Figure 2. Oval-shaped outcrop of sandstone exposed on a steep limestone face.

Figure 3. Sandstone body enclosed within limestone figured by Osborne (1984b). Note curved nature of boundary.

Figure 4. Thin elongate outcrops of sandstone that parallel main sandstone body.

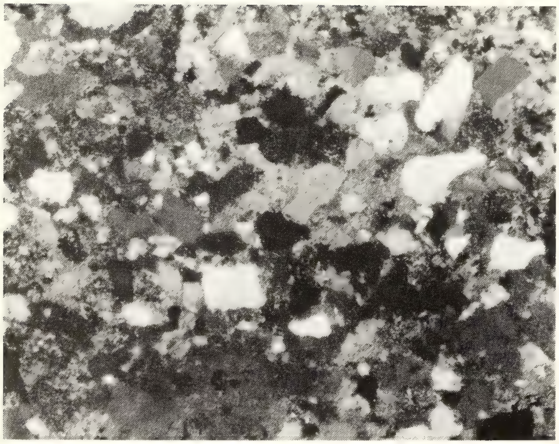

Figure 5. U.S.G.D.62857. Carbonate-rich sandstone from outcrop illustrated in Figure 2. Note extensive replacement of matrix by sparry calcite; crossed nicols, X4.

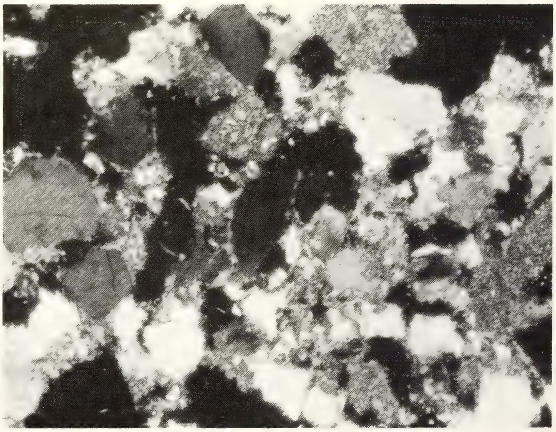

Figure 6. U.S.G.D.62856. Sandstone from upper part of large body; crossed nicols, X4.

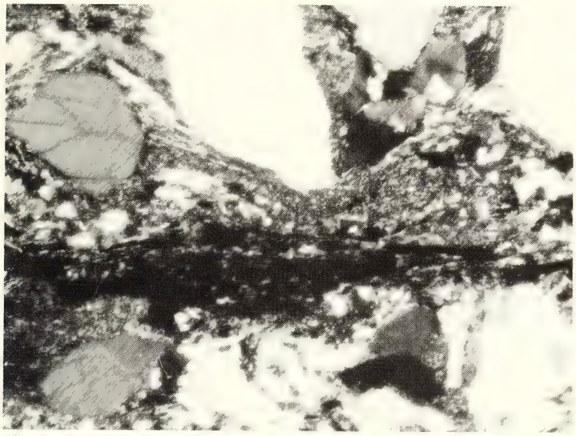

Figure 7. U.S.G.D.62858. Sandstone from towards base of large body. Note foliation of matrix running E-W in photograph; crossed nicols, X4.

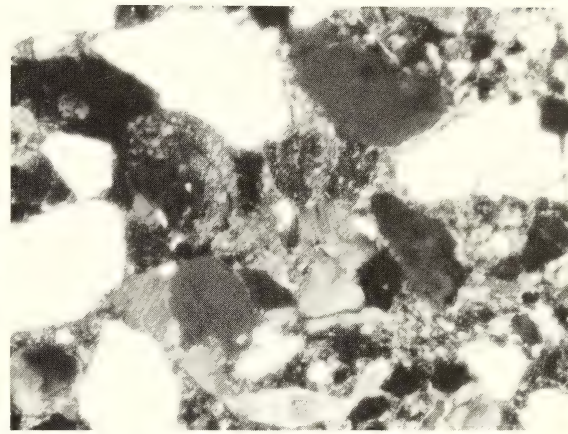

Figure 8. U.S.G.D.62859. Sandstone from basal Permian outcrop on Mt. Armour Road (locality indicated by dark arrow in Figure 1B). Note similarity in composition and texture to Figure 6 and lack of carbonate replacement of matrix; crossed nicols, X4.

While there is some similarity between the basal gritty sandstone and some Lambian (Upper Devonian) rocks, the latter have, of course, supplied some of the detritus which makes up the Permian sandstones.

AGE OF THE PALAEOKARST

Palaeokarst at Billys Creek, as represented by the sandstone bodies, could have developed during one or more of a number of occasions during the post-depositional history of the limestone. It could have formed soon after the limestone was deposited, during Mid-Devonian times prior to the deposition of the Lambie Group, during Permo-Carboniferous times prior to the deposition of the Sydney Basin or during late Mesozoic to Cainozoic times after removal of the Sydney Basin cover but prior to incision of the present streams.

An origin for the palaeokarst soon after deposition of the limestone seems highly unlikely since the sandstone bodies show no sign of the significant regional metamorphism exhibited by rocks of the Silurian sequence in the area. The sandstones apparently were deposited when the limestone had a structural attitude very similar to that which it has today. This attitude was very largely the result of deformation prior to Late Devonian times.

The Silurian sequence is unconformably overlain by the Upper Devonian Lambie Group to the east of the limestone (Powell and Edgecombe, 1978) forming the base of the Butchers Creek Syncline.

It is possible that the palaeokarst could have been formed by Upper Devonian sediment filling Middle Devonian caves with the sandstone forming the base of the Lambie Group which overlies the Silurian sequence to the east; however, this is unlikely. While Powell and Edgecombe (1978) report an angular discordance of 3^{o} to 26^{o} between the Upper Devonian Lambie Group and the underlying Silurian Sequence in the Kowmung-Yerranderie area, near Billys Creek the Lambie Group dips steeply to the southeast as is evident in Figure 1B. The Lambian surface must have a similar orientation to this steep dip. The projection of this erosion

surface would intersect the limestone some hundreds of metres above the present landsurface. Although phreatic karstification to depths of up to 2,000 m is known (Jordan, 1950), this seems an unlikely origin for vertical fissures filled with coarse clastics.

Nevertheless Conaghan (in Crook and Powell, 1976) recognised sub-Catombal Group (probable Middle Devonian) palaeokarst in the Garra Formation near Wellington in close stratigraphic/structural proximity to the Lambian unconformity there, leading Osborne (1984a) to propose that further Middle Devonian palaekarsts would be found where Lower Palaeozoic limestones are overlain by Upper Devonian sediments.

Upper Devonian palaeokarst sediments would be expected to show the effects of latest Devonian to Early Carboniferous tectonism. Powell *et al*. (1976) recognised in some places an axial-surface slaty or fracture cleavage in the Lambie Group caused they believed by regional tectonic events. As previously discussed there is no significant cleavage in the sandstone bodies discussed here and little evidence to support a Devonian age for the palaeokarst.

A late Mesozoic to early Cainozoic origin for the palaeokarst is a possibility but as with a Devonian or a Silurian origin seems unlikely. Removal of the Sydney Basin cover, in late Mesozoic to earliest Cainozoic times initiated the present period of karst and landform development. Any karst developed at that early phase of the present erosion cycle would have the form of relict karst (Jennings, 1982) rather than palaeokarst. The degree of lithification, the development of jointing and the exposure of the sandstone bodies in the highest parts of the limestone outcrop (where the present surface would constitute an early Cainozic landform) all mitigate against a Cainozoic origin for the palaeokarst.

A Permian origin for the palaeokarst sandstones at Billys Creek seems most likely. Their proximity to basal Permian sequences, lack of metamorphism and lithological similarity to adjoining Permian sandstones, strongly suggest that they are Permian in age.

DISCUSSION

The presence of possible Permian palaeokarst at Billys Creek has important consequences for the karst history of the area. Significant caves are developed at Colong, Billys Creek and at Mt. Armour-Church Creek in limestones whose topographically highest exposures are close to the level of the base of the Sydney Basin. Thus the highest outcrops of the limestones were at, or close to, the then surface during Permo-Carboniferous times.

Present surface relief on these limestones is approximately 190 m at Billys Creek, 170 m at Colong Caves, and 300 m at Mt. Armour-Church Creek placing much of the presently cavernous limestone within the depth of likely karstification prior to deposition of the Sydney Basin. It is thus possible that karst processes operating at that time may have had some influence on the present underground geomorphology of the area.

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Dept. of Geology and Geophysics,
University of Sydney, N.S.W., 2006,
Australia.

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A Note on Representation of Maxwell's Equations in a Curved Space-Time*

A. H. KLOTZ

1. Representation of Maxwell's equations

$$\operatorname{div} \underline{D} = \rho , \qquad \operatorname{div} \underline{B} = 0 ,$$
$$\operatorname{curl} \underline{H} = \underline{j} + \frac{\partial \underline{D}}{\partial t} , \qquad \operatorname{curl} \underline{E} = - \frac{\partial \underline{B}}{\partial t} , \tag{1}$$

both in Minkowski M_4 and a curved V_4 space-time of General Relativity, is well known and elementary. However, what does not seem to be usually considered is that even in the standard procedure there is a certain ambiguity. This arises in part from the fact that the duality operation

$$f^*_{\mu\nu} = \tfrac{1}{2}\surd(-g)\ \varepsilon_{\mu\nu\rho\sigma}\ f^{\rho\sigma} , \qquad f^{*\mu\nu} = \frac{}{2\surd(-g)}\ \varepsilon^{\mu\nu\rho\sigma}\ f_{\rho\sigma} \tag{2}$$

allows a way of raising and lowering of both indices of a skewsymmetric tensor, alternative to the common

$$f_{\mu\nu} = g_{\mu\rho}\ g_{\nu\sigma}\ f^{\rho\sigma} , \qquad f^{\mu\nu} = g^{\mu\rho}\ g^{\nu\sigma}\ f_{\rho\sigma} \tag{3}$$

which employs the metric tensor $g_{\mu\nu}$ (and its contravariant inverse $g^{\mu\nu}$). Again, the choice of the field tensor f (or f^*) can be made in a variety of ways leading to somewhat different conclusions.

These differences become significant in V_4 but are completely obscured in the flat M_4 where the volume element

$$\surd(-\eta) = \surd(-\det \eta_{\mu\nu}) = +1 \qquad (\eta_{\mu\nu}\text{: Minkowski metric}). \tag{4}$$

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* Communicated by D.E. Winch.

Because the transformation laws of the permutation symbols are

$$\varepsilon'^{\alpha\beta\gamma\delta} = J(x,x') \frac{\partial x'^{\alpha}}{\partial x^{\kappa}} \frac{\partial x'^{\beta}}{\partial x^{\lambda}} \frac{\partial x'^{\gamma}}{\partial x^{\mu}} \frac{\partial x'^{\delta}}{\partial x^{\nu}} \varepsilon^{\kappa\lambda\mu\nu}$$
$$\varepsilon'_{\alpha\beta\gamma\delta} = J(x',x) \frac{\partial x^{\kappa}}{\partial x'^{\alpha}} \frac{\partial x^{\lambda}}{\partial x'^{\beta}} \frac{\partial x^{\mu}}{\partial x'^{\gamma}} \frac{\partial x^{\nu}}{\partial x'^{\delta}} \varepsilon_{\kappa\lambda\mu\nu} \tag{5}$$

where $J(x,x')$ and $J(x',x)$ are the Jacobians of the inverse mappings

$$x' \to x , \qquad x \to x'$$

respectively, and

$$\sqrt{(-g)'} = J(x,x') \sqrt{(-g)} ,$$

all the two-index quantities in equations (2) are tensors. Let us, for the moment, confine ourselves to M_4.

2. The standard representation of Maxwell's equations in tensor form is fixed by choosing

$$B_k = f_{ij} , \qquad i,j,k = \text{cyclic } 1,2,3, \tag{6}$$

with $x^0 = t$) since, then, the first of the second set of equations (1) (div $\underline{B} = 0$) becomes

$$f_{23,1} + f_{31,2} + f_{12,3} = 0$$

and considering, say, the x^1 component of the remaining equations

$$0 = f_{23,0} + E_{3,2} - E_{2,3} = f_{23,0} + f_{30,2} + f_{02,3} .$$

Hence,

$$E_k = f_{k0}$$

and so, with skewsymmetric $f_{\mu\nu}$ (Greek indices going from 0 to 3),

$$f_{[\mu\nu,\lambda]} = 0 . \tag{7}$$

It is important to observe that this is a tensor equation also in V_4. In either flat or curved space (ref. 1) it implies that $f_{\mu\nu}$ is the curl of a necessarily covariant, potential four-vector

$$f_{\mu\nu} = \Phi_{\nu,\mu} - \Phi_{\mu,\nu} . \tag{8}$$

If the tensor density

$$\mathfrak{f}^{\mu\nu} = \tfrac{1}{2}\varepsilon^{\mu\nu\rho\sigma} f_{\rho\sigma} \tag{9}$$

equations (7) and (8) have the equivalent, generally invariant form

$$\mathfrak{f}^{\mu\nu}{}_{,\nu} = 0 , \tag{10}$$

i.e. the vanishing of a tensor (density) divergence.

The first set of Maxwell's equations, however, does not automatically lead to generally invariant definition of the current (density) vector unless the latter can be regarded as a vector density. For homogeneous and isotropic, electromagnetically active matter and constitutive relations (direct proportions)

$$\underline{D} = \varepsilon\underline{E} , \qquad \underline{H} = \mu^{-1}\underline{B} , \qquad \varepsilon\mu = 1 \quad (\varepsilon,\mu \text{ constant}) \tag{11}$$

the first set becomes

$$\mu j^{\lambda} = f^{\lambda\sigma}{}_{,\sigma} \tag{12}$$

with the necessarily contravariant vector

$$j^{\lambda} = (\rho,\underline{j}) ,$$

ρ being the charge and volume-density, and

$$f^{\mu\nu} = \eta^{\mu\rho}\,\eta^{\nu\sigma}\,f_{\rho\sigma}\,.$$

The generally invariant form is obtained by replacing ordinary partial by covariant derivatives in (12) (minimum coupling hypothesis) or by the equivalent replacement there of tensors by tensor densities

$$\mathcal{j}^{\lambda} = \sqrt{(-g)}\,j^{\lambda}\,, \qquad \mathcal{f}^{\mu\nu} = \sqrt{(-g)}\,f^{\mu\nu}\,. \tag{13}$$

3. Let us now suppose that, instead of the identification (6), we put

$$B_k = h_{0k} = -\,h_{k0}\,. \tag{14}$$

This is enough to induce

$$E_k = h_{ij} = -\,h_{ji}$$

when the second set of Maxwell's equations becomes

$$h^{\mu\nu}{}_{,\nu} = 0\,, \tag{15}$$

indices being raised with the Minkowski $\underline{\eta}$, or the generally invariant

$$\mathcal{h}^{\mu\nu}{}_{,\nu} = 0 \tag{16}$$

if the tensor density

$$\mathcal{h}^{\mu\nu} = \sqrt{(-g)}\,h^{\mu\nu} = \sqrt{(-g)}\,h^{\mu\rho}\,g^{\nu\sigma}\,h_{\rho\sigma}\,. \tag{17}$$

With the primitive constitutive relations (11), the first two of equations (1) become

$$h_{23,1} + h_{31,2} + h_{12,3} = \mu\rho\,,$$

$$h_{23,0} + h_{30,2} + h_{02,3} = -\,\mu j_1\,.$$

These are but the first two components of the invariant equation

$$\mu j^{\lambda} = h^{\lambda\sigma}{}_{,\sigma} , \tag{18}$$

where the tensor density

$$h^{\lambda\sigma} = \tfrac{1}{2}\varepsilon^{\lambda\sigma\alpha\beta}\, h_{\alpha\beta}$$

and

$$j^{\lambda} = (\rho, \underline{j}) \tag{19}$$

is a vector density.

If both $f_{\mu\nu}$ and $h_{\mu\nu}$ are tensors whose indices are raised or lowered with the metric $g_{\mu\nu}$, the generally invariant relation between them is easily seen to be

$$h^{\mu\nu} = \surd(-g)h^{\mu\nu} = \varepsilon^{\mu\nu\rho\sigma}\, \Phi_{\sigma,\rho} . \tag{20}$$

Here $h^{\mu\nu}$ is explicitly shown as a tensor density.

4. Either of the initial choices given by the relations (6) and (14) is entirely arbitrary, and therefore either the form (10) and (12) or (16) and (18) of Maxwell's equations should be logically, completely equivalent. Indeed, they are equivalent in the Minkowski space M_4 where the volume factor $\surd{-g}$ is unity. However, and this is the main point of this note, there is a very significant difference between them in a curved space-time V_4 either of General Relativity or of the so-called Nonsymmetric Unified Field Theory of macrophysical gravitation and electromagnetism (ref. 2).

It is that in the standard choice of section 2, the electromagnetic intensity field $f_{\mu\nu}$ is a tensor whereas in the notation of section 3, the field $h_{\mu\nu}$ is a tensor density. Then and only then can equation (16) be seen to be generally invariant although its counterpart, equation (7), is so whether we are in M_4 or in V_4. Otherwise, of course, that is if we took equation (15) as the correct form of the second set of Maxwell's equations (with $h_{\mu\nu}$ as a tensor), the invariance in a curved space-time could only be recovered by invoking the minimum coupling hypothesis and replacing arbitrarily

partial by covariant derivatives. There would be nothing wrong in this because, again, the difference between the latter is not noticeable in a flat (Minkowski) space-time. In any theory of physics which lays claim to comprehensiveness though, minimum coupling is a hypothesis just as any other, and should be avoided if at all possible.

It is, thus, particularly gratifying that the choice of section 3 (with $h_{\mu\nu}$ a tensor density) automatically implies the generally (i.e. in a curved space-time as well as in a flat one) invariant form (18) of the first set of Maxwell's equations also. The only price that has to be paid is to regard the form (19) of the current as a vector density. It could be argued perhaps that this is a kind of minimum coupling hypothesis already but the argument would be specious both for mathematical and physical reasons. Mathematically, the hypothesis is not needed in the standard case as far as equation (7) is concerned and only appears in equation (12). Physically, on the other hand, the right-hand-side of equation (19) is a density so why should it be a vector?

The identification of section 3 was advocated long ago by Einstein himself but, I believe, the above reasoning in its support has been largely overlooked. Of course, the main point is that the presence of $\sqrt{-g}$ cannot be ascertained in the case of a flat space-time.

I cannot enter here into specific demonstration of what difference to the conclusions of a physical theory can be obtained merely by selecting one as against the other invariant representations of the electromagnetic field. Suffice it to mention only that, since the factor $\sqrt{-g}$ appears also in invariant integrals, the choice of representation itself can have far-reaching physical consequences for a given theory. For example, it is not difficult to construct models of the universe which, in one representation (the standard one, with $f_{\mu\nu}$ a tensor), will have zero net charge on them but, in the other ($h_{\mu\nu}$ a tensor density), will exhibit a nonvanishing net charge. If the theory itself can provide us (as it does!) with independent reasons, say, for the second choice (for example, because it is more natural in the case of nonlinear electrodynamics), we would have to conclude that the universe we live in is, in fact, charged. It is interesting to note that the expansion of the universe itself can then be attributed to the excess charge.

It is important, therefore, to remember that the elementary and apparently arbitrary choice of representation of Maxwell's equations themselves may not be, in fact, all that arbitrary.

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Department of Applied Mathematics,
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Australia.

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Does Technology Need Science?*

TREVOR W. COLE

INTRODUCTION

Science and Technology have always had an ambivalent relationship. Recent questioning over funding, usefulness to technology, and appropriate priorities in Science is only the latest, but is perhaps the most vocal, period of discussion over the last century in Australia. The aim of this paper is to identify some of the issues and, based on the author's personal background and interests, survey science and technology in Australia both now and over those last one hundred years. The resulting view leads to the need for hard decision making and change.

DEFINITIONS

Science, basic science, is often presented as pure, as a spirit of enquiry, exploring the workings of Nature by postulation and experiment in order to arrive at a description or understanding of how Nature works. The resulting theories aim to simplify the description of the world around us by their elegance and their power to predict the results of experiments. The results of the research, the Science, belongs to the world and is traditionally assessed by its rapid publication in the open literature. James Watson, in his book "The Double Helix" provides a candid insight to the motivations and rewards of basic science at its best, a thrill and drive the author was able, in a small way, to feel in the 1967 discovery of pulsars in the Cavendish Laboratory, Cambridge.

Technology is quite different. Technology aims to exploit the workings of Nature, to set up an environment in which the rules of Nature are structured to achieve an action or result which is of USE. The product is designed to enhance the environment in which we live. Very often the product is designed to make MONEY for the person who makes it. Technology has its own rewards - often financial but also in the self-satisfaction one has in influencing the environment, the way we live. We remind ourselves of Alvin Toffler's definition that technology was developed to meet one of two criteria: "Does it make a buck or a big bang?"

There we see the nub of it all. Basic Science is as pure as the driven snow, increasingly costly but without concern for cost, a pursuit of the spirit with rewards measured by self-satisfaction, peer group esteem, by Nobel Prizes. Technology, on the other hand, is sullied by material aspects, by exploitation, by money. Both, however, have a profound influence on us as civilised beings, removing mysticism from our view of the world, increasing our capacity to control our environment, our fate.

ORIGINS

In Australia distinctions between science and technology seem drawn more strongly than elsewhere. In Australia there seems to be a difficulty in finding the relative and complementary roles that science and technology can play in the pursuit of prosperity and progress, in the pursuit of our cultural development and civilisation.

Australian technology has a popular image quite intimately linked to the heavy metal trades industries whose origins are in the central area of Britain and which formed the core of the Industrial Revolution. We associate that consciously or unconsciously with the working class. Yet Brunel, Stevenson, Trevithick, Wilkinson, Brindley and other great names of the Industrial Revolution belie that working class image even though they were still not part of the establishment, not part of the Royal Society, Oxford, Cambridge and Edinburgh circle. Because in that circle one has definitely entered the arena of basic science, of curiosity in Nature, of leisure time derived from wealth in which to follow academic pursuits. In that circle one finds the origins of our Australian view of science.

A middle ground between these groups did however exist but was centred at none of the places mentioned so far. Faraday at the Royal Institution in London, Wheatstone at King's College London and especially William Thomson (Lord Kelvin) in Glasgow were to some extent different from the others. These were the ones who translated the theoretical observations of Maxwell and Rayleigh into useful products, to motors, generators, communications signalling systems and the first trans-Atlantic cable in 1866. For there was at that time a need to translate the results of the theoreticians to a form suitable for application and the success of the British engineer Oliver Heaviside in doing so without extensive formal education is of

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* Address to the Faculty of Science Centenary Celebrations at the University of Sydney on April 18, 1985

significance. Here then was technological transfer in operation just over one hundred years ago - a two way flow of ideas as science was translated to applications and as Maxwell's theoretical work benefited so much from the practical"lines-of-force" ideas of Faraday.

The examples I have used are based on physics and electrical engineering although similar stories could be told of other areas of science. For example the fundamental observations of the science of chemistry had perhaps their most effective and dramatic (even explosive!) transfer into technology in Germany. The history of BASF (amongst others) highlights a most successful and complementary role of science and technology. The chemists Caro, Brunk and Glaser in the 1870s introduced scientific methods to production control and began research in BASF. The five chemists in BASF in 1870 grew to 61 by 1884. Whereas only a short time before this chemistry had been regarded as nothing more than an unremunerative trade for eccentrics, one sees just a decade or so later numerous people choosing it as a new and promising career. The development of the dyes in BASF was also achieved by a close and personal relationship between the industry and the academic community of chemists. An extensive collection of correspondence survives to attest to this.

Our questioning and discussions today are not then focussed on chemistry. They are indeed focussed on physics because, as we will see, it is the technology based on physical principles which is the vanguard of the recent technological thrust, it is physics within science which has had greatest difficulty in coming to grips with its technological or "practical" relation.

Today we celebrate a centenary and it is appropriate to concentrate on the decades around 1885 to really see if things have changed all that much. As a contrast to Australia, consider developments in science and technology in the United States of America where, for example, one finds in 1883, the foundation professor of physics at the Johns Hopkins University, Henry Rowland, announcing in his vice-presidential address to the American Association for the Advancement of Science that the word "science" should no longer be applied to the telegraph, telephone, electric light and electric motor. The earlier English developments had not drawn any such distinction between the science and the technology of new fields of electricity and magnetism. The American physicist could now choose - theoretical or practical. In the universities the training in electricity and magnetism was carried out in the physics schools with the students making the choice between a training for industry or in science (usually as a teacher). The separate discipline of electrical engineering was to grow then out of the physics schools over the following decades. Electrical engineering was then, even a hundred years ago, in a different position from the other branches of engineering. Mechanical engineering was, and is, the closest branch of engineering but never took the opportunity to build on the developments in electric motors and generators. Electrical engineering, and all the technology it spawned began in physics, then separated from it, and left behind an emasculated and struggling scientific discipline without a major role or purpose.

The popular imagination had been aroused by the inventiveness (in commercial rather than academic surroundings) of Bell, Edison and Tesla, and electrical engineering flourished - much to the chagrin of the physics schools which had poor laboratory facilities, had to teach these students and see only a small percentage of the students stay to follow the "pure" science path. The spread of use of electricity throughout commerce and society was seen as a most strong utilitarian justification of physics and tied in well with the American passion for practicality.

The supporters of basic or pure research, proud of their stand for cultural advance, and convinced of the nobleness of their cause, were under threat. John Tyndall went on lecture tours in the 1870s pleading for support of pure research. Simon Newcomb, the astronomer, deplored and bemoaned the low levels of national funding of pure research in 1876. Then in 1883, just 102 years ago, Henry Rowland delivered to the American AAAS his talk entitled simply "Plea for Pure Science".

This group of researchers, using arguments about the long term utilitarian worth of pure research but motivated by the classical pursuit of truth, were overtaken. The practical fruits of physics were pursued with vigour. Gilman, the president of Johns Hopkins proclaimed in 1882 that electricity had

> "wrought greater changes in commerce than the discovery of the passage around the Cape; greater modifications in domestic life than any invention since the days of Gutenburg."

The final blow came towards the end of the last century when the newly separated electrical engineering schools began to question the role of physics education to their curricula. In a discussion still proceeding , only the relevant parts of physics were to be taught and often had to be taught by the engineers themselves with an approach which typified the split which had developed between the newly spawned offspring and its parent. For the physics programme was found to be less and less concerned with the physical areas which formed the basis of the rapidly developing technology.

Chemistry had spawned its applications through chemical engineering, biology through medicine. But in both these cases the originator survived in a way that physics was unable to. It took well into the twentieth century for physics to reassert itself as a discipline in science and to the mid-twentieth century before it reached its peak of eminence amongst the sciences. Yet even then, the loss of its most powerful area of application to electrical engineering had left it isolated in the realm of "pure" science. It is a dilemma which we are addressing today.

So a century ago one sees the same discussions, fears and developments in the U.S.A. as one sees here

in Australia today. An emphasis on utilitarianism, the questioning of the value of pure science, the struggle of pure science for funding, are all of current concern. But before dealing in detail with today, we should look back at our own past, at Australia of the 1870s and 80s. For there we can also find experiences, arguments and attitudes well recognisable today, so well entrenched as to be the barrier to the technological changes being sought.

AUSTRALIAN SCIENCE

The development of science in Australia is linked closely to this University, and to the Royal Society of New South Wales. In the Inaugural Address to this Society on July 9, 1867, the role of science in Australia was clearly stated. Reverend William Branwhite Clarke (an original Fellow of St. Paul's College here at Sydney and a pioneer geologist) said that

"We have in this Colony a vast region, much of which is still untrodden ground. We have, as it were, a heaven for astronomy and a new earth for geology. We have climatic conditions of the atmosphere, which are not to be viewed by us merely as phenomena interesting to the meteorologist. We have facts to accumulate relating to Droughts and Floods which have deep financial and social importance. We have a superficial area which may engage the attention of Surveyors, Agriculturalists, and Engineers for years to come. We have unrevealed magazines of mineral wealth in which Chemists and Miners may find employment for ages after we shall have mingled with our parent earth."

Through the periods of Clarke, Russell, Liversidge and Edgeworth David; through the Royal Society of N.S.W., Australian Association for the Advancement of Science (to become ANZAAS in 1930), CSIR (CSIRO), and the Academy of Science one sees these same areas as unchanged fields for study, pursuit and for funding. In a unique way Australia was able to isolate its science from the physics developments of overseas, from the utilitarian application of physics so evident in Britain, U.S.A. and Germany. The doyens of Science of the last 100 years in Australia are epitomised by Edgeworth David of whom an obituary wrote

"Science was to him the eager quest for truth, a joyous adventure in which fresh wonders and delights were ever appearing to reward the diligent searcher."

Several Presidents of the Royal Society of New South Wales (amongst others) were aware of these limiting trends. C.O. Burge in 1904 lamented the lack of appreciation by Government and the people of the practical importance of science and commented that if one did not appreciate this we may be

"rudely awakened from self complacency by some crushing loss in trading or in war."

That shattering of our self complacency is the basis of our current debate, our current reassessments and our current challenge.

AUSTRALIAN TECHNOLOGY

Despite the particular emphasis found in Australian science, which ignored applications of physics, technology was introduced rapidly to Australia. As early as 1863 battery driven arc lamps were set up at Sydney Observatory as part of the celebrations of the marriage of the Prince of Wales. In 1878 arc lamps enabled nighttime construction to proceed on the new exhibition buildings.

In 1882, only four years after the world's first street lighting was set up on the Avenue de l'Opera in Paris, permanent electric lights were placed at the Redfern railway terminus driven from a small generator. Six years later, Tamworth became the first city council in the southern hemisphere to set up an electricity supply service when, for 300 pounds, it imported an 18 kilowatt Crompton and Co. generator from Birmingham to light 150 filament and 4 arc lamps.

There was not much science in this and really, not much electrical engineering. The technology and know-how was fully imported and it was a technology built more upon qualitative understanding and empirical rules rather than the exact and scientific approach. It was a technology not appropriately supported within our universities.

The developments overseas in electricity and magnetism spread quickly. Those of communications by morse code and cables spread so readily to a continent dominated by isolation and geographical spread. By the 1870s an extensive cable and telegraph system was in place. Onto this system could be placed the telephone and in the century old story of Australia's telephones are some lessons very relevant to our current concerns over technology and science.

The telephone was first demonstrated by Alexander Graham Bell on 10 March, 1876. As early as 1877 in Australia, following the arrival of several detailed journal articles, W.J. Thomas (a customs agent in Geelong) had linked several houses together with home constructed telephones. By the following year systems had been constructed - all with home made equipment, in Tasmania (by a medical practitioner) and in Brisbane.

Of most note was Henry Sutton who, in Ballarat at the age of 21, had devised and constructed over twenty different versions of telephones and who made the classical Australian mistake. He thought his

discoveries "should benefit other workers in science". In the end sixteen of his designs were patented overseas by others. Little wonder that Bell, on his 1910 visit to Australia, made an especial trip to Ballarat.

Not all made the same mistake. J.E. Edwards was a notable inventor and entrepreneur who emigrated from London in 1866 as an experienced telegrapher and electrical constructor. His invention of the "division bells" for houses of parliament came while still in the telegraph department. Edwards is a good role model for today as he set up his own company to manufacture relays and signalling equipment for the Victorian Railways in 1877. The following year he followed a childhood dream and patented his ideas on sending music and voice by wire. He made and sold telephones of his own design until closing his company in 1885. One hundred years ago!

So one hundred years ago we had an information technology industry, flourishing and producing the most recent of technological developments. The industry was locally owned, the technology locally controlled and managed. What went wrong over the following 100 years?

But that is another story - the lesson for today is that the telephone developments were, in general, quite separate from science in Australia and this holds several lessons for us as we argue today the relative positions of science and technology.

TODAY

With that as a background, let us now address more closely the current question of whether technology needs science and, more importantly, if the answer is yes then what sort and how much.

The key to answering our question is to ask whether one of two views is most appropriate. The first view is that pure science, generated from basic research, provides new insights to Nature and hence new opportunities to exploit those insights as technology. The second view is that technology develops mainly from technology and only occasionally does an input from science make a significant shift in technology.

Indeed, it is often the technology which drives and extends science, both by generating questions about nature answerable by the classic scientific approach and by provision of extensive instrumentation without which the basic questions in science could never be addressed.

It may be that the wrong questions in science have been asked. One comes back to motivation and just one example will be given to illustrate my theme. A success story in Australian science is its contribution to radioastronomy using innovative and pioneering instrumentation. A common justification of funds for radioastronomy is that it will have practical spinoffs into technology and in techniques for communication. I would rather see the attitude and motivation in Australia which says that if we developed a strong and vigorous communications industry, we would be ideally set up to build and afford the instruments needed for radioastronomy. If commercial success is the criterion to use then science and technology need each other only in so far as they affect the outcome in the market place. For arguments between science and technology are for nought if it is the wealth and living standards of a country which drive us. Commercial and marketing factors must be considered concurrently with science and technology if one is to understand their interrelationship. This consideration is what I call engineering and perhaps one has revealed at long last the real message of this talk. For engineering deals with humans and human-sized problems - it affects the human environment and the human style of living. Basic science on the other hand or nuclear physics and high energy particle physics on the other. It is as one moves away from the human scale using instrumentation which is in itself extreme, that the contributory links to technology and engineering weaken. Such work cannot be judged on commercial grounds and should be judged and valued in context - as the contribution to world science, to the world's understanding of Nature, to civilisation. But the trap is to transfer the same attitudes and concepts about science to the area of human scale. For there the links to technology, to engineering, to commercialisation are stronger and demand different motivations, different attitudes, different skills.

We have been sidetracked and blinded by the chosen traditions for our science in Australia. Physics has never taken the path over the last hundred years as it did in the U.S.A. and Europe. We have had our science dominated by other areas and we have not absorbed the lessons available in the world around us. For these lessons are there and have been there. We need only look to our north, to the developing nations on the Pacific basin to learn.

OUR NEIGHBOURS

Despite the dramatic levels of engineering development and technological productivity, science has not been strong in Japan, Korea, Singapore and the other rapidly developing technological societies to our North if one uses our criteria of Nobel prizes, papers published per capita, or even patents issued.

Japanese technological success is based upon bought technology. Over three decades Japan methodically has bought licence rights to the foreign technology it needs. This was, in the main, the vast bulk of the new technologies developed in the United States since the 1950s. The accumulated cost for acquisition of all this technology is only 15 billion dollars. A most efficient way for a country to build up its technology - much cheaper than a grand scheme of local invention and re-invention.

This cost for technology does not include the free contribution of ideas and technologies available to all in the published literature - the results of a misguided concept of contribution to world science in contrast with and alien to the concept of technological development.

Since the period 1951 to 1954 when Japan imported 100 times as much technological ideas than it exported, one now has the situation where the imbalance has been removed, where patents are as common as in other large industrial countries, where amounts spent on industrial research and development are higher than in the U.S.A. and Germany. Clearly, technology is building on technology (as it can in a position of economic strength) and Japan is increasingly willing and able to undertake its share of contribution to world science, to the very long term future.

Much of this change is due to the clearly stated set of national goals to which the somewhat more rigidly structured Japanese society responds. The country has a consensus about the future, a general agreement about what are the critical sectors for development. The country is agreed on a 'technology-oriented nation' as its future with emphasis on a 10 year programme to develop new technologies for next-generation industries. The focus is on three fields: new industrial materials, information processing and biotechnology.

Because of its relevance to the discussion of this paper it is important to outline in more detail these areas. Biological science and technology in Australia have already developed an appropriate relationship. But in industrial materials one is talking of ceramics, synthetic membranes, advanced composite materials, electrically conductive polymeric materials, advanced alloys and engineering plastics. The work on these topics is not to be found in many of the Australian physics schools depite the physical nature of the problems. The research work on these areas is in the engineering schools. In the third area, information processing, one enters a whole new field of development worthy of long discussion in itself but able to be put into perspective by "The Industrial Tree" in which the new areas are represented as the Quaternary sector of industry, a completely new sector divorced from much of existing engineering technology. Even more to be appreciated is the way that this technology is utterly and completely dependent on human interfaces for its application and usefulness. Whereas a bridge, dam or mechanical device is functional without human interface, the knowledge industry must involve human factors quite alien to the existing areas of technology and the science from which it has developed.

OUR FUTURE

Let us try then to draw together the strands of our discussion to identify the appropriate roles for science and technology. Australia is a small country whose size suggests it should contribute 5% to the world's science and technology. The immediate implication of that is that we expect to have to import 95% of our science and technology needs. Thus science as a basis to technological R&D must be placed in a very low priority when compared with the engineering needs to take existing technology and engineer it into the products and services which provide the wealth we all desire. There already exists much of the technology and science we need to support our industrial development. Further technology must come from extending the limits of products already in manufacture.

Australia still needs a large number of very skilled people if a technological society is to develop. But to quote L.M. Branscomb of IBM,

"The critical fields, I think, are electrical engineering, applied mathematics, computer science, information systems management and manufacturing systems engineering and, if one gets into the basic technologies, very importantly, the materials science, in particular ceramics, polymers and surface physics and chemistry."

My views are then becoming clear. I do not see that science in Australia supports the technologies to anywhere the level and style which Ifeel is appropriate. Science has backed away from the areas of critical importance to the technologies, from areas which are invariably complex, messy, often mundane, certainly not glamorous but which are, nevertheless extremely challenging. I refer to many areas but would offer the following short list as illustration.

heat flow and mixing in turbulence
structures and properties in new ceramics
physics and chemistry of the blast furnace
processes in injection and other moulding
properties and failure modes in composite materials
material properties and flows in warm-forging

Distinctions in science as regards motivation and style need to be made in ways not as yet done in Australia. Technology is of a human scale, is driven by attention to the limits and edges of current technology, is invariably involved with the cost of the processes, and is often dealing with intractible and difficult areas of science. It is invariably concerned with interaction with humans.

Of course, science is needed. It forms the cultural base on which we will understand the technology, place it in perspective, and resolve our conflicts brought upon us by technological change. Science is necessary in its own right, and offers a unique opportunity to the human race to understand its environment

and purpose. It is intimately linked to the philosophy of science and as it explores the most distant of the edges of nature it is able to contribute to the cultural development of humanity and to our civilization.

Needless to say I am therefore in conflict with current attitudes in much of our scientific community, be it Universities or CSIRO, and in conflict with the view of the Minister of Science, Barry Jones. For he has not made the distinctions I have, has ignored the true role that physics, in particular, plays and has not recognised that the overwhelming need is for product development skills, for management skills, for entrepreneural skills. These will not, in general, come from attempted conversion of scientists built up with over a century of adherence to a local scientific basis totally inappropriate for a technologically strong Australia.

The problem therefore does not all lie in a relationship between science and technology. It lies in a crying need to educate and prepare a nation FOR technological change. It does not lie in the creation of bodies to study the impact OF change. We need a concensus view of the future LED by well structured plans and decisions.

We need to do something - the penalty of doing nothing is too extreme. That something is achieveable by clearly stated national goals - a clear long term policy for Australia to manage its own technology. To buy if necessary, to implement systems using it, to sell these systems here and overseas, to build the technology and science base to support this wealth creating operation. We may then reach the point where we can better afford to take our proper share of the load of world science, of the basic science which marks us out as a nation with faith in mankind, with admiration for nature, which is civilised, and which has a true wonder but yet confidence in the rapid and dramatic changes which technology will bring upon us.

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Peter Nicol Russell Professor,
School of Electrical Engineering,
The University of Sydney,
N.S.W., 2006, Australia.

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Science and Truth*

LLOYD REINHARDT

Abraham Lincoln was once standing before a donkey conversing with a colleague. The dialogue went like this, Lincoln speaking first:

"Sir, how many legs does this donkey have?"
"Four, Mr. Lincoln."
"And how many tails does it have?"
"Why, one, Mr. Lincoln."
"Now, sir what if we were to call the tail a leg; how many legs would the donkey then have?"
"Five, Mr. Lincoln."
"No sir, for you cannot make a tail into a leg by calling one."

In some years of using this dialogue in pedagogical situations, I have found it inevitably generates disagreement among students. Occasionally I have taken straw votes. I wish I had been doing this for many more years and keeping records. It might be a barometer of the spread of the dark clouds of relativism. Nowadays the vote produces more support for Lincoln's interlocutor than for Lincoln, with a number of abstentions.

I suppose the most obvious feature of truth and objectivity to be that, whatever the subject matter, saying or thinking something to be so does not make it so. Something independent of thought or assertion determines whether what I think or say is so. There is a significant class of exceptions to this, however; and examples are worth giving if only to set them aside. Standing before the altar or the town clerk and saying 'I do' is not to describe my marriage; it is, rather, to perpetrate it. Similarly, it is, often, saying the words 'I promise' that makes it true that I have promised; or it is the umpire saying "you're out' that makes it true that you are out. In these and similar cases, which are called performative utterances, saying does make what is said so. Such cases aside, I can proceed.

Truth and objectivity, considered with this feature in mind, are bound up with what philosophers call the problem of the 'reality of the external world.' But it is worth mentioning, just to set something else aside, that mathematics is pretty clearly an inquiry in which saying does not make things so, while it is also an inquiry about which it is extremely problematic to speak of the things studied being part of external reality. The metaphysics of mathematics is difficult and controversial. But at the very least, it is bizarre to think of numbers as having natural histories as species of animals or mountain ranges do. I shall have no more to say about mathematics however. I used the well-known phrase 'reality of the external world.' Now I do not like that phrase. It invokes a picture of our minds or our consciousness as inside and the rest outside. This is a distortion of the nature of the relations between ourselves and the rest of reality; and it is eventually also a distortion of the nature of what is our inner life. It is a picture the Oxford philosopher, Gilbert Ryle, denigrated as of the ghost in the machine. It stems from Descartes. The Cartesian picture has been superseded recently by an image that is modern and up-to-date. It is the picture of each of as possibly a brain in a vat, being manipulated by a marvellous computer program into having the experiences and acquiring the beliefs that we do have and acquire; we are completely and systematically deluded in our attitude that we are human bodies, including other people. But dispute with proponents of the neo-Cartesian vision - a vision that calls itself modern materialism,

* An address to the Faculty of Science Centenary Celebrations, University of Sydney, 22nd April, 1985.

identifying our minds with our brains - is not my concern here, though some of what I say toward the very end will bear on it. Proponents of that view and I are in agreement about the commonsensical points that, e.g., the solar system and the galaxy were here before us or before any sentient life was here; and we are in agreement that much plant life and geological activity occurred on the earth before there was any sentience or consciousness there; and we are in agreement that, unless the nuclear disaster we dread goes even further than we anticipate, much plant life and geological activity, among other things, will continue on its way long after we are extinct.

My interest here is to acquaint you with a dimension of the current phase in the struggle to cling to the idea of things being as they are whether we know it or not, whether we like it or not; and even whether we have developed, in our thought and language, conceptions of it or not. The anecdote about Abraham Lincoln is to the point. You cannot make a tail into a leg by calling it one. The implication of that is that the differences we have conceptualized through our words 'tail' and 'leg' have, in their way, carved reality at the joints, to use a fine metaphor of Plato's.

Now this does not obstruct the possible development of a scientific theory which trivializes the significance of the differences between tails and legs. After all, we already use the word 'limb' to cover both arms and legs. What I've said in no way rules out the possible usefulness of assimilating arms, legs and tails in virtue of all of them being appendages to torsos. Philosophy has little to do with that.

Earlier, I spoke of the dark clouds of relativism. Here is another philosopher speaking in 1940 of the threat of those clouds:

> "If the people who share a civilization are no longer on the whole convinced that the form of life which it tries to realize is worth realizing, nothing can save it. If European civilization based on the belief that truth is the most precious thing in the world and that pursuing it is the whole duty of man, an irrationalist epidemic, if it ran through Europe unchecked, would in a relatively short time destroy everything that goes by the name of European civilization."
> (R.G. Collingwood, *An Essay on Metaphysics* (1940))

I have reservations about the phrases 'the most precious thing' and 'the whole duty'. Surely bad enough if truth and its pursuit are among the very most important things in our civilization and they are threatened.

Others, far less attractive to me than Collingwood, can be heard speaking of the faith of our civilization, our Christian civilization, in God; and of how the loss of that faith will throw us into barbarism and chaos. Perhaps their concern and Collingwood's are connected. I think there is a connexion; for I think that philosophy has a current task of providing a conception of reality as independently determining whether our thoughts and assertions are true or false, a conception which does not, covertly or explicitly, invoke the notion of a God's eye view. How does that work? It seems to me it is a matter of feeling that truth involves a point of view outside all our local points of view. By 'local' here I mean something we should understand both in terms of history and the contemporary surface of the earth; and indeed in relation to possible thinking beings elsewhere in the universe. There is a temptation, in trying to formulate a conception of reality that meets the requirements of objectivity, to sneak in the idea of an ideal observer, a knowing being who stands outside all our points of view. And that is what a God's eye view comes to. What I want to do here is speak about some key conditions for achieving an *absolute conception* which spares itself the burden of God. But I must come to that gradually, beginning with comment on relativism about truth.

I have coined the word 'truforism' for this relativism, a 'truforist' for a supporter of it. I would be astonished if you did not see the aptness of this label. It is not hard to find in much conversation the phrases 'true for me' or 'true for them', or 'true for the Eskimos' or 'true for New Guinea Highlanders' or 'true for Buddhists'. And naturally we hear 'true for us' (whoever we are; I suppose members of

western European civilization are intended; but it is a feature of relativism itself that there is no obvious way to make things stop there; why not true for New Yorkers or true for residents of Bondi?). It is quite easy to find anthropologists these days who will speak of other peoples who 'live in different worlds'. If we take this literally, we must be dumbfounded at how it is that we can kill those people so easily. Now an initial response to truforism is that of my colleague David Stove. If 'true for them' means anything more than 'they believe it' what exactly is the more that it means? This is a good response. For there is an obvious difficulty about making sense out of 'It is true for them; but it is not true'; while there is no difficulty at all about understanding 'They believe it; but it is not so'. It is common as rain to say truly 'It's raining; but he doesn't believe it' or 'It's not raining; but he believes it is.'

But I think we must go further than David Stove goes to stop the virus from spreading. I have no great confidence that my suggestions will succeed. Truforism is not new. It started in at least ancient Greece, and Plato tackled it vigorously. In its current outbreak and form, I believe it to be largely a product of the spread of European civilization across the earth, with its attendant uprooting of civilizations more ancient than itself and of primitive cultures. We are paying a heavy price for this uprooting and there is little telling where it is all going to end up. Sympathy for the peoples involved and concern about our crimes against them and their ways of life is widespread among members of our own civilization and is especially strong in the sensitive and intelligent young, our students. I don't, indeed cannot, despise this sympathy and concern. Further it goes with the commonly found idea that the very ways of life we have uprooted may be sources of enlightenment for us, either because we gain appreciation of ourselves through comparisons or, more dubiously, through attempts to adopt as our attitudes, attitudes they have, especially toward nature. I said this was more dubious than learning through comparison. This is because such adoptions on our part just aren't live options. And that they are not live options must diminish our ability even to imagine our way into those attitudes. We can no more do that than try to make ourselves into Samurai warriors. I do not mean there is no point in the imaginative effort. But putative adoptions of such attitudes are deep distortions. The effort deserves comparison with those who avow that they practice Christianity because some religion is needed to keep the community together. This is the doctrine of religion as social cement. What is important about such an adoption is that it will precisely not be a religious attitude. Indeed, so far as I understand Christianity, it may be some sort of heresy. Even so, I recognize that going to Church regularly, participating in the mass and other rituals, could lead to authentic faith, a sort of self-induced brainwashing. But that word may be unfair. We do not think of ourselves as brainwashed because we first pay attention to, say, Mozart, in order to impress a lover and eventually find ourselves truly admiring and delighting in his music. So perhaps I must hope that not very many are imitating Samurai warriors.

I said I cannot despise the sympathy and concern that the uprooting effect or European civilization has caused in the young and in others. How to accommodate the concern without condescension and without lapsing into truforism?

A start is to develop David Stove's point further in a way I hope he would agree with. Instead of just asking for the difference in meaning between 'P is true for them' and 'They believe P', let us go further and notice these things: First, it is possible rationally to believe what is false. Second, it is possible irrationally to believe what is true. We can say, in many cases, "They believeP; it is not at all surprising that they do given their life and environment and history; it is even justifiable on their part to believe as they do. Nevertheless, not P". Some examples ought to make this obvious. As we know, it is not true that everybody believed the earth was flat until quite recently. But just about everybody who thought about it in Europe believed Ptolemaic astronomy, which placed the earth so that the heavens revolved around it. Even so, Aristarchus long preceded Copernicus in theorizing, with very good evidence,

that the earth was spinning on its axis and revolving around the sun. Aristotle, quite reasonably, said this could not be, else we should be thrown off the surface due to the ferocious winds that would be generated. Then the Romans took over and darkness descended for a long time. But even when Galileo did much to establish the heliocentric view, it was not irrational to reject it. For there was the problem of the stellar parallax. The fixed stars do not change their appearance to the naked eye; nor did they do so with the best telescopes of the day. This fact made it seem ludicrous to believe that the earth had the large orbit Kepler attributed to it. This could only be so if the distance of the fixed stars was so great as to be merely mathematically conceivable. The distance involved staggers the imagination. The enormously large and the incredibly small still boggle the mind. It could easily have been unjustifiable to accept Aristarchus or Copernicus without some decent explanation of the apparent evidence against their views. So these are case of rationally believing what is false and irrationally believing what is true. It is enough to justify saying, as I want to say, that it was not unreasonable to deny what were in fact truths and that it was reasonable to insist on what were in fact falsehoods. *We can explain the errors.*

Now it would be quite impossible illuminatingly to write the history of science without recognizing these points. Even the new discipline called the 'sociology of knowledge' wants to ask not only why false beliefs were so long sustainable and give answers in terms of non-scientific interests that influenced people; that inquiry also seeks answers to questions as to why truths were discovered when they were, and not sooner or later.

For instance, I have been told that in Germany after the First World War, the indeterminacy involved in quantum mechanics appealed to many scientists because of the question 'How did Germany lose the war?' I guess what was involved was a certain solace in the idea that some things happen just at random. We can see that, even if there is no rigorous way to connect the micro-phenomena of physics with the macro-phenomena of tanks crossing the borders, bullets penetrating skulls, and diplomats engaging in their machinations, this fact about Germany at that time may be significant in relation to the question why a theory was attractive to many people.

Even so, it is worth distinguishing between the sorts of interests and cares which diminish our surprise at people believing certain things because the interest induced the beliefs, and things that relate to whether the propositions involved are justifiable. I doubt if those German scientists would ever have *argued* that Germany's loss of the war was a confirming datum for quantum mechanics. But in the case of stellar parallax, the unchanging appearance of the stars throughout the year was relevant to whether or not Copernicus was right. To take another case, we are unsurprised that the Pope believes that God exists. But even he, especially since the current holder of that office was trained in philosophy, would hardly argue: "I am the Pope, therefore God exists". We expect the Pope to believe in God, but not to believe that his being Pope is a reason to believe what he believes. It is no reason for our believing that what he believes is true; I expect he knows that.

So far, then, we have examples from our own history and civilization of reasonably believing what is false and unreasonably believing what is true. Throughout this, I am using the word 'true' in its everyday way. That way can be called absolute, not relative; it repudiates truforism. The word 'absolute' causes problems here and something is worth saying about it. I could put my point by saying that truth is just truth, and enough said. There is nothing to be gained by adding 'absolute'; indeed it concedes to the opposition that there may be something in the words 'relative truth;. And I have to admit that there is *something.* So let me say what that is. And this too is part of a more conciliatory way with truforists. I know that Spinoza, and I think that Hegel, gave a sort of sense to the notion of *absolute truth.* Spinoza, along with many thinkers in the 17th and 18th Century, made a distinction between obscure and clear ideas of the very same thing. For example, a bird flying by me fast when I haven't got my

glasses on will just be a blur. That blur is a very obscure idea of that bird. Someone with better eyes would have a clearer idea and a good ornithologist an even clearer one. These will all be *ideas of just the same bit of reality*. This is an intriguing way to think. Spinoza is an ancestor of writers on religion and theology who interpret the ideas of traditional religion as seeing through a glass darkly what the more enlightened can see clearly.

It is quite explicit in Spinoza that he thought this way about popular religion. As for Hegel, about whom I am far more ignorant, he thought that religion and art could produce conceptions about life and reality that would fall short of the conceptions that philosophy - his philosophy anyway - could produce. But the religious and artistic productions would be ideas or conceptions of just the same basic truths. He called the ideas, as philosophy would produce them, the absolute truth. It strikes me that rather than 'relative', the word needed here to contrast with 'absolute' would be 'partial'. It is also seems to me that 'absolute', as it is used here, might be well replaced by 'ultimate'. Given that, I cannot forebear to quote something Arthur Koestler once said: "The ultimate truth is the penultimate lie". Still, along these lines, we seem to be able to make some sort of sense out of the notion of absolute truth. But this is no help for truforist relativism. This, as I admit, intriguing way of thinking of intellectual, even spiritual if you like, development, does not threaten plain truth. For basically, little more is involved than is involved in the fact that 'very large city on the East Coast of Australia' may register someone having spotted that city which is, more accurately, registered by 'the largest city in Australia' or 'capital city of N.S.W.'. We may identify and describe the very same thing from the point of view of different interests. Still, I don't know what the ultimate or absolute description of Sydney would be. That is irrelevant to a horde of descriptions just being plain true of Sydney. Accommodating the Spinoza/ Hegel insight, assuming it really is one, is no threat to truth and objectivity. Indeed, in its suggestion that there is a truth to be captured in different ways, some superior to others, it may be seen as positively supporting the realistic attitude I am defending.

The rationalist way of thinking I have sketched here leads to many controversial claims, such as that religion is the opium of the people and the heart of a heartless world; or that belief in God is fear of the power of the father; or that belief in God is recognition of our dependency on community or society. These views seek to explain why religious belief is so tenacious. They do not seek to support the proposition that God exists. They claim, in effect, that to say 'I believe that God exists' is to say 'I recognize my deep dependency on things I cannot control' or something like that. Well, all you can do is see if believers will make that substitution when they are enlightened. That is not my problem. I suspect it is less likely than proponents of these explanations of religious belief may have thought. My point has only been that this notion of absolute in contrast to partial truth is no threat to truth. It is no help for truforism. My hope is that budding truforists will see that, with it, and with what I have said about reasonable and unreasonable belief, one has enough intellectual apparatus to accommodate the sympathy and concern one has for uprooted ways of life without being driven into relativism.

Recall that I am trying to salvage an absolute conception of independent reality without indulging in the God's eye view. One might even say I am trying to express clearly what the idea of a God's eye view expresses obscurely. For the most part, I have offered materials for a theory of error. But the examples have been from our own culture.

In order to address mayself to the question of other contemporary cultures, especially those which are unscientific, I am going to borrow heavily from the Cambridge anthropologist, Robin Horton. Horton belongs to the intellectualist camp in theoretical anthropology. That is, he rejects the idea that the fundamental thinking processes of African tribes he is familiar with are different from ours. They are

significantly different and I will say how he thinks they are. But the difference is not a matter of basic rationality. I will not, for lack of time, say anything about the opposing camps in anthropology on this matter. Suffice to say that the issue there is one of how we are to make sense of the wildly false propositions primitive people assert quite readily and often. They *say* a lot of bewildering things. These cultures have cosmologies, large-scale views of how the world works and the place of human beings in it. Horton believes they arrive at theories in the same fundamental way that western scientists arrive at them. That is they use the hypothetico-deductive approach. Unobservable things are postulated to explain observable phenomena. Such hypotheses get more or less confirmation from observations that are made as a result of expectations about how things will work, expectations derived from the hypotheses. What is different is the content, not the form, of this approach. First of all, Horton, with many philosophers of science, holds that models of the unobservable are developed by analogy with processes that are observables. Think here of those elegant lectures on physics given by people like Feynman. We are given ping-pong balls bouncing off each other in a birdcage. We are then invited to develop our understanding of what water getting hot is like through this analogy. There are many such pedagogical devices. Analogies and models have played a vital role in the development of our scientific theories. Of course, mathematical equations take over eventually; the models may only be stages on the way. Note the parallel with the rationalist idea of obscure ideas being replaced by clearer ones.

Now, given this, Horton suggests that we develop our theory of error in connection with his Africans in this way: All human beings believe what Bertrand Russell called 'stone-age metaphysics'. That is, in the domain of everyday life, control and understanding of the environment has two dimensions. First, everybody knows that if its sharper it will cut easier; if its heavier, you have to work harder to move it; if it has a hole in it, it won't float; if its coming at you, get our of the way; if you step on it hard, you will often crush it; if you stab him, he will bleed, etc. But in our stone-age metaphysics we also hold that people make things happen by giving orders and that they themselves act; individual human beings are more or less initiators of changes, even if these be only changes in the positions of their limbs in walking or moving food from plate to mouth. They bend things and break them. They also are the givers and receivers of orders and commands, the producers of anger and resentment, of distress through irresponsibility or of pleasure through generosity. To sum this up, stone-age metaphysics includes mechanical goings on and also the goings on of agents, of persons with intentions and desires, and of persons with authority or power. Now all this, Horton notes, goes on more or less within a range of things not much more than 1000 times smaller or larger than ourselves. But then we come to the problems of the very large and the very small, the macrocosm and the microcosm. What if we want to pursue either understanding or some forms of control into those regions of reality?

We must, says Horton, do this by analogy with something from our stone-age metaphysics. This must be done to get the enterprise off the ground. Horton's idea is that we, especially since the 17th century - but also in ancient Greece - had the good fortune to develop our analogies with the mechanical dimension of goings on in the everday world. The Africans, on the other hand, developed their analogies from the other dimension, from the area we may call the area of agency or the powers of persons. And this was also a feature of much European thought in the medieval period. Horton thinks that we got it right and he thinks that it is undeniable that we did. Our greater success at control is inexplicable without that being so. About the natural order, as distinct from the social or the cultural order, western science has hit on the right path. Horton makes the intriguing suggestion, though, that in the understanding of social relations and in the area of psychosomatic illness, the Africans may have the edge on us. That is, their social psychology may actually be, in significant ways, much subtler than ours due to profound reflection on the nature of interpersonal relations. I do not know his work well enough to assess this striking suggestion; and it is not necessary to this discussion.

So here again we have an explanation of why people have the beliefs they have which may make it rational on their part to have such beliefs. And we do not diminish our sympathy and concern for them by going on to say they are just mistaken in many of their beliefs.

We may wonder, in relation to the natural order, what is so powerful about our approach. I think it is that all human beings are bound to acknowledge that something has been got right when a form of description of some occurrence or class of occurrences in nature enables or facilitates the *reproduction of that occurrence* from growing things to splitting atoms. Concomitantly, there is prevention in some cases. This is not to say that science is only interested in controlling things. That would be an insult to the contemplative, wondering moment that so deeply informed the lives of thinkers such as Newton, Faraday and Einstein, just to mention a few. I have spoken of understanding as well as of control. Scientific inquiry, like philosophy, begins, as Aristotle said, in wonder.

The wonder need not cease when power enters on the scene. What I have called the contemplative moment makes no sense at all if a scientific theory is just a recipe for manipulation; nor does it make sense if a theory is just a way for a professional scientist to gain prestige and authority in the community of scientists or the wider community. Ideas, in any domain, are not just so much steam puffing out of the engines of power and prestige. Or, if they are, what engine is puffing out the steam of that idea? This is in no way to deny that many scientists may be corrupt in the ways indicated. They can even be corrupt and produce good science.

Let us return to the probelm of the God's eye view. We want plain truth and independent reality without God. We want to reach a conception of *how things would be anyway*, as it seems useful to put it. Such a notion is inextricably bound up in our concept of knowledge, in the very meaning of the words 'to know'. As in the move from stone-age metaphysics, we must proceed by analogy. The model it would help to be able to apply is a simple one. If people are all at varying distances and directions from, say, a cube, and asked to provide a representation of what they see, you can get different results. I say 'can' for, after all, they may know, on independent grounds that it is a cube and just say they are seeing a cube and there is an end to it. So we have to think of the task as being to say how it looks from a variety of viewpoints, especially if we think of the thing as suspended in mid-air, some people above and others below it and people at many different angles of sight from it. Initially, many of the claims will appear inconsistent with each other. It may look diamond shaped from here or like a pyramid from there, etc. It cannot be all these. But we can systematically relate the diversity of representations to each other and, taking into account that the thing is a cube, everything can be made coherent and consistent.

Let us elaborate the model a bit. So far, all the viewers, as far as I told you, have ordinary eyesight. But we can also suppose some of them to have bad eyesight, cataracts on their eyes; we may suppose some of them to belong to cults which find the cube repellent, but the pyramid a gift from God; or there may be clouds of smoke between the cube and some of them, giving them only brief glimpses. This will produce a much more complex diversity among the set of representations we come up with. Now the task of reconciling the representations will have something more like the difficulty the thing it is a model for has. And we shall have to exclude some of the representations as just not worth taking seriously, even if we can explain how they came about.

Now what we achieve here is the relating of a series of representations to each other in a way that renders their diversity intelligible. We have not only the cube itself, but all those representations of it. We do not want to say that the representations are somehow outside the world, hanging about in some other substance, mind-substance or the like. The representations - we might say the represen*tings* are themselves facts in the reality we are concerned with. We want a conception of reality which includes not

only what is being represented but the very representations of it. That means that we are moving up to the level of representations of representations. At that level our thinking has to include a theory of error. For the first-level representations will almost certainly be inconsistent with each other. We want to be able to explain the irregularities in this representational situation. After all, even if some of the representations are false, they are themselves occurrences or circumstances within the world. Moreover, since it isn't just us western sophisticates who may notice this second level problem, that of representation of representation, we are stuck with a potential infinite regress. But this is a harmless regress. Consider a similar regress that may actually be realized, but not to infinity. Anthropologists get a grant to study a tribe. Some sociologists get a grant to study the practices anthropologists engage in when studying tribes. Some philosophers of science get a grant to study the methods of sociologists when they study anthropologists studying tribes. And then, some other sociologists get a grant to study the approaches of the philosophers of science, etc. It will get very difficult, perhaps impossible, to devise interesting questions if this continues. There is no logical difficulty in the general idea, only a financial difficulty or, as J.L. Austin once said in a different connexion, we reach the point of diminishing fleas.

With such a model in mind, just substitute for my word 'representations', the word 'beliefs'. The totality of our beliefs - though I don't mean to suggest that we have the remotest idea how to count them - is our representation of the world. The needed absolute conception is not that of a position *outside* all this. That would be the God's eye view creeping in again. It just is the idea of the possibility of systematically relating all the representations. This is, of course, an incredible project and the division of labour involved is intense. We need, among other things, theories about how the original cube affects its perceivers via their bodies and especially - as our best current theories have it - their brains and nervous systems. But it is still an open question how to apply our ordinary notion of causality - which is that of something that makes something else happen - to this issue. Can we here, in a theory about perception and belief, proceed as we have in those inquiries where we get laws of nature, invariable or statistically stable universal variations between types of events and properties? It is still an open question whether the role we assign to our concepts of belief and representation will be that of being the effects of the causal properties of the cube on the observers. The representing itself may be, to such a large extent, a social and linguistic activity, the human activity of conceptualization and communication, that the project of reconciling the diversity of representations won't just be a matter of a theory of how various human bodies are stimulated by interrelations to other bodies. That theory, of representation and belief, may have to be more like a social theory than like a psychological or physiological theory. But the fundamental idea remains. Not a God's eye view from somewhere else, eternity or outside the world; rather the idea of the myriad of complex interrelations of all the points of view within the world. And this is what deserves, I believe, to be called the absolute conception. It involves not just the idea that continued and varied observation of the cube will result in convergence of representations of it; it also involves the idea of convergence in our theory of error and diversity of representation. There is plenty left to do. But it cannot all belong to natural science. For natural science demands that our representational practices be seen as just vocable blasts with possibly interesting auditory properties and relations. I believe that the representational activities - which are not of course our only social practices - are overt, public and observable. They are not behind the verbal scene, not the fire of thought behind the smoke of language. They are rather shown forth to our direct observation in the speech and actions of one another. And you cannot fruitfully study them with an eye to the task of reconciling the representations by taking them as sound emitted from complex mechanisms. Thus theorizing and thinking about understanding itself, not as a physical process, but as a human, social process, is indispensable. This investigation requires us not just to try to get at the truth; but to think about truth itself. And truth is not a physical property. I hope this means that my profession

may continue its long run performance on the stage of enquiry, even though it gave the lead in the play to natural science a long time ago.

Department of General Philosophy,
University of Sydney, N.S.W., 2006,
Australia.

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Geography: An Integrative Science*

B. G. THOM

INTRODUCTION

What is Geography? Who are Geographers? What do Geographers do? I will not answer these questions immediately, but will attempt to develop answers in the course of the lecture. These answers will be put into the context of past, present and future nature of Geography, especially as practised at the University of Sydney. However, at this point I want to dismiss as irrelevant and trite the oft-quoted saying that "Geographers do what Geographers do". Such a definition helps no one, least of all those of us who claim to be geographers. We must be more responsible to our discipline, our students and might I say as well, the taxpayer, in providing a more rigorous statement of our aims, achievements and ambitions.

My own association with this University and the Department of Geography began in 1957 when I entered as an undergraduate. I had the good fortune of spending four years with the department as a student. After graduation, I was employed for a brief period as a Teaching Fellow before heading for the then perceived brighter lights of North America. After eight years abroad, I was lured to the somewhat more subdued but academically fascinating city of Canberra and, with the exception of a year as a lecturer at Sydney in 1976, I remained in Canberra until January this year. In my present capacity as Professor of Physical Geography, I feel honoured to be able to look back at developments in this discipline in the Science Faculty over the last century, but perhaps more importantly to be able to look forward to a vision of the subject that attempts to form a bridge between the natural and social sciences.

Without any doubt geography has been good to me, and might I be so bold as to say, to all my colleagues at this University. We can be professional travellers enjoying not only a worldwide comradeship with geographers elsewhere, but also being in a position to evaluate and enjoy all sorts of landscapes which fascinate us. Often friends in other disciplines and walks of life envy our lifestyle to the point of wishing to be reincarnated as geographers! I hope to be able to convey some of that feeling to you today, to attempt to provide you with a geographer's perspective of the earth as the home of man as seen by geographers over the years at this University.

In my introductory lecture to first year students this year, I emphasised the traditions which underlie the teaching and research programmes in Geography at this University. I stressed the need for them to have a 'feel' for the past, to recognise that their scholarly efforts are based to a large extent on what has gone on before. Whilst the same can be said for most other disciplines at this University and elsewhere, I wanted my students to appreciate the somewhat special position they hold in Australian geography.

FOUNDATIONS

The Department of Geography at the University of Sydney is by far the oldest in Australia. It was founded 64 years ago in 1921. Moreover, the first course in Geography taught in an Australian university was in Sir Edgeworth David's Geology Department in 1907. This was a course in "Commercial Geography" and it was taught by a young geologist just about to embark on one of the world's great exploratory efforts - Scott's ill-fated expedition to Antarctica. I refer of course to Thomas Griffith Taylor, born in Essex in 1880, but educated at Kings School, Parramatta. Here he developed two pet hates - Latin and Sport (Taylor, 1958). He undertook university studies at Sydney as a student of David's and at Cambridge. Fortunately, he was not a member of the group that perished on the return from the dash to the pole. He conducted valuable studies of glacial geomorphology in Antarctica for which he was later awarded a DSc by this University in 1916. Before and after his excursion to Antarctica he worked as a "Physiographer" (although officially titled a Physicist) with the Australian Weather Service. Taylor was able to liberally interpret his position and realise the possibilities open to him. He wrote to David on 19 November, 1909:

> I am not quite clear as to the work to be done by the proposed Physicist to the Meteorological Bureau. If it consists in a careful and scientific study of the *physical controls governing life* and industry in Australia then there is hardly any position in the world I should better like to apply for.

So in 1910 he began his geographical career in a meteorological agency linked in wartime with the Intelligence Branch, firstly writing on the physical setting and potential for settlement in Canberra, then contributing to monographs on climate and weather in Australia. However, as noted in the letter to David his prime concerns were not just physical geography, and in 1915 the famous government-sponsored report

* An address to the Faculty of Science Centenary Celebrations, University of Sydney, April 23, 1985.

entitled "Climatic Control of Australian Production" was published. Griffith Taylor had now embarked on what was to become a collision course with officialdom.

Taylor became attached to David's department in 1917 and lectured on physiography - a subject which now embraces most of physical geography - and also on human settlement. His views on problems of settlement in the tropics did not go unchallenged and he even had a textbook banned by less than enlightened educational authorities in Western Australia. In 1921 he was appointed Associate Professor in charge of the newly founded Department of Geography at Sydney University - the department was off and running along a rather bumpy track!

Taylor's extensive travels and synthesizing skills were put to use in combating the national fevour in spreading more evenly the white population by utilizing the untapped empty spaces of the interior and north. A biographer has commented:

> During his brief but stormy career at Sydney University he took every opportunity to air his views on 'Environmental Control'. Taylor resolved to become an 'educator' in the broadest possible sense: although he never completely eschewed the mandatory purification rituals of the established professional journals he was not content to confine his important work in this way, travelling widely in 'problem' areas, giving many public lectures, and contributing frequently to a wide variety of newspapers and magazines. In attempting therein to communicate some of the basic approaches of his own subject to the clarification of contentious issues, Taylor could not resist the opportunity to ridicule every sacred cow: the White Australia Policy, tropical settlement, the extension of railways into the interior and the north (Powell, 1979, p.20).

It is not difficult in today's climate of irrationality in response to Professor Geoffrey Blainey's views on immigration, to imagine the response of vast sections of the community to Professor Taylor's pronouncements on the ignorant exclusiveness of those supporting the White Australia Policy. He argued that the Chinese especially need not and should not be excluded.

As a result of these views, geography was news. The Sydney Morning Herald and other papers fanned the flames in a manner not unlike the criticism of Blainey. Taylor was attacked as a pessimist, for being unpatriotic, and by the likes of Daisy Bates as a slanderer of the spirit and talent of Australia's British pioneers. His reputation was challenged by eminent visiting geographers brought out to serve as a counter to this forceful academic However, he counter-attacked with simple descriptions and maps which blasted away at the "Australia Unlimited" propaganda:

> The vital problem in Australia is to add suitable population with the least of public money. This is not the time to hinder settlement by wasting our wealth on a perfectly futile endeavour to settle arid Australia (Taylor, S.M.H., January 31, 1925). Where then will the future millions of Australians settle? Precisely where live the millions of today (Taylor, S.M.H., March 7, 1925).

These statements were prophetic, but for Taylor in the 20s they were made in the context of bitter isolation. Denied promotion to full Professor at Sydney, he chose in 1928 to join Chicago's department and leave Australia for a continent where his research was more warmly received.

Griffith Taylor founded Geography at Sydney and in Australia. When he retired to Seaforth in 1952 he received a much warmer welcome home both scientifically and personally than he received a send-off some 24 years earlier. The University has honoured him by naming what was the former Geography Department building the Griffith Taylor Building.

There can be no doubting that his "high profile" image was both a blessing and a curse to the discipline. He swung a two-edged sword slicing away at those he perceived as ignorant and wasteful of our natural and human resources, but in return leaving an array of wounded who were not prepared to support the discipline for many years. Taylor did see man and land as two intimately linked entities. The physical environment determined man's settlement patterns and agricultural capacities. He belonged to a school of geography (environmental determinism) which although dying in the 30s and 40s nevertheless remains with us in a highly modified form.

THE MACDONALD HOLMES ERA

Griffith Taylor's successor was a dour Scot, a product of the University of Glasgow. James Macdonald Holmes took up his position as Associate Professor and Head of Department in 1929. He became a full professor in 1945, the first to have this title in a Geography Department in Australia. To some extent he lived under Taylor's shadow and certainly as Taylor's enemy because one of his first acts was to arrange the dismissal of Taylor's sister, Dorothy, from the department. Holmes was tough-minded and authoritarian, and he relished in playing the role of "God-Professor". He was also conscious of the role he could play in social and charitable affairs serving for many years with his wife as a promoter and fund-raiser for the Royal Flying Doctor Service. He put into practical terms his concern for the hardships faced by those who lived in the "outback".

Macdonald Holmes maintained and slowly developed the department through the depression years. Throughout the thirties his involvement in soil conservation and regional planning became more apparent.

With more staff and students he developed his role as leader and co-ordinator of collective research on topics which he supported including pioneering research in climate and agriculture by some of his staff.

Let us look at some of his achievements. Unlike Taylor he courted politicians like Sir William McKell. He was not an environmental determinist, rather the opposite because he saw Australians adapting to the physical circumstances and developing a new culture which would cope with various problems like lack of water, salt in soil, floods, fire etc.

> Antipodean is not enough, for out of a new environment new ideas will arise (Macdonald Holmes, 1963, p.xvi).

His views were sought by those in government particularly on the mapping systems and the regionalisation of administration.

> During the Second World War he was consulted on the determination of viable regions for civil defence and the pooling of machinery, a small but vital step towards the adoption of the regional idea throughout the country (Powell, 1983, p.52).

He was a member of a government three-man Regional Planning Committee during the war, and partly through this committee he developed a reputation as an applied geographer involved in the production of regional atlases, descriptive resource inventories and land use surveys. What he saw very clearly was the need for local areas to assume "regional responsibility for the development of new wealth". These ideas were well received especially following the publication of his book *The Geographical Basis of Government* in 1944.

His other major thrust was in soil conservation. He took an extremely practical approach, relying heavily on the technical ideas of others including farmers whom he deemed innovative and responsible. Macdonald Holmes saw little use for a knowledge of the physics of water flow in soil when it was time to plug up gullies, and spread the gospel of contour ploughing to inhibit sheet erosion.

One cannot overlook his contributions as an educator. He involved his senior students in original research which he closely co-ordinated and sponsored. Both his regional monographs *Murray Valley* and *Open North* involved extensive student input. These students had to do considerable field work.

> My purpose in forcing them into these contacts is to give them a training in adult practical work and to understudy, as it were, competent people at high levels doing their day's work (cited by Powell, p. 53).

This tradition lives on within the department as we strive to inculcate into our honours class what he called "a respect for scientific approaches to multi-faceted, real-life situations" (Powell, 1983, p.53). Many senior geographers in Australia and overseas were enriched by this tradition stemming from Macdonald Holmes.

Both Holmes and Taylor addressed public issues in vastly different ways. One was locally successful, the other more internationally successful. They were very different yet both had a similar approach to the discipline. The common theme running through both their works was a concern for the interrelation of man and land.

> To be a geographer, therefore, is to be a student of community persistence on a background of accurately determined environmental knowledge, and geography is the study of land in relation to people - land as regions and as resources and people as groups with group aspirations and ambitions (Macdonald Holmes, 1961, p.xv).

GROWTH AND SPECIALISATION

The 1960s saw the course of Geography at Sydney University take a very different turn. Partly in tune with developments in the discipline in Britain and the U.S.A., the department took a more specialist thrust. The so-called "quantitative revolution" was underway - a paradigm shift of enormous proportions had occurred in the minds of some geographers who felt that:

> Geography ought to break decisively from its traditional preoccupation with the description of absolute spaces (regions), perhaps even to construct a 'general theory of locational relativity', and to concern itself with the rigorous analysis of distinctive geometrical forms with particular mathematical properties, whose unrestricted purchase would establish geography as a properly 'nomothetic' science (Billinge, Gregory and Martin, 1984, p.6).

At Sydney, the revolution took the form of the new Professor in 1962 - George Dury. He may not have been the archtypal example of a "quantifer" , but he brought with him the spirit of change which was sweeping the campuses of the Northern Hemisphere. Gone was the over-riding concern for regions, for climate and man, for soil and man, and for integrated regional planning in relation to local welfare; in its place there was a drive for increased specialisation. I may be over-simplifying the nature of the change, but to me Dury symbolised the pursuit of excellence in a specialist branch of geography. He

demanded quality of work which would be accepted in the best journals of allied disciplines such as geology.

Dury was seen as an anti-establishment rebel in the eyes of his former colleagues at the University of London. He took up the cudgel to beat the traditional concepts of geography in general and geomorphology in particular. By going to U.S.A. in the early 60s to work with the US Geological Survey he established a degree of independence and a reputation in a field quite separate from "mainstream" geography of the 1950s. He used quantitative techniques, he made detailed field measurements and he developed explanations in the process of testing hypotheses on river channel evolution (Dury, 1982). At Sydney, courses were changed to accommodate his interests including the introduction of what was a headache to many a geography student - statistics. He was an extremely busy researcher and prolific writer seeing in Australia unlimited field opportunities especially in the arid zone.

Colleagues in the department under Dury were stimulated to pursue their specialist interests. During the 1960s one can see the flowering of many specialist studies: fluvial geomorphology involving a spate of PhD theses, arid zone geomorphology, and agricultural and urban geography both with a strong economic bias. The surge of activity was seen in a marked increase in publications from an average of three per year between 1950 and 1960 rising steadily to 29 per year by the time Dury left in 1969 (Dury, 1970). This must be seen as a period of change from the general approach of the man/land theme in geography to a more specialist emphasis. Did our colleagues cease to be geographers at this time, or did they represent a change in the way geographers saw their role, or more pointedly, how they saw their world? I don't think I am being unfair when I say that the specialisation trend represented by Dury and his associates in the 1960s was a natural reaction in a science where individuals failed to master all or even a major part of a growing body of knowledge. Freeman had in 1961 in his book *A Hundred Years of Geography* recognised this trend:

> ...as the volume of research literature increased and the techniques of investigation demanded longer and more rigorous training so it became necessary for the individual to specialise, first as a geographer, and then within geography (cited in Johnston, 1983a, p.39).

To maintain credibility with colleagues in allied disciplines there was a need to compete for funds, to publish in their journals, to attend their conferences and to participate with them in field studies. The new ball game required new players better equipped in methodological skills, more rigorous in scientific method and more willing to mix it with "young turks" in economics or geology or botany. Many of those who came through Dury's department in the 60s possessed the confidence to pursue research in highly specialised fields without being too worried about whether what they were doing was geography. Perhaps they preferred to be called geomorphologists, or land economists or spatial analysts. Who cares they would say, let's get on with the job of solving problems, of publishing and teaching and to hell with the philosophical morass in which traditional geography was buried.

PLURALISM, TENSIONS AND CHALLENGES OF MODERN GEOGRAPHY

As we enter the last decade, and emerge from the Dury era, we see modern geography dominated by a high degree of plurality. We also see a discipline which possesses certain tensions as well as challenges with great opportunities. Firstly *pluralism*; this is represented not only by the various specialisms, but also by those who see the need to retain and even claim the man/land themes as the traditional mainstream of geography. At Sydney in the 70s the specialised thrust was symbolised by the establishment of two chairs, one in physical geography, the other in human geography. Each chair nominally had responsibility for sub-disciplines which fell within either physical or human geography. There was a determined push into coastal geomorphology on the physical side, a thrust which has achieved international recognition. However, the traditional perspective was not entirely neglected. The need to understand more about the various processes by which man and land have interacted over time in creating the rural landscape was pursued, for example in the study of irrigated farming and associated dry-land farms in southern N.S.W. by Professors Langford-Smith and Rutherford. All these studies have been conducted in an increasingly more sophisticated and rigorous way compared to the days of Taylor and Holmes with product of research often being acclaimed by colleagues in other disciplines as well as within geography. I must concur with Harold Brookfield of A.N.U. when he said last year:

> A quarter of century of change in geography has not yielded a consensus. It has, on the other hand, yielded a greater degree of pluralism, and for this much we must be thankful (Brookfield, 1984, p.27).

What of the *tensions*? In the 70s, there emerged within geography the so-called "radical revolution". This aimed to throw the discipline wide open giving it a societal as well as a purely academic role. David Harvey said in 1973:

> It is our emerging objective social conditions and our patent inability to cope with them which essentially explains the necessity for a revolution in geographic thought (Harvey, 1973, p. 129)... (The) immediate task is nothing more nor less than the self conscious and aware construction of a new paradigm for social geographic thought through a deep and profound critique of our existing analytical constructs....our task is to mobilize our power of thought, which we can apply to the task of bringing about a humanizing social change (ibid, p.144-5).

Such a theory should be built on a marxist base according to its proponents. Social reform will be

achieved through education. Accordingly strong differences of style have characterised geography teaching in departments across the western world in the last decade. Sydney was not immune. Some students may have been confused, but at least they were made aware of different philosophies within the discipline (see Johnston, 1983b, for more elaborate discussion of these philosophies). Graffiti in the male toilets of the department epitomise the feelings of those who fought the intellectual battles between the traditionalists, the spatial analysts and the radicals. Thus modern human geography at Sydney could be seen as operating in and fiercely supporting three paradigms: The man-land (traditional); the quantitative-spatial (analytical) and the socially relevant (marxist or welfare).

To one side of all there were the "introspective" physical specialists, often oblivous to the concerns of human geographers; they were devoted to problem-solving and operated totally within the positivist sphere of the natural sciences (e.g. coastal geomorphologists like myself). Some of these physical geographers touched on various aspects of the new field of environmental sciences and even linked up with human geographers in studies of natural hazards and resource management.

The future should embrace these perspectives. The plurality and tensions to which I have referred will remain and so be the source of much stimulation to those of us who are privileged to occupy positions as staff or students in geography departments. But these are also *challenges*. In many ways these are similar to those which our forefathers encountered and so boldly took up. Taylor and Holmes both saw geography in the context of the Earth as the Home of Man. They both interested themselves in the affairs of man in relation to land. The past two decades have seen such specialisation within the discipline that concern for the interaction of man and land has been reduced to a lesser role within the subject.

THE PRESENT AND FUTURE

At this point I want to summarise the state of present-day geography. Figure 1 shows how the "spectrum of geographic enquiry" may be sub-divided. Three main fields are represented by circles in this figure: Physical Geography, Man/Land Relations and Human Geography. Some of the sub-disciplines are also shown in Figure 1. In all branches, there is a common interest in spatial phenomena of some sort. Geographers, no matter what the breed, are concerned with phenomena on the surface of the earth.

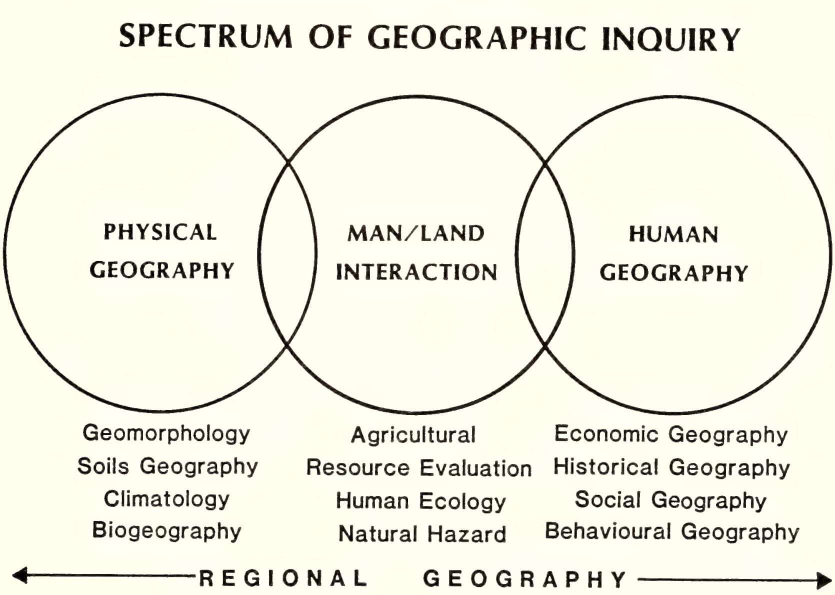

Figure 1.

Within the specialist framework shown in Figure 1 many approaches can be recognised, all of which are leading to the generation of, and revision of, models explaining earth-surface phenomena. These include:

(a) Recognition of *patterns* in the distribution of individual or collective phenomena (e.g. landforms, house types); this is the morphological approach.

(b) Study of *changes* in form properties and patterns of distribution over time representing the historical or evolutionary approach.

(c) Identification, measurement and evaluation of *flows* of energy, information, organisms, finance etc. which are responsible for the transformation of patterns from one state to another - the process or dynamic approach.

(d) The ecological approach which emphasises the *interaction between organisms and their environment* in different habitats; this approach embodies systems analysis and provides an inter-disciplinary focus on environmental structure and techniques (Bennett and Chorley, 1978, p.21).

(e) Analysis of human behaviour and actions from the point of view of determining spatial variations in the quality of life and thereby promulgating policies which are aimed at *social change* - the welfare approach.

(f) The *differentiation of the earth's surface* into areas which possess a degree of unity or homogeneity and thus constitute an identifiable land zone for purposes of education or planning - the chorological or regional approach.

A critical question confronting geographers in the future is how to undertake man/land studies. As an "integrative science" geography should have something to say on the harmonies and phenomena which interact on the earth's surface.

The starting point for my perspective of an approach to man/land geography in the future is to appreciate that the surface of planet Earth is characterised by a mixture of interacting physical, chemical, biological and cultural phenomena represented to geographers as various spatial and temporal domains -

the lithosphere - the crust, rocks landforms
the hydrosphere - oceans, rivers, lakes
the atmosphere - weather, climate
the biosphere - plants, animals, soils
and the sociosphere - cultural and socioeconomic activities

The real world consists of differences between places which are meaningful and understandable in terms of concepts designed to explain patterns and processes responsible for those patterns. Thus plants of a given type do grow in climates to which they are best adapted; man does achieve economic advantages in exploiting regional differences in productiveness of one soil-climate combination over another.

The holistic philosophy involving earth-surface phenomena seeks to focus on the interrelatedness of all phenomena within any given area, which at one extreme is represented by the planet as a whole. The reductionist philosophy concentrates on selected features or processes seeking to know more and more about less and less. Both philosophies must be accommodated within any discipline, especially one like geography which has pretentions of seeking synthesis and of identifying problems where knowledge of the whole is more important than the sum of the parts.

Capra in his recent overview of trends in science, notes the limitations of the reductionist approach in explaining biological phenomena:

> There is no unifying framework that would enable biologists to overcome the fragmentation of their science by evaluating the relative importance of research problems and recognising how they interrelate. The only framework used for such an evaluation is still the Cartesian, in which living organisms are seen as physical and biochemical machines, to be explained completely in terms of their molecular mechanisms (Capra, 1983, p.116).

Likewise, geographers must seek a more unifying, systemic approach which enables the landscape "whole" to be analysed. I am not claiming all geographers should pursue the holistic integrative theme, but I am conscious of the need for someone to provide answers to questions which require an understanding of the ways natural and social forces differentially interact over the earth's surface.

We live in a globally interconnected world, in which biological, environmental, and cultural phenomena are all interdependent. As scientific communication and technological capacities of man expand, the interconnections and interdependencies become more apparent. The nuclear threat and increased awareness of environmental degradation and exploitation make us more conscious of the fragility of life on earth.

Concepts such as the harmonious interrelatedness in nature and the unity of spatial phenomena have been part of geographical thought since early last century. It is ironic that geographers gave up their traditional holistic approach in the 50s to become more "scientific", that is to be more reductionistic. They adopted the classical Cartesian-Newtonian mechanistic approach to the phenomena they sought to study whilst physicists and others were adopting a different approach.

> The exploration of the atom has forced physicists to revive their basic concepts about the nature of physical reality in a radical way. The result of this revision is a coherent dynamic theory, quantum mechanics, which transcends the principal concepts of Cartesian-Newtonian science. In biology, on the other hand, the exploration of the gene has not led to a comparable revision of basic concepts, nor has it resulted in a universal dynamic theory (Capra, 1983, p.116).

Geography has been in more of a mess than biology because it was encountering the strong specialist tendencies of the natural sciences, and as well being subjected to whims, fads, conflicts and successes of diverse social sciences.

Today we begin to see natural and social spatial systems inextricably intertwined, and through feedback mechanisms seeking to adjust to a variety of forces capable of disturbing any harmony or equilibrium. Spatial variability gives the geographer his opportunity to say something meaningful about the operation of these forces at various scales. Rather than chasing that elusive goal of describing the unity of it all which so captivated 19th century geographers, and rather than seeking all powerful models which explain how man is controlled by his environment, or how the natural system is dominated by that intelligent creature called *Homo sapiens*, geographers are now capable of studying how phenomena interact to create spatial change.

The surface of the earth is undergoing continuous change by the operation of natural and human forces. The solar input is not constant, climates are subject to fluctuations, the soil is forever being modified, forests are being destroyed or planted, factories pour variable amounts of pollution into the air and waters, urban areas are both expanding and decaying, population grows in some areas but declines in others, capital is moved daily from one place to another creating new investment opportunities in some regions but less in others; all these constitute just a few examples of the changes which transform the face of the earth. The changes take place in different directions, at different rates and at various magnitudes and frequencies. This means we should be able to use our specialist skills (or work with those who can) to identify equilibrium tendencies. In other words, adjustments which are taking place in response to the forces operating differentially on the surface of the earth must be investigated. Forces upsetting equilibrium states may then be recognised and consequences of further change can be predicted. The example of drought in the lands south of the Sahara, the Sahel, is a case in point; this represents one of the great man/land dramas being played out on the face of the earth at the present time. In equilibrium, this man/land system would involve:

(a) plentiful summer rains;
(b) slow rate of population growth; and
(c) minimum interference to the traditional socio-economic pastoral life style which has developed a capacity to cope with perturbations in (a) and (b).

Disequilibrium resulting from changes to these forces (e.g. climatic change) has led to further instability through a positive feedback mechanism resulting in horrifying human consequences. What will happen in the future can be predicted for different parts of Sahel when we know more about the factors involved.

Another example which gravely affected the Australian landscape was the impact of the first wave of migrants to this island continent. They burnt at frequent intervals a fire-sensitive vegetation. This caused dramatic change in the flora producing a vegetation dominated by eucalypts - a new "balance" was then achieved. This was to be upset in many areas by another wave of immigrants, this time from Europe who brought an "if it grows cut it down, if it moves shoot it" philosophy to the land they stole.

How can we pursue such studies and provide a much more holistic construction to the understanding of continuously changing, interrelated and interdependent earth-surface phenomena? Two assets are possessed by geographers:

(a) A powerful pedagogical concern to teach an awareness of man/land relations; this concern arises at Sydney because of the traditions of the discipline as developed by Griffith Taylor and Macdonald Holmes. Furthermore, the human and physical specialists live together create their own interactive social environment capable of generating ideas and field projects. In this setting many geographers are involved in management, planning, resource or historical studies which necessitates integrative thinking of natural and human phenomena.

(b) The second asset is the growing availability of multisource data. Here we have to thank the physicists, the electrical engineers and computer scientists. Tools of remote sensing, for instance, provide ways of mapping the earth's surface and accessing vast amounts of data at different resolutions. Modern and future computers will store these data as vast geographic information systems. What we have to do is find ways of making sense of all these data, to define the relationships which presumably exist between them using these sensing and computational tools.

Geographers mostly opted out of the man/land field during the period of growing concern about the viability of the environment. They opted out because they were undergoing their own internal revolutions in seeking specialist credibility, in quantification and in being socially relevant. The 60s and 70s, however, yielded a degree of professionalism which optimists like me believe now provide the backbone for future developments in the discipline. Our students acquire skills and concepts which should enable them to use their imagination to look at interacting phenomena at whatever spatial scale, over whatever time scale, they choose. Geography for them will not be seen as a natural science and not as a social science. It will be an "integrative science" built on the foundations of specialist knowledge and encompassing earth-surface phenomena which involves both man and land.

To paraphrase Professor Mabogunje, the Nigerian past President of the International Geographical

Union, geography today finds itself concerned with describing and analysing physical and human activities and "with determining the extent to which environmental quality can be improved and how best to achieve such improvement" (1984, p.6). It has been a long and often tortuous haul from the days of Griffith Taylor and his concept of environmental determinism when the forces of man-land interaction were poorly understood. Even though " fif" could see the dangers, the threats and the consequences of misuse of a "hostile land", he was not the right man in the right era to have his ideas used by decision makers. Geographers today can play a role as a bridge between natural and social sciences thereby having an unequalled opportunity to use their rich pool of concepts and increasingly sophisticated analytical tools towards a much enhanced understanding of how to make the earth a better home for man.

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Department of Geography,
University of Sydney, N.S.W., 2006,
Australia.

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Science and Gambling: Psychological Perspectives*

MICHAEL B. WALKER

ABSTRACT. Explanations are examined concerning the reason that people engage in heavy gambling. It is argued that these explanations will be psychological rather than economic in content. Four prospectives are taken: the psychoanalytic account of gambling; gambling considered as an addiction; an instrumental learning theory account; and a cognitive perspective based on irrational beliefs. The implications for clinical practice in adopting these perspectives are described.

Gambling occurs in every culture and is recorded throughout history. However despite its widespread occurrence the motivation to gamble is not well understood. The purpose of this paper is to explore some of the theories proposed to account for and explain heavy gambling. It can be seen that the core explanation for heavy gambling will be psychological rather than economic in character. All legal and institutionalised forms of gambling in Australia are so constructed that the gambler should expect to lose money in the long run. On a purely chance basis the more frequently a gambler bets the more likely it is that loss of money will result. In Table 1 the percentages of investment returned in prize money are shown for the major types of legal gambling in Australia.

TABLE 1

The percentage of invested moneys returned to the public by the major forms of legal institutionalised gambling in Australia.

Form of Gambling	% Return
Poker Machines	85
T.A.B.	84
Lotteries	64
Instant Lotteries	64
Lotto	60
Soccer Pools	35

Since each dollar invested in gambling has an economic value of less than a dollar, the heavy involvement of people in one gambling form or another is difficult to understand in purely economic terms. Furthermore, it is not the case that people in general have a mistaken impression that they are likely to win money through gambling. In a large scale survey of gambling in Australia involving interviews with nearly 2000 persons, MacMillan (1985) found that 79% of those who gambled expected to lose money while only 17% expected to break even or make money. On the basis of figures such as these it would be reasonable to speculate that for most people gambling has some other value than purely the expectation of winning money. Interviews gamblers suggest that for many, gambling is fun entertainment or an exciting time (Kallick-Kaufmann, 1979; MacMillan, 1985). However, nearly all the persons interviewed in each case gambled less often than once a week and might properly be called low-frequency or social gamblers. In the

* An address to the Faculty of Science Centenary Celebrations, University of Sydney, 26th April, 1985.

MacMillan survey only 3% of the sample gambled two or more times a week on the TAB and only 2% reported gambling that frequently on Bingo. The implication of these figures is that only a small percentage of gamblers can be regarded as heavy gamblers. In a survey of 400 families sampled from two suburbs in Sydney it was found that only 10% of the sample spent 10% or more of their leisure time gambling while less than 1% of the sample spent more than 20% of their leisure time gambling. Thus estimates of the incidence of compulsive gambling setting the figure at 1% of the population (Blaszczynski, 1983) appear excessively high. Nevertheless, there exists a minority of gamblers who spend most of their leisure time in gambling activities. The losses incurred by these gamblers can be very heavy and for some the outcome of gambling is financial ruin. Furthermore, the losses suffered are not only financial but also social. Heavy gambling places excessive stresses on the family unit and may lead to the breakup of the home.

Since Australians are reputed to be the heaviest gamblers in the world (Haig, 1985; Caldwell, 1974) and given the increasing proliferation of legalised gambling activities in Australia, it becomes especially important to provide therapy and rehabilitation for those who have been ruined by gambling. However, effective therapy presupposes an understanding of the causes of heavy gambling. At this time there are several competing explanations for heavy gambling. The main forms that these explanations take can be set out and examined with respect to the available data concerning the phenomenon. Although the evidence is not conclusive, it will be seen that these explanations are not equally tenable or satisfying. Historically, the psychoanalytic perspective occurs first and will be dealt with first. Then will follow the gambling addiction perspective, the instrumental learning account, and the cognitive account.

THE PSYCHOANALYTIC EXPLANATION

It is perhaps fair to say that there is no single psychoanalytic account of heavy gambling. According to which writer is followed the problem may centre on regression to the oral, anal, or genital phases of development (Halliday & Fuller, 1974). Nevertheless, one particular theme underlies much of the psychoanalytic reasoning: the heavy gambler unconsciously wishes to lose (Bergler, 1958; Fuller, 1974). Losing is a self inflicted punishment brought about by guilt over incidents going back to early childhood. During the Oedipal phase the boy competes with his father for the affection of the mother. This rivalry may become extreme for some children who come to hate their father bitterly and wish to destroy him. The Oedipal conflict is typically resolved by the boy identifying with the father but, depending on the severity of the conflict, the boy may be left with residual guilt for having harboured such destructive impulses toward the father. The gambling venue becomes a father figure who punishes the child and thereby relieves the feelings of guilt. A similar explanation can be advanced for girls based on their rivalry with the mother for the father's affections.

The psychoanalytic assumption that the gambler unconsciously wishes to lose is difficult to refute since unconscious wishes are not accessible to the gambler. Nevertheless, the extreme elation expressed by many gamblers after winning is difficult to explain using this approach. Kusyszyn (1977), is an example of a writer who is not at all impressed by the unconscious desire to lose as an explanation: "This explanation is, of course, grossly inadequate and totally wrong for most gamblers." (Kusyszyn, 1977, p.23).

GAMBLING AS AN ADDICTION

Freud (1928) contributed the notion that gambling is an addiction. According to Freud gambling is a substitute for the primary addictive cycle involving masturbation. However, it has been argued elsewhere that the analogy between gambling and masturbation is incorrect and that cultural differences in the tolerance or repression of masturbatory practices are not paralleled by differences in the incidence of compulsive gambling (Walker, 1985). Nevertheless, the notion that gambling may be an addiction remains

popular and has been revived by the advent of the opponent process model of acquired motivation (Solomon & Corbit, 1974). The parallel between alcoholism and heavy gambling is frequently alluded to as Rankin (1982) states: "Not surprisingly, those who are involved with this problem conceptualize excessive gambling as a form of addiction and have borrowed heavily from the alcoholism literature, in particular, to guide their thoughts and their practice." (Rankin, 1982, p.185).

A drug addiction may be defined as the regular self-administration of a drug which activity is outside voluntary control. It is clear that, by definition, gambling is not a drug addiction. Nevertheless it is possible to compare the characteristics of heavy gambling with the characteristics of drug addiction in order to determine whether they match. A basis for examining this comparison can be found in the opponent process model advanced by Solomon (1980). According to Solomon, the action of opponent processes underlies all behaviour with associated positive or negative affect.

The opponent process model of affect assumes that affect is controlled by a negative feedback loop. When a stimulus causes an affective state (state A) which may be either positive or negative an opposing process is set in motion which acts to dampen the response to the stimulus and reduce the intensity of state A. When the stimulus is removed the opponent process remains for a while and causes affective state B which is opposite in affective tone to state A (affective contrast). The a process underlying state A is typically rapid in onset and offset but becomes less intense with use (habituation or tolerance). The b process underlying state B is assumed to be typically slow in onset and slow to extinguish. Initially it is weak but with repeated use it increases in strength. These two processes with the characteristics ascribed are sufficient to describe the basic affective phenomena of addiction.

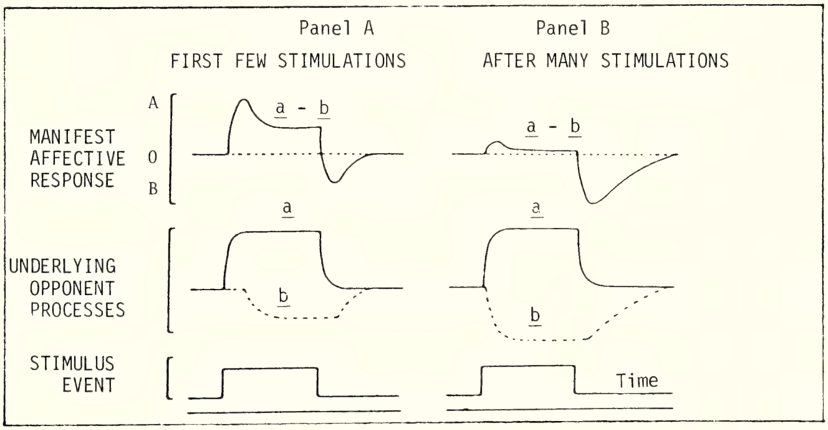

FIGURE 1. The comparison of the effects of b processes for relatively novel unconditioned stimuli and for unconditioned stimuli that are familiar and have frequently been repeated. (Note that the strengthening of the b process is assumed to shorten its latency, increase its asymptotic value, and lengthen its decay time.) (Solomon, 1980, p.700).

In Figure 1, the model is presented diagrammatically. In panel A the a process is strong while the b process is relatively weak. As a result state A is experienced intensely while state B is relatively mild. However, after repeated stimulation the state of affairs is as shown in panel B. The a process is now relatively weak while the b process is strong with early onset and a long period of extinction. State A is now weak and attenuated whereas state B is powerful and enduring.

The effect on the individual of these changes in the opponent processes is dependence. Initially the drug was self-administered in order to obtain state A. However as state A decreases in strength with repeated dosage and state B gains in strength the motivation for continuing with the drug becomes avoidance of the B state (abstinence agony). The addiction is now complete. In order to break the addictive cycle it would appear that sufficient time must pass without use of the drug so that the b process can dissipate. Unfortunately, not only is this period of 'cold turkey' extraordinarily painful (withdrawal syndrome) for the addicted individual but also as the b process declines in strength the power of the drug to elicit the a process increases and use of the drug again becomes attractive.

If gambling is an addiction then the phenomena of affective contrast, tolerance, and a withdrawal syndrome should be observed in heavy gamblers. Unfortunately, there are few systematic studies and thus most of the evidence is provided by case analysis. Affective contrast has been reported many times (McGlothin, 1954; Victor & Krug, 1967; Gooney, 1968; Bond, 1974; Livingston, 1974; and Dickerson, 1979). The feeling elation that precedes and accompanies the gambling behaviour is followed by a period of dejection and guilt.

Affective tolerance refers to a change from intense pleasure early on in the activity to mild or non-existent pleasure after the activity has been repeated many times. Custer (1977) reports that, with reference to compulsive gamblers: "When these people are gambling they're on as much of a high as if they were on a narcotic. Now in the late stage those highs aren't anywhere as high as they used to be." (Reported by Shubin, 1977, p.2). Similar observations have been made by Barker and Miller (1968), Cohen (1972), Livingston (1974), Cromer (1978), Dickerson (1979), and Greene (1982).

Perhaps more than any other phenomena it is the occurrence of distressing withdrawal symptoms which characterises drug addiction and which makes an opponent process analysis of addiction so powerful. The withdrawal symptoms following cessation of ingestion of opiates include trembling and shaking, heart rate and blood pressure changes, sweating and temperature changes, and difficulty in sleeping. The existence of a withdrawal syndrome following the cessation of heavy gambling has been investigated in a more systematic way by Wray and Dickerson (1981) using a questionnaire survey of one third of regular members of Gamblers Anonymous in Britain. Half of the subjects responded to the open question, "Describe how you felt in the first few weeks after stopping." The other half of the subjects were given a checklist of possible symptoms and asked to indicate whether they felt any of them more than usual when they gave up gambling. The results from the two approaches were consistent. About one third of the sample reported withdrawal symptoms involving negative affect. These included 'miserable', 'very irritable and bad tempered', and 'sick in my stomach'. The remainder reported feeling happy at giving up. The thirty percent with a negative withdrawal syndrome compares well with approximately forty percent who suffer withdrawal symptoms when they give up alcohol (Wray & Dickerson, 1981).

Taken together the evidence suggests that the affective dynamics of heavy gambling are consistent with the description provided by the opponent process model. However, if the opponent process model is correct then more detailed predictions of physiological changes follow. For example, if increased arousal is associated with the elation that accompanies gambling then systematic differences should be found in the arousal response to gambling between low frequency and high frequence gamblers. The action of the b process will cause a lower arousal response among high frequency gamblers relative to low frequency gamblers during the gambling activity (tolerance). After cessation of gambling the stronger b process in high frequency gamblers will cause a greater decrease in arousal below the baseline level than will occur for low frequency gamblers.

Leary and Dickinson (1983) examined arousal changes in two groups of subjects as a result of three minutes spent playing a poker machine. One group of subjects were high frequency players (played three or more times per week) while the second group were low frequency players (played only occasionally). Heart rate data was collected for both groups once every minute over four minutes preceding play, three minutes during play, and four minutes after play. The results are shown in Figure 2 and are clearly discrepant from the predictions derived from the opponent process model. The high frequency group were more aroused both during play and after cessation of play. Furthermore, the drop in heart rate as a result of stopping play is approximately the same for both groups.

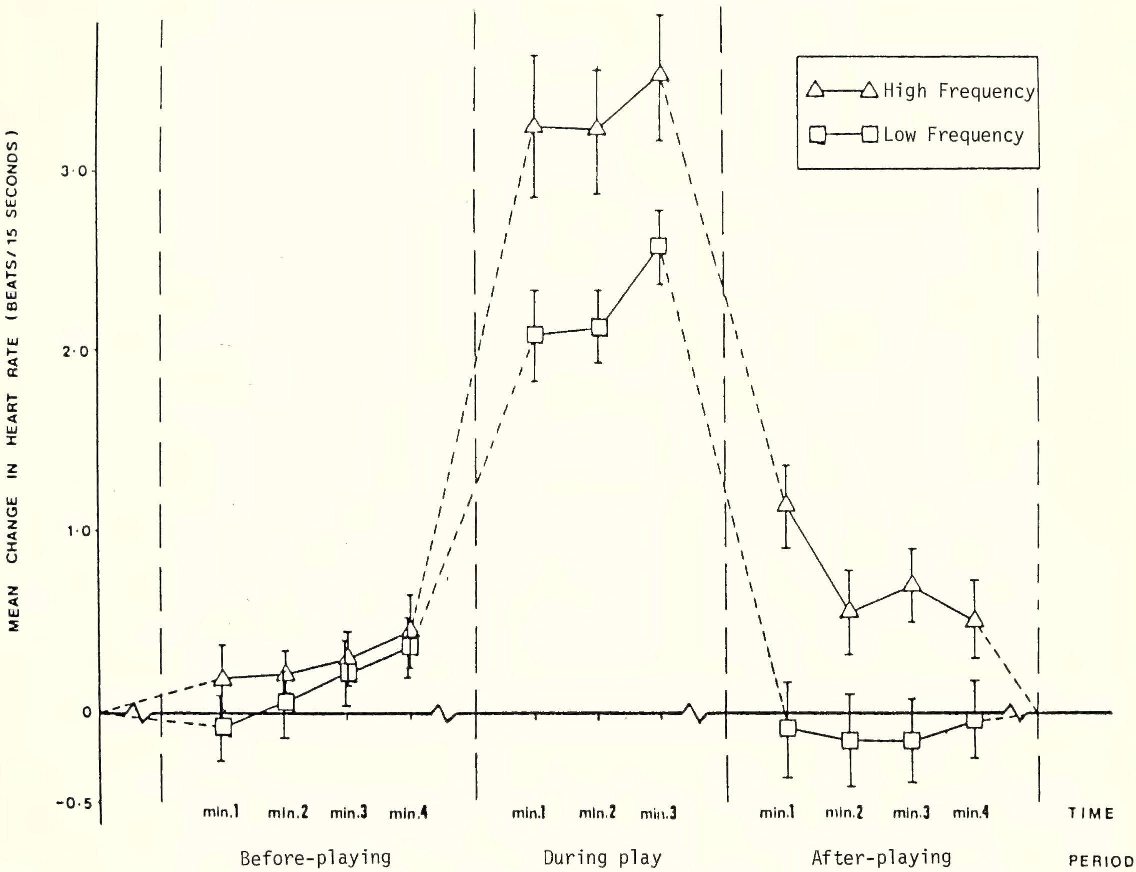

FIGURE 2. Change in heart rate (beats/15 seconds) for the low- and high-frequency poker machine playing groups. Mean ±S.E. (from Leary and Dickerson, 1983)

In summary, although case study data support the opponent process addiction model of gambling, the work of Leary and Dickerson (1983) involving the measurement of arousal before, during, and after gambling does not conform to predictions drawn from the model. It is possible that the discrepancies between the theory and the data can be explained by the operation of other processes such as Pavlovian conditioning of the initial state of arousal. However, until such processes can be demonstrated the outlook for the addiction model of gambling is not promising.

AN INSTRUMENTAL LEARNING THEORY APPROACH

From a completely different theoretical perspective, Skinner (1953, 1974) has suggested a mechanism which would explain heavy gambling. According to Skinner, gambling is an excellent example of a human operant response conditioned by a variable ratio reinforcement schedule. Taking the poker machine as the paradigm case, the lever pulling behaviour of the gambler is reinforced every so often, depending on the completion of a variable number of pulls on the handle. Ferster and Skinner (1957) demonstrated with animals that variable ratio reinforcement schedules strongly maintain behaviour and that the conditioned response established by such a schedule is very difficult to extinguish. Thus an attractive analogy can be made to heavy involvement among humans with the poker machine and this analogy is often cited, as evidence for the explanatory power of learning theory, in elementary psychology textbooks. For example, Buss (1978) asserts, "The best example of variable ratio schedules may be found in places like Las Vegas. Slot machines are programmed to pay off on a variable ratio schedule. Predictably, gamblers respond by maintaining extremely high rates of responding ... Compulsive gamblers, seen in this perspective, are merely victims of a variable ratio schedule of reward." (Buss, 1978, p.181).

While the analogy to pigeons pecking discs or rats pulling levers may be attractive when considering people in clubs and casinos operating poker machines, there appear to be good reasons for remaining sceptical about this explanation of gambling behaviour. First of all, although the analogy may be close for gambling on poker machines, it is less obvious for other forms of gambling. In a recent survey of a small sample of Sydney residents conducted by my students, several people responded by saying that they had been buying lottery tickets regularly for some time (in one case, more than two years) and had yet to win a prize. In general, the ratio of reinforcement is far too large in the case of lotteries to explain the popularity and persistence of people in this form of gambling. Secondly, the analogy of poker machines to Skinner boxes is not as close as Skinner and Buss have suggested. Pigeons and rats condition very strongly on variable ratio schedules, but they do not have the continuous schedule of punishments suffered by poker machine players. And it is not simply that the poker machine player pays money for the chance to be reinforced, but at the end of the session nearly all such players will have lost more than they have won. The whole activity is punished repeatedly. Skinner is aware of this argument and asserts that nevertheless, "The ultimate loss (the 'negative utility') does not offset the effect of the schedule." (Skinner, 1974, p.56). However, this is far from satisfactory, since the heavy gambler loses more than money. The long gambling sessions cost time and inevitably lead to the gambler being less effective in his or her other roles in life. Frequently the heavy gambler is punished by family and friends (Kusyszyn, 1978), receives complaints about his or her work (Greene, 1982), and inevitably faces serious financial difficulties. Yet the gambling persists. If the variable ratio reinforcement schedule is so powerful that no other reinforcement contingencies can disrupt the activity, then one would expect nearly every person who makes a bet to become entrapped. Yet the large majority of gamblers are occasional gamblers who spend relatively little time in the activity (Downes et al., 1976; Kallick-Kaufman, 1979). Finally, Cohen (1972) has demonstrated that the actual behaviour of roulette players fails to be consistent with the expectations of the operant conditioning model. In Cohen's study the subjects were asked to bet only on black or red. The important comparison is that between the effect of a correct choice (a win) on the next choice and the effect of an incorrect choice (a loss) on the next choice. Simple operant conditioning principles would lead to the expectation that a win (on red, for example) would increase the likelihood that the same colour (red) would be chosen for the next bet, whereas a loss (on red, for example) would decrease the likelihood that the same colour (red) would be chosen for the next bet. In fact, the likelihood of choosing the same colour after a win (0.46) was less than the likelihood of choosing the same colour again after a loss (0.76). Of course, what Cohen was demonstrating was the well known gambler's fallacy that a loss this time makes the same choice a better bet next time. Such gambling behaviour is difficult to explain using operant conditioning principles.

While these objections make it plain that conditioning and reinforcement explanations are likely to be inadequate for some aspects of gambling, there is yet another reason why we should dispense with these behaviouristic analyses as a core explanatory mechanism. Such analyses deny a role for the gambler's plans, hopes, goals and motives. Also, as with the psychoanalytic notions, they deny a central role for the gambler's rational and conscious processes. Yet it is known that at least for some gamblers, actions are calculated and reasons can be given in advance (see, for example, with respect to Poker: Yardley, 1959; or Hayano, 1978). Thus, it may prove more effective, in deriving an explanation for heavy gambling, to analyse the interaction of the gambler's actions and explanations taken together in the context of the gambling situations as perceived by the gambler.

A COGNITIVE PERSPECTIVE

According to the cognitive view, heavy gambling is not driven by unconscious masochistic urges nor is the behaviour out of control in the sense of an addiction. The gambler is not a pawn of a vicious reinforcement cycle. The core of the explanation for heavy gambling lies in the irrational beliefs held by the gambler and the support those beliefs derive from a biased evaluation of gambling outcomes made by the gambler. Walker (1985) has documented two central beliefs held by heavy gamblers: (1) That through effort and skill a person can make money through gambling; and (2) That the heavy gambler himself or herself has the ability to win in the gambling situation.

It is true that the skilled Bridge player or Poker player can consistently make money over time and in the United States the lives and details of professional players have been documented (Hayano, 1977). However, the proportional influence of skill decreases as one moves from Bridge and Poker to the TAB and Poker Machines. Nevertheless, the heavy punter at the race-course believes that he or she has the knowledge and skill to get an "edge" on the bookmaker. Heavy gamblers believe that they are better than the average "mug" punter. If it was true that everyone who bets with the bookmakers or the TAB loses in the long run then it would be more difficult for a gambler to believe in the possibility of an edge. However, there are successful professional punters at Australian racecourses (Scott, 1978). Similarly, there are professional gamblers in Casino games (Thorp, 1966; Hayano, 1977), there are claims of professional success (Leigh, 1977), and there are fictional works supporting the notion of the successful professional (Jessup, 1963). It is not surprising therefore that some gamblers may believe that they, too, have the means of winning.

It is not clear how the heavy gambler acquires the belief that he or she has the knowledge and skill to "beat the system". However, one important factor may be the occurrence of substantial wins early in their gambling careers. The early success factor has been noted by several writers (Livingston, 1974; Lesieur, 1979; Custer, 1982). Presumably, the gambler at this early stage is trying out various approaches to choosing their bets. Some large wins would confirm that a successful technique had been found and might implant the idea that they will be among the few who can beat the system. No matter what the cause of this belief in personal skill, the belief itself is very important to the gambler. Hence, a succession of losses will be a very threatening event. In order to preserve the belief in their own abilities these gamblers must now bet successfully in order to recoup what has been lost. Lesieur (1979) has called this phase in the development of heavy gambling "the chase". Cohen (1972) has suggested that chasing losses is the major difference between the occasional punter and the heavy gambler: the occasional punter may regret losses but soon forgets them whereas for the heavy gambler losses are unacceptable since they invalidate the gambler's self-concept.

The occasional wins that come to the heavy gambler are taken as signs that ultimate success is near. The more frequently occurring losses drive the gambler to greater and greater investment of time and money

and ultimately to financial ruin. Perhaps the surprising aspect of this vicious circle of events is the fact that the evidence that the gambler is not being successful is discounted. The gambler preserves the belief that their system or approach is successful, despite the loss of money, by forming biased evaluations of gambling outcomes (Walker, 1985). The belief that skill is being used in the placement of bets has been demonstrated by Langer (1975) while the belief that the outcomes of bets evidence that skill has been demonstrated by Gilovich (1983). Langer sold raffle tickets and compared those who chose their own ticket with those who were simply given the next ticket available. Those who chose their own ticket valued the ticket more highly and would only part with the ticket for several times its original value. Langer has called this phenomenon "illusion of control" and suggests that gamblers believe they have more control in obtaining desirable outcomes than is in fact the case. Gilovich examined the evaluations made by gamblers betting on the outcomes of basketball and baseball games in the United States. Particularly where the result of the game was close, those who bet on the winning team evaluated the outcome as showing their skill in team selection while those who bet on the losing team blamed bad luck and other external and unforeseeable features of the environment for their error. Thus, even support for the losing team could be construed as evidence of skill in selection which was thwarted by bad luck.

The reason why the heavy gambler does not quit when it is evident that his or her gambling strategy is failing can be summarised by saying that successes confirm the gambling strategy while failures are attributed to external features (for example, bad luck) and thus do not disconfirm the gambling strategy. Using such biased evaluations the gambler can maintain the illusion of eminent success while losing large sums of money.

COMPULSIVE GAMBLING: IMPLICATIONS FOR THERAPY

Gamblers Anonymous is an organisation which seeks to help those whose lives have been ruined by excessive gambling. It is run on similar lines to Alcoholics Anonymous. According to Gamblers Anonymous, compulsive gambling is an illness, progressive in its nature, which can never be cured, but can be arrested. This view of compulsive gambling, which is often called the disease model, provides a baseline for comparison with the four perspectives described earlier. The disease model suggests that excessive gambling derives from a personality defect which continues throughout life. It is similar therefore to the psychoanalytic view. Successful therapy must be based on total abstinence from gambling as is urged by Gamblers Anonymous. However, the psychoanalytic perspective suggests pessimism about the successfulness of any therapy. If the heavy gambler has an unconscious wish for punishment that is fulfilled by heavy gambling then cessation of gambling must lead to some other punishing activity such as petty crime or drug addiction.

According to the addictive model of gambling the compulsive gambler is not sick so much as entrapped by the desire to avoid the negative feelings associated with not gambling (the B state resulting from the b process). It would seem that the only way to break the addiction would involve a period of abstinence sufficient for the b-process to diminish and disappear. Unfortunately, in losing the B state the individual regains the potential for an intense A state and must remain vigilant against resuming the addictive cycle. Nevertheless, the full A state can be experienced provided that sufficient time is allowed to elapse before the A state is again invoked. What this suggests is that total abstinence may not be necessary but rather a controlled involvement in gambling may equally prevent the spiral into excessive gambling. Certainly, case studies in which controlled gambling established through contractual agreements has been the therapy have been reported and the successful control of gambling has been achieved over a two year follow up period (Dickerson & Weekes, 1979; Rankin, 1982). Unfortunately, despite these two successful case studies, the outlook for controlled gambling must remain bleak according to

Solomon's theory. If controlled gambling is to remain effective the frequency and extent of gambling must be sufficiently spaced so that any b-processes initiated are extinguished. If for any reason the control over the gambling is lost and the gambling becomes heavier then the b-processes will gain in strength and the spiral into addiction be initiated. Perhaps on grounds such as these the ethic of Gamblers Anonymous that complete abstinence must be combined with day-by-day vigilance is to be preferred.

According to the learning theory approach, it is the incentive value of the occasional win which maintains the gambling behaviour. Thus, in principle, any person might become a compulsive gambler. What prevents most occasional gamblers from progressing to heavy gambling and ultimately financial ruin is presumably the other reinforcement contingencies operating in each person's life. Thus, similar to the addiction model, the learning theory approach does not imply that the compulsive gambler is sick but merely unfortunate. The therapy appropriate to the behaviour problem will be one that extinguishes the stimulus to gamble. Little can be done to alter the relationship that exists between the gambling response and its reinforcement. However, the stimulus to begin gambling may be the environment in which the gambling takes place. This conditioned stimulus-response connection could be extinguished by having the gambler avoid gambling while in the stimulus environment. Alternatively, a more convenient therapy would involve the compulsive gambler imagining being in the gambling environment but feeling bored and uninterested in gambling. Such a procedure is called imaginal desensitization and has been used successfully in the treatment of compulsive gambling by McConaghy, Armstrong, Blaszcynski and Allcock (1983). However, such therapies are not effective for all compulsive gamblers and at this time no adequate follow-up data is available to determine whether these therapies are effective in the long term.

The cognitive perspective, similar to the learning theory approach, does not view compulsive gambling as an illness but rather as a tragedy. Each person engages in a variety of life projects. These projects become the arena in which beliefs about oneself and one's capacities are tested. For one person, the central project in life will be the garden, for another the family, and for yet another work. Each project provides the opportunity to validate the core beliefs about self that each person holds. The tragedy of the compulsive gambler is that he or she has chosen an arena in which failure is overwhelmingly likely and consequently the core beliefs invalidated. Therapy will consist of persuasion to abandon the irrational beliefs held about self in relation to gambling and to replace these beliefs by factual information. Since the loss of what is for many compulsive gamblers a life consuming project will leave a void, therapy must also attempt to rekindle interest in abandoned projects or to stimulate involvement in new projects. At this time there is no available evidence concerning the effectiveness of such therapies.

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Dept. of Psychology,
University of Sydney, N.S.W., 2006,
Australia.

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Genetic Engineering — By Man For Man*

W. J. PEACOCK, FRS, FAA

In the past few years biologists have learned to cut and join DNA in a controlled manner. DNA is the molecule which codes our genetic makeup. It contains the blueprint for our development. Each cell in our body contains 46 chromosomes and each chromosome is one long DNA molecule with genetic message enough for 1,000 genes. The gene is the unit of the genetic language - a language of 3 letter words and a 4 letter alphabet.

We can now isolate a gene, study it, change it, and in some organisms reinsert it into the chromosome so that it functions properly again. Also we can join a gene from a human or a wheat plant into the chromosome of a bacterium or vice versa.

This recombinant DNA technology is the basis of genetic engineering - the controlled manipulation of the genetic constitution of a living organism. Such manipulation almost always involves one or more processes carried out in a test tube in a laboratory.

Genetic engineering has revolutionised biological research. This is certainly the age of biology, not of computer science or atomic physics. Genetic engineering has also revolutionised the public's perceptions of biological research. It is an emotional subject. Often the emotions are fear and worry, this, frequently, because of misconceptions and misinformation.

Genetic engineering, or molecular biology in a broader sense, is my science, and I'd like to share some of that science with you tonight and I hope appraise you fairly of what it is and what problems there are in the way in which it interacts with us and our society.

Genetic engineering will soon impinge in many ways on our daily lives. A family at breakfast may eat a breakfast cereal which has an advertisement on the packet stating that the cornflakes are amino acid enriched - this having been achieved by genetic engineering to adjust the proteins in the seed of the corn crop. They may then have some bacon which may have come from a pig production system where the rate and extent of growth of the pigs had been enhanced by the provision of pig growth hormone; which had been produced in a bacterial culture fermentation system, the bacteria having received a pig growth hormone gene by genetic engineering. It may well be that one of the family is a diabetic and has to regularly take insulin - the insulin will no longer be that isolated from pig carcasses but will be human insulin, again produced in a fermentation system - perhaps by yeast cells, perhaps by animal culture cells. The cells will have had the human insulin gene introduced by genetic engineering methods.

One of the family may that day be joining the blood donor scheme and will have a test for AIDS by a test kit which has been produced by genetic engineering methods. That person also may be vaccinated against hepatitis, the vaccine in this case being a safe and efficient vaccine, produced by genetic engineering methodology.

Perhaps the mother in the family is pregnant and will that day be having an amniocentesis test which will enable screening of the foetus, not only for major chromosomal disorders as is currently the case,

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* Address to the Faculty of Science Centenary Celebrations at the University of Sydney on 14 May, 1985.

but which will also permit screening for a number of inherited diseases. The early detection of such diseases may sometimes warrant termination of the pregnancy but will frequently allow the correct early nutritional or other treatment to be given which will considerably ameliorate the effects of any such disease.

None of these things sound particularly frightening, in fact they obviously are going to be improving the quality of our lives. So what is it about genetic engineering that at some times and for some people makes them think that it is terrifying, whereas I would say it's terrific. Well, first of all let me tell you a little bit about what it is and exactly what happens. It is not, as many people think, the cloning of animals or parts of animals. The only cloning that is done in genetic engineering is the cloning of a specific piece of genetic message.

About 10 years ago the only way a person like myself could study a gene, from man or from a wheat plant, was to work with the whole population of genes in a nucleus. Now we have a lot of DNA, you may be interested that if we were to take all of the DNA that is in the cell nuclei of our body and stretch it end to end, that line of molecules would stretch to the moon and back several times. It really is an enormous total amount of DNA and even within one nucleus there is something like several hundred thousand genes-worth of genetic message.

So you can imagine what a blinding light it was when two things happened which enabled us to pick out a specific piece of genetic message and work with it. One of the two tools provided were enzymes which cut DNA at certain sequences coded along the molecule. These are called restriction enzymes which are used by bacteria to protect themselves from other pieces of DNA that enter through their cell walls. These restriction enzymes chew up foreign DNA, yet because of certain modifications to the recognition sequence they do not touch the DNA of that host cell. There are a whole variety of restriction enzymes and they have provided us with a series of precise genetic scissors.

The other major finding was that of small circles of DNA in bacterial cells. These are called plasmids. They have the ability to replicate, to multiply in the bacterial cell in large numbers. These, too, can be cut by genetic scissors in a controlled way. It's possible to join in other pieces of DNA in rebuilding that circular plasmid. By putting on one particular DNA segment we can multiply it up in a bacterial culture as much as we want and purify large amounts of the segment to study and use in various ways.

Those two discoveries sound very simple. They are. So simple and so important that each of them have won Nobel Prizes for their discoverers.

These days it's possible to read the genetic message of a piece of DNA very quickly. It is also possible to modify the genetic message in a controlled way, and for a number of animals and a number of plants it has been possible to take engineered DNA - isolated DNA segments - and reintroduce them back into a living organism so that the gene segment is working properly and becomes part of the genetic blue-print or programming of that individual organism.

I think now it might be best if I describe to you some of the things that have been found out and give you an idea of what it is possible to do with these techniques.

Let me first talk about some findings with plants, which is my own research area. You may remember that last year Barbara McClintock received the Nobel Prize for work that she did nearly 40 years ago in which she deduced that there were certain genes in the corn plant, and probably in all plants, that did

not have a regular place in a chromosome. Generally we think of genes being in a strict order along a chromosome. These special genes were able to move, sometimes with quite high frequency from one place to another. They are called transposing elements, or we might call them 'Jumping Genes'. For quite a long time little notice was taken of McClintock's seemingly heretic views, however, recombinant DNA technology has enabled us to confirm her conclusions and just a little over two years ago in Canberra we isolated the first jumping gene of maize. Thus the jumping genes are now a true physical entity and we have studied them a great deal, as have a number of other laboratories around the world; they are proving to be extremely useful in research. For example, since they can move themselves from one location to another in a chromosome, they presumably could help us to introduce genes back into plant chromosomes. If we were able to stitch in the gene we wanted to introduce into the middle of one of these jumping genes that we'd isolated, then it may well be able to provide us with a vector or vehicle for taking that gene into a target plant.

Another use is that when these jumping genes move around the chromosomes of an organism, if they land in the middle of a working gene they will inactivate it and turn the gene off. This causes a mutation and it provides us with a way of finding a gene which has a particular function. We can follow the jumping gene and when we find a mutation in the function we're interested in then we can go in, pick up the jumping gene and know that the sequence on either side of it is the gene we want. This has now been practised in maize and I anticipate will be used in many other plant species.

I've talked about the introduction of genes into plants. This has already been achieved in a number of plant species. It has been done in two ways. One way uses a disease organism, the soil bacterium that causes Crown Gall disease in a number of plants like peach trees, tomatoes, potatoes. This bacterium, we have found out by using recombinant DNA methods, regularly inserts a part of its chromosome into the plant chromosome and that's what causes the cancer - it's really an oncogene causing uncontrolled growth - the Crown Gall. We have been able to take that piece of DNA and remove the tumour-causing functions from it but use its other biological properties, that is the way in which the bacterium enters the plant cell and knows how to introduce the segment into a plant. So we now have a number of instances where a bacterial gene or another plant gene has been introduced into a target plant in a way that makes a stable addition of that gene to the plant's genetic make up.

I'd like to give you just one example of this process because it points out another extremely important phenomenon that we've learned about. The French bean, the bean seed that you eat, has a lot of storage proteins, and one of the major storage proteins in the French bean is called phaseolin - that's after the name of the bean plant. The gene coding for that storage protein has been isolated by recombinant DNA methodology and has been joined into the inserting region of the Crown Gall bacterium chromosome. It has, for example, been put into the tobacco plant which is used a great deal in laboratory experimentation because of its ease of culture. The important feature was that the gene did not work in the tissue that grew immediately after the infection, nor did it work in the stem tissue of the plant that grew out of the infected callus tissue, nor did it work in the leaves or the flowers. The only time that gene was switched on was in the seed of the tobacco plant, which is where it normally functions; it operated at the time of development and in the tissue that it would normally work in the bean plant. What this has told us is that just in front of the genetic coding region for the storage protein gene there is a segment which contains the control switches which determine when that gene will be switched on, both in developmental time and in what tissues. This is very important because it means that if this is a general situation, and it certainly appears to be the case, it simplifies the task of the genetic engineer to introduce genes into a recipient target species. A lot of the correct control is automatically coded for in the segments immediately adjacent to the gene.

This is true too not only for genes that are switched on at a certain time of development but for genes that respond to certain environmental signals. In fact, if some of your cells are exposed to extremely hot conditions, most of your genes will switch off and only a certain array of them - so called heat shock genes - will be turned on. It has been shown that that is due to a particular switch region in front of the genes - the heat shock switch.

Plants have exactly the same response and in fact it's almost exactly the same switch code in animals and plants. There are other coded messages in plants which switch some genes on in leaves only when light shines on those leaves, which makes a lot of sense. You don't want some of the genes involved in photosynthesis working in the night when there's no sunlight - they only switch on when light actually shines on the leaf.

Just recently at a meeting in America I learned of another way of introducing genes into plants. This is where the DNA preparation can be allowed to pass through the membrane of a naked plant cell; the cell wall can be removed, to produce what is called a protoplast with just a cell membrane. DNA can enter through that membrane and become incorporated into chromosomes. For some plants, where a whole plant can be induced to regenerate from a single protoplast or cell - the protoplast will re-grow a cell wall and go into division again - this promises to be a very powerful technique.

The really quite fantastic things that we have begun to learn about plant genes and their properties, of their structure and the way in which their expression is controlled - all of this has happened in the last couple of years and is very exciting for a person like myself. Couple this with the ways of introducing genes into plants and you see that we're in the process of developing another method of plant breeding which will supplement the existing methods and make us better able to tailor plants to our needs. This is a very exciting prospect. For example, although recombinant DNA genetic engineering will probably be restricted for many years to the introduction of single genes, or at most a few genes, that still gives some hope that we will be better able to equip crop plants against diseases and pests and other problems where we know that single genes can be of importance. There's a long way to go but it's a very fast moving field.

We might also be introducing quite novel genes into plants. I want to give you one example. There's a bacterium which produces a protein that is a strong insecticide. In fact, this protein, inside the bacterial spores, is already sprayed in some agricultural operations, for example, to protect cotton crops against the moth pest *Heliothis*. It is a difficult process to produce enough of the bacterial spores and then all the conditions have to be just right in order to get an effective spray. What many labs around the world are doing now is to take the gene from the bacterium, the gene that codes for that protein, and it has been isolated, and introduce it into a plant so that the plant will make its own insecticide. Of course it would be ideal to equip the gene with a switch mechanism which switches on the gene only in the leaves, or in any other parts of the plant we want to protect from insect attack. It's a very exciting possibility and is well under way in trials.

Perhaps I should now turn to some examples of what is happening with animals. The molecular biology of animal genes is even further advanced than that for plant genes. There have been quite remarkable discoveries. I always think that our current understanding of the mechanism of antibody production, the tremendous variety of antibodies that we produce to protect ourselves against foreign proteins, is a tour-de-force of the recombinant DNA method; we now have an understanding of much of this process at a molecular level. It is interesting to not that this is one of the cases where nature has used jumping genes in a controlled way to produce a lot of variation. I can't go into that in detail tonight.

Recombinant DNA methods have also resulted in enormous increases in our understanding of the causes of many types of cancer.

We also now understand the basis of many inherited diseases. For many of the diseases that are associated with abnormal haemoglobin, for example sickle cell anaemia and thallasemia of one type or another, we now know precisely what error or change has occurred in the genes producing haemoglobin. Sometimes the change is in a single letter of the genetic code, one nucleotide altering the code for an amino acid in the haemoglobin protein. Sometimes the error is a much larger one, with a whole piece of the gene missing.

We also know a lot about the way in which animal genes are controlled, and again there are specific switch regions, specific control regions coded into the DNA, which cause many genes to be turned on or xeroxed in mRNA at particular times in the cell cycle or in particular tissues at particular stages of development.

This knowledge is helping us to design ways in which to treat diseases. We have at hand a great increase in our potential to diagnose certain diseases; some cloned genes can act as probes to determine whether an individual has a certain disease or not. I mentioned at the beginning of my talk the amniocentesis procedure which allowed certain inherited diseases to be scanned for in the blood cells of a foetus.

Apart from this great power of diagnosis there is the opportunity for therapeutic uses of recombinant DNA. On the one hand genetic engineering is being used for making better vaccines, especially in producing vaccines against disease where normal methods of vaccination have not been successful. We can now dissect a virus genome and find out which part of the genome is important in permitting vaccination against the disease caused by that virus. Hepatitis is one vaccine being produced in this way - it will come onto the market quite soon.

One can also ask about more direct gene therapy. What if we discover that an individual has an imperfect haemoglobin gene - can we overcome this imperfection by introducing a normal gene into the bone marrow cells? The answer would seem to be yes for a number of diseases. But this is where we immediately run into a high emotional context for recombinant DNA techniques. Should we be interfering and controlling the genetic makeup of man? Let me first of all discuss one or two issues about this. Are we able to introduce genes into animal cells? Yes, we can. They certainly can be introduced into cultured animal cells in the same way as for naked plant protoplasts, but that's really an experimental tool. We cannot grow animals back from cultured cells, whereas plants can often be grown back from single cells. If we wish to modify an organism and have that organism pass the gene onto future generations, then the modification, the addition of the gene, has to be in the germ line. Has this been done in any animals? Yes, it has. It has been done, for example, with mice. One example is that a rat growth hormone gene has been successfully introduced into the chromosomes of a developing mouse embryo which then developed normally, reproduced and passed the gene on to future generations. If that gene is equipped with the right sort of control mechanism, then it can lead to the production of bigger mice than normal. But it depends on what switch gear is attached to the gene.

Scientists in CSIRO at Prospect here in Sydney are attempting to introduce genes into developing sheep embryos by this same sort of technology. You can see that if disease resistance genes could be added into sheep embryos then it could be advantageous for our wool and fat lamb industries.

What about humans? The technology is not yet developed for introducing genes into human embryos. We have no way at the moment of successfully introducing a piece of foreign DNA into a human embryo, although I believe that technically this does not present a huge task. I expect it could, and probably will be solved, in the next few years. I will come back to that issue and the ethical considerations that it most certainly introduces. But before I consider that I want to discuss the other general type of gene introduction into humans.

This is where we will introduce a gene into somatic cells, the body cells; the gene won't be passed on to future generations. It will only be a manipulation of the genetic make-up of the organism in particular tissues. There are already proposals being considered by authorities in the United States where it is proposed to introduce a normal gene to bring about an amelioration or cure of a disease caused by an imperfect gene. It is not yet possible to cure a number of the haemoglobin diseases because of the complex nature of the control of gene expression of these genes - that's another bit of knowledge that has come from the use of recombinant DNA techniques. But there are certain diseases where gene introduction may help. For example some you may have heard of the Lesch-Nyhan Syndrome caused by one deficient enzyme (HPRT - hypoxanthine-guanine phosphoribosyl transferase) in the body. It leads to a quite horrible disease. Fortunately it's rare. Any individual affected by this disease tends towards self-mutilation - biting off fingers and lips and other extremities. These individuals have a miserable existence and usually have to be under extreme restraint. If we could introduce the gene coding for the normal enzyme into the bone marrow cells of the suffering individual it could bring about a cure.

There are other examples where similar gene therapy may be a humane and effective answer to disease.

Let me consider a few of the questions which surround some of the prospects for recombinant DNA technology or genetic engineering. We certainly can be optimistic for the applications in developing new biotechnological industries. This applies to diagnostics for use in agriculture and veterinary medicine for example, and for the use of bacterial cultures, yeast cultures or cell cultures in producing proteins that otherwise are in extremely short supply. For human medicine there is promise of much increased diagnostic powers in the early detection of diseases. A corollary of this is more accurate genetic counselling. These all would seem to be highly acceptable in our society.

But there are other aspects that must be considered with regard to the increased diagnostic abilities. We may be able to determine that certain individuals are going to be prone to a late onset disease. Something that we really can't do now. A number of genetically controlled diseases have late onsets such as Huntington's chorea. We will now be able to tell by looking at the gene structure of a young person whether that individual is going to be prone to such a disease. Cystic fibrosis is another example.

Given that the potential for diagnosis is there, over what scale in the population should it be applied? Should we in fact be telling people that they will be prone to a late onset disease? Is it better that they don't know? Would such a diagnostic tool perhaps be misused in employment screens by potential employers? What about the way in which it might be used in determining insurance policy rates or suitabilities for insurance policies?

All of these questions are questions that must be faced. They have been met already for many similar issues. I don't think recombinant DNA technology is introducing any particularly new problems in this regard. But they are problems that have to be considered and talked out. Most of the control and cultural procedures are already in practice in our society and will be able to be applied to these new circumstances in the way that they have been applied to existing situations.

With respect to the ethics of gene therapy - this has been widely debated for many years and still needs to be further debated. I think it is very important to make the distinction between germ line intervention and somatic cell therapy in the way I have discussed. I believe that most of us would think it ethical to insert genetic material into a human being to medically correct a severe genetically caused disease. This form of somatic cell gene therapy does not have any different connotations, so far as I can see, from other medical techniques such as transplants. However, attempts to correct germ cells to enhance or improve a person by gene manipulation certainly would not have societal acceptance at present - and perhaps it never should.

The other thing I would like to stress is that somatic gene therapy will certainly be constrained to single gene disorders for some considerable time. There is no possibility of approaching more complex attributes such as intelligence or physical stature or anything like that, with any such therapy. These characteristics have complex genetic and environmental determinants.

The ethical problems are not restricted to gene therapy with humans. We must also consider the regulatory controls for release of genetic engineered plants or animals into the environment. Are there any untoward hazards of releasing an improved plant cultivar where the improvement has been brought about by recombinant DNA technology rather than solely by other means of plant breeding? These questions are being considered in this country and other countries at the present time and once again we must seek a wide variety of opinion, these opinions hopefully being based on the best possible factual data.

Genetic engineering has become possible because of a number of substantial break-throughs. Its impact in basic biological knowledge is already profound and its applications in agriculture, industry and medicine are just being realised. Human gene therapy, if approved for use, will be applied to patients who have no better prospect for treatment of their disease, which is caused by a defective gene. This is not genetic engineering being applied as a eugenic tool to "improve" the human gene pool.

There are 2,000 to 3,000 human genetic diseases, and as many as two percent of newborn infants suffer from a genetic disease. At present for most of these diseases the defective gene has not been located and isolated, but we can expect rapid progress in this area.

Gene therapy has been successfully carried out in the fruit fly *Drosophila*, and laboratory mice. It should be possible to correct some specific genetic defects in individual patients. The major question seems to me to be when to begin clinical trials and not whether to begin them at all.

William Bateson, a prominent English geneticist, wrote in 1902 in this Rediscovery of Mendel's work "... Determination of the laws of heredity will probably work more change in man's outlook on the world and in his power over nature than any other advance in natural knowledge that can be clearly foreseen". This statement has heightened emphasis these days with the advent of recombinant DNA technology. We can cut and stitch DNA segments no matter whether the segments are from plant, animal or micro-organism. We are now licenced borrowers of the books in the genetic libraries of nature.

In 1974 this science was born with a high degree of concern by the scientific community with regard to potential hazards of recombinant DNA molecules. This led to a lot of regulation surrounding experimentation. But it also resulted in a lot of work which showed that the concerns were somewhat naive and unsubstantiated. The risk assessment studies showed the techniques posed no special new and unanticipated hazards. We are now seeing consideration of risk acceptance with regard to the release of genetically engineered organisms. I believe there is still a danger of restrictive legislation, but

hopefully regulations which will be formulated will avoid rigid legislation. We cannot afford to suppress the application of our new knowledge of genes and how they work. We, the scientists, are not "playing God" but critics who call for the abandonment of this technology may well be doing so.

CSIRO Division of Plant Industry,
Canberra, A.C.T., 2600, Australia.

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"Scientific Sydney" — Introduction

As we approach the celebrations of the Bicentennial Year, few subjects can be at once so highly important, yet least understood, than the history of Australian science. A country which depends on so many ways upon the application of knowledge has, in the sciences, an enviable heritage. That heritage, the cultural legacy of our colonial past, is significant to both our sense of nationhood and our spirit of internationalism. Today, scholars are acquiring the materials that will give us a firmer grasp of those factors which directed the pursuit of knowledge in the early years of the settler colony, and which have ever since shaped the character of our scientific enterprise.

In this task, co-operation between historiansand scientists is essential. Technical expertise must be matched by historical perspective; institutional folklore must be placed in broader context; biographical anecdote must be sifted and weighed against comparative recrods. "Metropolitan" zeal for laboratory research must be juxtaposed against field traditions, and the image of science as a metaphor of reason and enlightenment must be qualified by its use as a "tool of empire". Above all, the economic and intrinsically political character of science in a new land of settlement must be understood. Science, like architecture, has shaped our lives, and we must now look more closely at its fine structure.

Rising to this challenge, the Council of the Royal Society of New South Wales, in collaboration with the Royal Australian Historical Society, agreed in 1984 to hold at "History House" a series of exploratory workshops devoted to the historical reconstruction of "Scientific Sydney". This series began in November, 1984, with a day devoted to "Artisans and Managers: Technical Education in New South Wales, 1884-1984". In the event, this occasion celebrated the centennial of the Sydney Technical College, and the sesqui-centennial of the Sydney Mechanics' School of Arts. Both these institutions cooperated fully, as did the Technical and Further Education Department of New South Wales which is soon to constitute its own museum in the city.

This first meeting, to which seven papers were delivered, was so well received, that it was followed in May 1985, by a day devoted to "Culture and Learning in the Colonial Metropolis". This second meeting heard five papers, of which three are presented in the following pages. Abstracts of all papers of both meetings are included as Appendices to this note.

In November, 1985, our third and, arguably, our most successful workshop to date was held to commemorate the centenary of Professor John Smith, foundation Professor of Chemistry and Experimental Philosophy at the University of Sydney. The papers from that session, as edited by myself, will be the basis of the next issue of the *Journal and Proceedings*.

To the Royal Australian Historical Society, go our warm thanks for their hospitality and enthusiasm. To Ms Christa Ludlow, go the thanks of all concerned with the series' organisation. And to the Royal Society of New South Wales, which enjoys a premier place in the history of science in this country, goes the appreciation of all who rejoice to see the treasures of "Scientific Sydney" thus re-discovered and made public.

Roy M. MacLeod,
Dept. of History, University of Sydney, N.S.W., 2006.

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Beneficent Providence and the Quest for Harmony: The Cultural Setting for Colonial Science in Sydney, 1850-1890*

GREGORY MELLEUISH

> The true notion of Providence is, that it uses moral beings, everywhere throughout its immeasurable realms, as its own instruments for the completion of its grand designs in ultimate futurity, without rendering those beings the less moral and accountable. And it is but consistent with the notion of a Providence so perfect and so absolute, that its designs should be at once beneficent and just. And thence must be inferred, in the words of Pope, considering everything in the light of an instrumentality in a supreme hand, that "whatever is, is best", yet so as that it shall not be best for the perpetrator, and within the contracted circle of his immediate connections, unless it be morally good and right.[1]

The sentences quoted above come from an editorial of the *Empire* newspaper published in 1855 and entitled "How the World is Really Governed". This article (and it was not unusual for that time as clergymen were newspaper editors or leader writers) was a form of "secular sermon"preached at the readers, but it also provides a fair summary of the fundamental values and beliefs prevailing among the articulate and the educated in Sydney during the middle years of the nineteenth century.

It was these men who were actively involved in giving lectures at the Sydney School of Arts, the Philosophical Society and later the Royal Society of New South Wales, who contributed articles and letters to the newspapers and journals of Sydney both on questions of immediate concern and matters of general interest, who created the "intellectual climate" of Sydney during this period. They were clergymen, lawyers, professional men - invariably good, solid respectable members of the community. What the inarticulate, the labourers, farm workers and the like thought on such matters we will never know for such people rarely leave behind a record of their opinions - although the writings of the poet Charles Harpur, who was very much a man of the people, indicate that he shared many of the values of his more middle class compatriots. In any case it was these active, articulate members of the middle class who, through their speeches and writings, set the agenda for the way in which political, social, moral and even scientific issues were discussed during these years. When we speak of colonial culture, it is largely the culture of these men to which we are referring; but I believe it is fair to say that an accurate picture of their "mental furniture" can be extracted form their writings, a picture which provides us with a fairly good idea of the place of science in the culture of colonial Sydney.

It is a truism to say that what we call science or scientific activity does not operate in a vacuum but is part of the more general complex of values operating in a society. If possible, this was even more the case in the nineteenth century. Ours is the age of specialization and of the professional scholar - this is the case in the humanities as much as in the sciences. But there were virtually no professional scientists in mid nineteenth century Sydney, only people interested in "natural philosophy", enthusiasts rather than people seeking to climb "the greasy pole" of academic life. Such people had no vested interest in becoming "experts"; indeed there was a suspicion of specialization. This was a time when the interrelatedness of all knowledge was emphasized, a belief expressed by John Woolley, first Principal of Sydney University, when he described the way in which Niebuhr, the philologist, had been forced to pursue his studies into philosophy, ethnography, social science and medicine.[2]

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* Paper given at the "Scientific Sydney" Seminar on 18 May, 1985, at History House, Macquarie St., Sydney.

Natural philosophy or science was considered to be an activity related to all the other activities of the human mind - in many ways it had not yet fully established its independence and autonomy from these activities. There is much to gained for an understanding of colonial science from an analysis of the age, from an exploration of the categories that were used to interpret the world and man's place in it.

With the development of railways, the telegraph, photography this was indeed a time of great prestige for science but it was also an age which placed its faith in the values of progress, free trade, self-culture, improvement and the triumph of civilization. As one colonial writer put it:

> commercial enterprise has a direct tendency to promote science, literature and the arts; .... it is only when these are united that the one is dignified and the other useful; and that this union has a beneficial influence on human happiness and the peace of mankind.[3]

Science was but one element, albeit a very important element, in the ultimate victory of humanity.

So what we must imagine is a picture with the Statue of Science standing in the foreground; next we sketch in the surrounding Gods and Goddesses of the Pantheon and with a few quick sweeps of the brush the grove in which they are standing. Hopefully we can achieve the necessary perspective and proportion between the elements of the painting, or, more prosaically, a sense of context.

Beneficent Providence, the belief that God was directing the world and mankind along a pre-ordained path towards a better future, and Harmony the ultimate goal of that endeavour together encapsulate the view of the world held by many of the educated and articulate in mid nineteenth century Sydney. It was a view of the world which emphasized "progress" and change but it is important to see that it held out the promise of ultimate stability. Change was not aimless and purposeless; mankind was not the victim of an all-conquering Fortuna which rolled on until eternity with no apparent rhyme or reason.

Men are frightened of change; it disturbs the equilibrium of their values, it disorients them - it creates that state of insecurity which the sociologist Emile Durkheim called anomie: a psychological condition which can afflict the successful as much as the failed. Peter Gay has drawn our attention to the fact that "innovation" was for a long time a dirty word;[4] preservation not creation was considered to be the supreme virtue. Even the French Revolutionaries who so completely upturned the established order were seeking not so much to destroy as to restore their society to its true principles.

The nineteenth century was not merely an era of great change but it was perhaps the first time in human history that men accepted the legitimacy of change. But at the same time they had to take the sting out of the tail of innovation and render it sensible and orderly. This was an especial need in a new and fluid society like New South Wales which had been created by migration and which in the early 1850s still bore the stigma of its convict past. It was a society renowned for its sinfulness although this can be explained partially by the fact that clergymen, through their involvement in its intellectual life, helped to "manufacture" this image; then in the early 1850s it was a community turned upside down by the gold-rushes. This was a society both in turmoil and perceived to be in turmoil; in the leading articles of the newspapers of that period there can be found many references to the supposed high incidence of drunkenness, suicide and mental illness in the colony.[5]

When the colonial thinkers came to discuss and write about progress and Providence it was not just a matter of explaining change, it was also a question of managing it, of demonstrating that change, re-baptised as progress, meant order and the promise of stability. They took the erratic gyrations of change and turned them into the well modulated rhythms of progress.

There was nothing to be afraid of; Providence was acting for the benefit of mankind and moving towards a harmonious unified world bound together by the free operation of God's laws. The whole operation was proceeding according to the natural laws established by God and knowable by man through the exercise of his divine faculty of Reason. Man could consequently participate in realising the aims of Providence and God or he could hinder it and disobey God's laws (and suffer the consequences).

The position is best summed up in the concluding sentences of John West's *History of Tasmania*:

> The happiness and prosperity of the people is by Divine Providence placed within their power. If they grasp at wealth to the neglect of their social and political duties; if, for the sake of selfish ease, they resign to ignorant and violent men the business of legislation; if they tolerate systematic debauchery, gambling and sharping; if they countenance the press when sporting with religion, or rendering private reputation worthless; if they neglect the education of the rising generation, and the instruction of the working classes; of the rich attempt to secure the privileges of rank by restricting the franchises of the less powerful; if worldly pleasure invade the seasons of devotion; and the worship of God be neglected by the masses of the people, - then will they become unfit for liberty; base and sensual, they will be loathed and despised; the moral Governor of the world will assert his sovereignty, and will visit a worthless and ungrateful race with the yoke of bondage, the scourge of anarchy, or the besom of destruction.[6]

In summary, the belief in a beneficent Providence achieved the following aims:

i) It rendered change orderly and understandable, thereby removing its threatening character.

ii) It allowed the colonial educated classes to believe that they, living in a tainted, insignificant colony at the end of the world, had an important role to play in the progress of humanity.

In this context science is not revolutionary force leading the assault on the bastions of Absolutism and Reaction. Henry Parkes once suggested that in the colony democracy was the true conservative political principle.[7] In a sense this is also true of Science; it was conceived to be a force for stability which would establish the permanent laws through which man comprehends God. This sentiment is summed up in a leader from the *Empire*, which claimed that the resources of the Land could only be developed by a "thorough acquaintance with those great truths of Science which are the Laws of the Creator, and a knowledge of which serves at once to preserve from superstition, and to form the solid basis of true religion."[8]

Now I want to devote the rest of this paper to examining the roots of these values more closely and to uncovering the religious and metaphysical - perhaps one could go as far as to say the ideological - outlook which underlay them. I propose to do this under three headings:

i) Natural Religion: the idea that God rules His universe through laws which can be known by human reason

ii) Beneficent Providence: the idea of a progressive world moving towards a pre-ordained goal.

iii) Ultimate Harmony: the optimistic belief that the goal of history is a sort of Platonic universe from which conflict has been banished. In fact this can be seen as an attempt to translate neo-Platonism into historical terms.[9]

Natural religion is founded on the belief that God's existence and presence in the world can be demonstrated by looking at the way in which the natural world is ordered and structured. It rests on the argument from design which claims that such a well constructed machine as the Universe must have a builder and designer.

This belief in "Nature's God" was common amongst the educated in the colony, and was extended to include the idea that the Universe was an interconnected whole held together by God's laws. Firstly, I would like to examine two relatively formal expressions of this idea, the first originally from 1851, the second from 1881.

Barzillai Quaife produced the only substantial work of philosophy written in the Australian colonies before 1880; published in 1872 this work entitled *The Intellectual Sciences* was based on lectures he had given at the Australian College in 1851. In it he developed the idea that the universe is a harmonious unity which derives its existence from God's Reason.

"The Universe is one. Its author is one. His government is one"[9] - so proclaimed Quaife. Man can know and understand the universe because God's Reason, man's reason and the structure of the universe necessarily coincide. Every part of creation has laws suitable for its mode of existence and three departments of laws rule the universe - Physical, Intellectual and Moral, all of which find their ultimate causality in Reason Absolute. Organic and inorganic matter is governed by "the operation of laws containing a mechanical causality."[10] Mind equally receives its arrangement from the hand of the Creator and, although it is as objective as matter, it contains a subjectivity;possessing Reason it also exhibits subjectively the law of causality. Thought, the product of Mind, is made possible by the laws of Mind and so a Mind free of morbid despositions collects and arranges ideas under the laws of Reason.

This road from physical laws to intellectual laws finds its ultimate goal in God's moral laws, made "by God for mankind and ... drawn out of the very nature of man."[11] Man's obedience to these moral laws constitutes his true nature just as obedience to the physical and intellectual laws of the universe is the road to knowledge and Truth.

In an article entitled "Biological Science" in the *Sydney University Review* of 1881, Professor Stephens elaborated his vision of the hierarchy of knowledge. Man, Stephens claimed, had a threefold nature - animal, moral and intellectual - and the elevation of mankind, which he viewed as the aim of education, requires the "simultaneous cultivation of all these in true harmony and just proportion."[12]

The University should contain a threefold division of labour to maintain this harmonious balance: Humanities to inculcate moral and religious truths, beauty and goodness; Physical sciences which provide the basis for the material advancement of society and the biological sciences dealing with the structure and functions of organised existence. "All three trunks are equally essential" claimed Stephens but the "Sovereign is philosophy; the sciences are her administrators."[13] Moving forward together, "living Philosophy and living Science" will provide the defences that demonstrate the necessity of pure Religion.

Stephens' attitude is somewhat different to that of Quaife and yet he shares with him a number of similar concerns: the unity of knowledge, the primacy of the moral over the physical, the desire to use science as a buttress for religious belief. They both have a hierarchical conception of knowledge, a vision of a well-ordered universe created by God and knowable by man, through Reason.

Most of the educated men of that age shared this belief that the universe was an ordered, rational whole or as Charles Harpur put it "a symbolization, or language of the Divine Mind."[14] Hence William Woolls could claim that every vegetable has its uses; "The Divine Architect made nothing in vain." The progress of science, he argued, "gives daily increasing proofs of the power, wisdom, skill, and goodness of the First Great Cause of Things."[15]

Even in discussing such an apparently prosaic matter as "Sanitary Reform", Isaac Aarons could refer to "The Power that has, in so wonderful a manner, adapted everything in creation to its allotted purpose, has ordained that in accordance with the organic laws of animal existence, the atmosphere we breathe shall be composed of such materials and in such proportions as best fit it for sustaining life."[16]

But this was not just an intellectual matter; Natural Religion was not founded solely on the rather bloodless picture of the Universe as a cold and mechanistic entity constructed out of a series of abstract laws. "All nature proclaims the knowledge, the wisdom of the Creator"[17] claimed one writer. For him and most of his compatriots Nature was a source of marvel, of wonder which man could learn to appreciate aesthetically, religiously and emotionally as well as intellectually. These writers did not separate out the cognitive aspect of the human personality as being alone capable of knowledge; knowing and feeling were but two aspects of the total response of an individual to the world. Nature was the model of the Good and the Beautiful as well as the True.

Nature was the source of morality; external influences "drawn from the book of nature," commented one writer, were used by the "Great Author to expand the infant intellect and inculcate through observation of his creatures many of the social virtues which make man useful to man."[18]

From God the beauty of the Universe equally was derived, and within Nature there could be found models of symmetry and elegance towards which Art, the product of man strove in its attempts to imitate God's original model. As John Woolley aptly summed up: "The Perfection of Art, is to approach, however distantly, to the simplicity and ease of Nature."[19]

This Nature then was Nature of Priestley onto which had been grafted the Nature of Wordsworth; a machine run by laws according to its Creator's specifications but also a source of spiritual strength and joy to which an individual could retire for spiritual renewal.

> Now nourishing is Nature to the soul
> That loves her well! not only as she acts
> In constant contact with its quickened powers,
> But as she tempers all its after-moods
> Through distant memories and remotest tokens.
> And hence, when thus beloved, not only here
> By the great Sea, or amid forests wild,
> Or pastures luminous with lakes, is she
> A genial Ministress; - but everywhere![20]

Nature, was a harmonious whole to which man could go for knowledge, for spiritual renewal, for artistic inspiration, for the rules of morality and social life. Nature was a guide for every aspect of human existence. The way to Perfection lay in living in accordance with the laws she had laid down.

Just as God established the laws of physics and chemistry so had he laid down the laws of economics and society. In the economic realm, as in the physical world God had done his job well and the economic sphere of Nature ran like a well oiled machine. The elements of the mechanism, that is to say the various economic interests of society, did not conflict and bring about inefficiencies, but were complementary; and so long as men allowed them to operate freely and unhindered they would ensure a prosperous and stable society. For example, it was argued that the interests of town and country were identical because they depended on each other for prosperity and hence both were necessary for the completion of the economic and political system.[21] The same was held to be true for the interests of Capital and Labour; socialism was castigated as the creed of selfish mammonists.[22]

In the international context this conception of economic laws gave rise to the doctrine of Free Trade. Put crudely this creed claimed that every country and part of the earth produced goods or resources of which other parts of the world had a need. The free inter-change of these goods and resources would result in mankind being peacefully entangled in a giant web of commerce; war would no longer be desirable or possible; mankind would be bound together in a Brotherhood of Humanity. The triumph of free trade

principles was also associated with the fiffusion of knowledge and the spreading of "civilization" into the four corners of the Earth:

For I am Herald of the Dawn
Of civilization! - Closer drawn
Within my world encircling link
No more can nations ignorant shrink
From out one common brotherhood
Nor deem each other's ill their good.[23]

Economic protection was immoral; a form of stupidity, idleness and robbery. The compact of all mankind was commended by Nature. The "natural faculties of man and the advantages of the country in which he lives, can only be properly utilized when he is free to trade where he likes without obstruction."[24] Free trade was as much a metaphysical as an economic doctrine; the laws of Nature ultimately were moral laws - to support those laws was to do God's will.

Australian democracy could also be viewed as the fulfilment of the laws of nature. Here was a society freed from all the unequal divisions of Europe, a society which could allow the full growth and development of Nature's system of morals, politics and economics, "of simpler and more rational forms of government, as well as a higher state of individual freedom, independence and character."[25]

In this democracy without classes there was no need for parties - progress could indeed be measured by the rate at which the individual came to supersede classes or groups.[26] Replacing "parties" would be a group of right-thinking men, their principles formed in accordance with the Laws of nature and between whom consequently there could be no fundamental difference in outlook.

Representative government was a mechanism which allowed free citizens, their views unclouded by class prejudice, to elect these virtuous, intelligent "natural aristocrats" who would then rule in the interest of the whole community. John Dunmore Lang even used a scientific analogy to describe this process. Colonial society, he wrote, was like a collection of chemical salts thrown into a common solvent and once the water cooled a new form of crystallization would occur in which Nature's aristocracy would rise to the surface.[27]

Nature was the one reliable guide for mankind in every aspect of its existence - material, economic, social, ethical, political. Amidst all the change and upheavals of the age Nature remained solid and objective, a firm set of principles and rules which could be known by any right thinking individual using his faculty of Reason.

At the moment the Laws of Nature were not being allowed to operate freely - indeed England was suffering the consequences of willfully disobeying these laws[28] - but the world was moving towards an era in which they would guide the actions of all mankind. A Beneficent Providence was leading humanity out of darkness and into the light - the present period was to be viewed as a transition stage between the Age of Might of the feudal era and a coming Age of Right, of Justice and liberal principles.[29]

Beneficent Providence was a general, not a particular Providence. It was not the unpredictable intervention of a God whose ways were beyond man's comprehension seeking to punish man for his sins and bring him back to the true and narrow. It was instead a general plan laid down by a God who had man's best interests at heart and who acted rationally through laws knowable by man through Reason. For example, in 1857 there were some fears that a comet would crash into the earth and possibly destroy it. God would not allow the world to be destroyed, announced the *Sydney Morning Herald*, it was not in His nature to

destroy His handiwork through one chance act.[30] No, Beneficent Providence would ensure that God's plans for men, the scheme of History would be fulfilled:

> Each man's Life history is that of all the world
> From that which earthly is, the Spirit is unfurled
> The world is growing up from childhood unto man
> All history has thus, though dark, a might plan[31]

Few, perhaps, would have agreed with Charles Harpur when he wrote that the physical universe, under God's direction, was moving towards perfection"to instance the fact by the way, when the orbit of our Earth, now an ellipse, shall have been a circle, towards which it obviously tending .... be thereby perfected in perpetuity,"[32] but they would have concurred in his belief in "the ultimately perfect plan" of Providential Design.[33]

The triumph of Providence would bring about the final establishment of the Laws of Nature in the Universe - it would abolish the distinction between 'is' and 'ought'. As God intended that there should be free intercourse between the nations of the world, Free Trade was both a statement of an economic law and a moral imperative. As John West summed it up "the mutual dependence of nations is the result of design, not accident. It is the decree of Providence as well as of nature."[34]

Indeed the whole colonial endeavour could be justified in terms of following the will of God as expressed in His Providential Design. Colonization "the possession of the uncivilized world by civilized man" was not as we may believe today, a means to the "aggrandizement" of these men but something far nobler - "a working out of the dispensations of Providence."[35] Not personal avarice and greed but the necessity of carrying out God's impersonal plan is what motivates colonization - it is a matter of duty, not of personal gain; so it was claimed. Colonization then was the Divine Mission of Nations[36] and its role was to ensure that the European races spread into every corner of the Earth. But this was not merely a sordid question of economics and the exploitation of material resources; it was also the means through which the light of science would be spread. Providence was spurring the Europenas on to march hand in hand carrying with them the torches of civilization and enlightenment.

Providence had given science as a gift to mankind for the purpose of "realising those benefits prepared for us by the beneficent Being, the Creator and Ruler of all."[37] To promote science was to strengthen the hand of civilization, to enhance mankind's intellectual capacities, to purify its moral instincts. It was to do God's will. Some writers even went further and adopted a variety of Pelagianism - science was a tool which mankind could use to attain grace and obliterate, or at least ameliorate "the punishment to which man, in the garden of Eden, was condemned for his disobedience."[38]

The idea of Providence was a powerful tool; it explained and justified both the growing European dominance of the world during the course of the nineteenth century and the place of Australia in that victory of European civilization. Reference to it can be found scattered throughout the colonial writings of this period and although most of my examples come from the 1850s it is still possible to read in 1879 of the "part in the history of the world .... this young nation .... under Providence, is destined to play."[39] Everywhere the hand of Providence could be detected - in the Indian Mutiny of 1857, in the growth of cities; even Women's Rights could be justified by reference to it.[40]

Still lest we be carried away by the high minded rhetoric of these pure souls claiming to be no more than the instruments of Providence I will just refer to a lecture on political economy given in 1859 in which we are informed that an advantage of a knowledge of political economy is that it informs one that he

is not only carrying out the directions of Providence - he is also directly benefiting himself.[41]

What then was the goal of progress and providence? As we have seen already some rather extravagant hopes were entertained. The ultimate object of nature, claimed Dr. Campbell in his "Notes on Human Biology" published in the *Sydney Morning Herald*, was the perfection of the species.[42] Charles Harpur expressed his Utopian vision poetically:

> And in this Southern Land there yet shall be
> A race begotten in the Spirit of Beauty,
> Such as the olden Greeks were, limbed and shaped
> By that deep ideality which works
> Into the stuff of nature, and becomes
> Progressively its mould; and in and through
> This physical perfection manifest,
> Shall burn a soul of power surpassing that
> Which was in Greece only the effluence
> Of an artistic, not an actual life.
> But here it shall be Actual - making all
> Man's instincts with his motions modulate
> Till thus perfectionised, his native growth
> Shall body forth the Living Beautiful.[43]

The general tendency was to place the promise of Utopia somewhere in the dim future as Rolleston in a lecture on, of all things, Savings Banks, in 1857, demonstrated when he spoke of the "glorious destiny which, like the dim shadow of a man's hand,can as yet but faintly be distinguished on the verge of a far horizon."[44] Or as W.A. Duncan expressed it, "although the progress is slow we do believe that the golden era has dawned."[45]

If progress, the realization through Beneficent Providence of the laws of nature, had a goal then that goal was Harmony - the harmony of man, nature and the universe. Then man would be restored to his pre-fallen state.

It is commonly believed today that nineteenth century liberalism stressed conflict and competition to the detriment of social cohesion and co-operation. My reading of the evidence, for colonial N.S.W. is that, at least at the level of ideas, this was not the case. The emphasis was very much on co-operation, the common interest, harmony; and as I have already suggested the source of this emphasis comes from the peculiar social circumstances of the colony and the effect of rapid change in a new society. To advocate competition would be to run the risk of social disintegration.

The goal was harmony: at an individual, at a social and at a political level. Once man discovers the theoretical harmony of God's reason, of the laws of nature, he will be able to translate that harmony into practice. Once the individual understands the harmony of nature he can develop his personality so that its various elements attain a state of harmony and balance. God wants man to cultivate all of his faculties, to develop his powers and the individual who cultivates all of his attributes and makes them harmonize comes closest to the state God intended for him.[46] Education is the "cultivation of the entire moral and physical nature"[47]; effectively what we have here is a belief in culture, in the harmonious development of the personality as a means of perfecting it.

This desire for harmony applied to society as well as the individual. According to the laws of nature society operated harmoniously and man possessed a social sense or social sympathy which encouraged him to act in co-operation with his fellow man. There was, in this view, nothing more beautiful or holier than for an individual "in conjunction with others developing the higher faculties, and feelings, and the enjoyments of his spiritual nature."[48]

A natural society was a harmonious society; co-operation, founded on a noble and Christian spirit was to be encouraged. No society, it was claimed, could be prosperous without the harmonious co-operation of capital and labour. Co-operation rather than the selfish class interest of communism was the true key to the workman's paradise.[49]

The key to the creation of the ideal society lay not in the development of a rugged individualism and the competitive spirit but in the natural unfolding of man's social sympathy:

> the social compact attains its maximum of perfection where it draws out of each individual citizen the greatest amount of good that in him lies, as his share in the contribution to the general stock.[50]

As a summing up of the direction in which many believed the age to be going I would like to quote from a *Sydney Morning Herald* editorial of 1872. In the first part of the article the progress of England during the first half of the nineteenth centrury id described:

> But a mighty change was wrought when the spell of isolation was broken by the establishment of means of transit and intercourse. Gradually the nation became an organic unity, the vital energies circulated from the great centres of social life to its remotest extremities, and all its movements were consentaneous. The intellectual light which before shone at a distance, dazzling rather than illuminating, conducted down into the mass, passes now from mind to mind, and penetrates every class of the population with some portion of its heat and radiance. The resources of the country, both moral and material, are developed; the comforts and conveniences of life increased, prejudices are unlearned, causes of separation are removed, and superstitions vanished.

The article then goes on to discuss the more general effects of these developments for the world as a whole and concludes on this rather optimistic note:

> Amidst all the variety of party distinctions, however, there is much that is common to the whole human family. There is a resemblance between men's minds as there is between their bodies - a specific identity, consisting in a similarity of faculties and functions, of emotions and desires. From this affinity of our moral and intellectual nature arises the attraction which mind exercises upon mind, and which is continually drawing men into closer and more intimate communion. This gregarious principle, if not more powerful than the causes that repel men from one another, is more constant in its operation. It inheres in humanity, whilst the causes of separation are accidental and local. There is in the world a far greater number of things to unite the suffrages of men than to divide them. The vices and villainies of our nature are all anti-social. Its heroisms and great virtues are promotive of union and harmony, and flourish most where these prevail. The mind also receives its most perfect form, its highest polish and brightest lustre from contact with other minds, as the diamond receives its shape and brilliancy from kindred substances. Passing by the peculiarities of special combinations amongst men, and contemplating the whole race in co-operation, we come to the conclusion, therefore, that the fundamental and ultimate principles of our nature, which, under all circumstances, would combine their individual impulses, are such as tend to good, and that the prospect of a universal correspondence and approximation of human interest is the result to be expected from the social union of mankind. It is to the instinctive efforts to approach one another, to interchange thoughts and feelings not fless than to the force of those necessities which compel them to seek interchangeable supplies, that men are indebted for most of their great achievements in science and their conquests of nature. It is this social passion that has inspired the noblest works of genius. It is this that rejoices in the peace and prosperity of nations: and it is to this that we must look for the consummation of human happiness. Implanted in our hearts, and interwoven with all our affections, it is one of the primary laws of our being, and must ultimatley supervene all separating causes, which are but circumstantial and fortuitous, as the constant though inappreciable force of gravitation gradually reduces the loftiest ramparts to the dust. Already it has removed all physical and material obstructions to its full operation by inciting to the discovery of steam and electricity. The obstacles that remain are of a moral and intellectual kind, many of them irregular, and others the unnatural growth of vicious training; but they also are destined to give way before the law of approximation, to be erased and trodden down by the mighty agencies it has called into being and activity.[59]

The faith that harmony would ultimately be achieved was a very fragile thing and could be seriously

threatened by disasters in the same way as the Lisbon earthquake of 1755 had shaken a similar sort of optimism amongst the philosophers of the early eighteenth century. Society, politics, the natural world clearly do not work in a harmonious fashion for the benefit of man - a great deal of suffering in the world is caused by the workings of Nature's laws just as the "laws" of political economy punish the defenceless as much as those who disobey its dictates.

But empirical verification is often no real barrier to belief in such ideas; failures can always be explained away and the responsibility placed on men and circumstances rather than the validity of the intellectual framework which interprets their actions being questioned. The pattern of belief remains and it largely determines how men see their world; it sets intellectual priorities. For example most people read Rolf Boldrewood's novels as simple romances and adventure stories. Yet his works do have an underlying "metaphysics" which is very much concerned with the workings of Beneficent Providence. Boldrewood asks is there a Beneficent Providence operating in the world. If so why is luck so important in determining the fortunes of men?[52] His interest in bushrangers was not accidental - if Nature is allowed to operate freely in Australia why then do bushrangers rise and flourish.[53] Can man and society really be perfected?

The framework froms the categories which determine what questions men will ask of their environment - hence the faith in harmony remains largely unchallenged but the members of society who fail to live up to its precepts are castigated and condemned as self-interested. The goal of Harmony remains but men are seen as frustrating its achievement. This simple insight helps to explain the constant railings against the workings of political demoncracy in New South Wales during these years. Politicians were constantly reviled as "selfish and recklessly adventurous men"[54] driven only by love of office who had chased the high-minded men of capacity and intelligence out of public life - those men who stood for the "common good" and who were capable of producing harmony. Instead of harmony there were party struggles which impeded legislation; in the place of the striving for the Ideals of Nature were "vulgar ambitions and sordid aims".[55]

And yet the faith remained: in a special supplement on the occasion of the Sydney Exhibition of 1879 the *Sydney Morning Herald* claimed that improvement had occurred in the colony despite "delays and obstructions, the confusion of so-called parties and the distractions of personal interest, the dearth of trained statesmen, and the eagerness of untrained men to force themselves to the front."[56]

Another writer expressed it this way:

> However legislation may patter on in an idiotic way, the silent powers of nature and the expand-common sense of the community, carry us in a certain direction. We do not go back.[57]

What then was the implication of all this for science in Sydney during these years? I think the first, and in someways most important thing, is that these ideals reflect the values of the sort of society in which science, like literature and the arts, is still the pursuit of amateurs, of gentlemen. It is a relatively simple society; it lacks a large government bureaucracy and well developed education system - the sorts of institutions which today give employment to the educated and the professional. The Law was virtually the only outlet for a young man of ability.

Science was part of the "general culture" of those in colonial society who sought to improve and educate themselves. Only slowly did Science establish itself as an autonomous entity. In the 1850s almost everyone used the words "Science" and "Art" in their pre-modern meanings - science being the theoretical and pure knowledge of an object, and art the practical application of that knowledge. Therefore "Art" was assumed to be both beautiful and useful. When Sir Thomas Mitchell spoke of "The Importance of Art and the Necessity of it in New Colonies" he meant that science could be used in various ways to improve the colony

and to reclaim lands from nature, assuming that making nature useful to man invariably meant beautifying it.[58]

An *Empire* leading article spoke of the need to promote true science and good taste as if the two things were identical. It claimed that an improvement in colonial architecture would raise both the level of knowledge and the standard of taste, and the individual should "invoke science and Art to beautify his family mansion and to plant his garden."[59]

The journals of the age tended to include articles on science and scientific matters because they assumed that educated men were interested in such matters. Just to take two examples, both from rather late in the period - in the *Sydney University Review* of 1881-3 alongside essays on Charles Lamb, Carlyle and Cathedrals there are articles on Charles Darwin, Artesian water, Linnaeus and Technical Education.[60] The more literary *Australian* (1879-81) included a regular section on "Practical Science", by which it meant such things as sanitation and railways.

This leads us to a more coherent understanding of colonial attitudes to science. Although, as we have already seen there was a strong tendency towards Utopianism and dreamy visions of the role of science in building the perfect society there was also an intensely practical and utilitarian streak in colonial attitudes to science as summed up in this phrase "Practical Science". The "Introduction" to the *Sydney Magazine of Science and Art* (1857-9) stated that the British had a "passion for utility" and were more renowned for the genius of their inventors than for the glories of their literature. It went on to stress the "fresh boons" which science was conferring on mankind, and the assistance it could render the colonists in developing their continent.[61] Later articles in this journal stressed the great benefits science could confer on the community and claimed that "a man, with some scientific knowledge, in the bush, is a benefactor to his neighbours for miles around."[62]

Many of the articles published by this journal were marked by the spirit of utility. For example in an address to the Horticultural and Agricultural Society, Sir William Denison said that there was a need for the farmer to use machinery and science to increase the return on capital he had invested in his land, a sentiment which underlay much of the interest in science in the colony.[63] Articles often discussed new practical scientific advances of the day which might be applicable to the colony in such areas as railways, sanitation and health, the local water supply, photography, irrigation and statistics: all of which were matters which would aid the "general health, welfare, happiness, longevity, and hence .... the general improvement and advancement of the human race."[64]

This instrumentalist conception of science, the interest in the benefits it could provide as opposed to an interest in science as a thing in itself, as a means of attaining truth, fits in very well with a Utopian conception of the place of science in society. Both stress what science will produce rahter than the value of the scientific activity itself. This instrumentalism pervaded the whole of colonial culture and is implicit in the nation of "improvement" - literature and the arts were equally studied not as ends in themselves but as means to an end: in this case the harmonic cultivation of the powers of the individual.

Another consequence of the relative undevelopment of science as an autonomous entity was the prevalence of much pseudo-science in the colony. These were the years during which first phrenology and then spiritualism became an abiding interest for many people. But there was also much interest in such things as animal magnetism and the supposed occult powers of electricity. To take but one example - in the *Sydney University Magazine* of 1855 there appeared an article entitled "Electricity and Magnetism, in

Connection with the Human Frame". It begins soberly enough by stating that "phenomena, commonly called inexplicable, may be accounted for, and shewn to be but a newly discovered result of the universal laws of nature."[65] It then goes on to discuss, with reference to Mesmerism, animal magnetism, electricity and the "aura" theories of Reichenbach, the idea that "there seems to be a power in the will, of evolving an atmosphere from oneself, which is capable of repelling the ill effects of the diseased atmosphere emanating from a sick person" and that this power be called "contagious health".[66] And the discussion is carried on in a serious, scientific fashion in the hope that this line of investigation will help to eradicate disease.

The final effect on the attitudes to science in Sydney during this period which comes from the stress on unity and harmony was the tendency to integrate new ideas into the existing framework and to demonstrate that all knowledge formed a coherent whole. These colonial thinkers wanted science and religion, the bible and history, evolution and creation; therefore they emphasized not the conflict of ideas but their harmonious integration into one vast system of knowledge.

As an example of this tendency I refer to an article entitled "Evolution and Faith in History" by one W. Carlile which appeared in the *Australian*. In this article Carlile argues that it is necessary to connect the Christian Spirit with Evolution because Christianity renders individuals, families, nations "more effective combatants in the struggle for existence."[67] He believes that there is no conflict between Evolution and design and cites the English Constitution as a "great and beneficent system that is the work, at once, of Evolution and of Design."[68] Citing as his authorities Sir Henry Maine and Bagehot, Carlile comes to the conclusion that progress is best when it grows and "evolves" slowly out of the circumstances that preceded it. "Evolution", in this view, is not the struggle for existence but smooth harmonious development in which conflict is smoothed out and opposites reconciled.

The 1880s mark a watershed in Australian development in exactly the same way as Norman Stone has observed that they did in Europe.[69] That decade saw an intensification of those tendencies which have given the modern world its distinctive character - urbanization, secularization, nationalism, bureaucratization: in other words those developments we associate with modern industrial society. Thus in the 1880s Sydney becomes a burgeoning urban centre facing all the problems of modern metropolitis; trade unionism and the issue of Labour becomes increasingly important; a new educational framework is put into place; even the sleepy old University of Sydney begins to grow and diversify.

As a new more complex socity emerges so are the old verities questioned: the liberal faith is challenged by a new secular nationalism exemplified by the *Bulletin*.

The bush ethos, largely the product of alienated urban intellectuals, and stressing the virtues of rural simplicity, takes up the cudgels against "civilization" and commercial, urban values. A new class of professional writers and journalists replaces the gentlemen "litterateurs" of earlier times.[70]

The emergence of this new, professionally oriented culture meant the death-knell of the old-style gentleman scientist or writer and of his vision of a universal system of knowledge bound together by God and His Laws. In Science, as in all areas of knowledge, the day of the specialist and the academic was at hand.[71]

This is not to say that men ceased to believe in the ideal of harmony and in the possibility of achieving a society bound together by social sympathy. Indeed the introduction of Hegelianism and the new liberalism into Sydney during the 1890s injected a new vitality into these ideals of liberal consensus. But such ideas were now the province of intellectuals and professional philosophers, and more importantly

the changed context had altered the nature of many of the ideas. For example the new liberals came to feel increasingly that a conflict existed between the ideals of harmony and the laws of nature.

But that is another story. In the period from 1850 to the 1880s no such doubts existed: most educated men trusted that they lived in a world ruled by the natural laws of an all-wise, Just and Good God, that Beneficent Providence was taking them towards a better world and that they would eventually create a harmonious world bound together by man's natural social sympathy. It was an optimistic vision and one in which Science played a leading role but, and I refer again to the article with which I began this paper, who would not have optimism knowing that the future was "wholly in the hands of One whose purposes are infinitely wise and beneficent and who will infallibly make all things fulfil his mind"?

NOTES

1. *Empire* (6 April 1855)
2. John Woolley, *Lectures Delivered in Australia* (London: Macmillan, 1862), 57.
3. 'On the Advantages which Science and Commerce derive from each other', No 1, *Australian Era*, Vol. 1, No. 6 (February 1851), 88. Although not stated the author was W.A. Duncan.
4. P. Gay, *The Bourgeois Experience*, Vol. 1, *The Education of the Senses* (New York: Oxford University Press, 1984), 48.
5. On drunkenness - *Sydney Morning Herald* (5 May 1857, 1 November 1858), *Empire* (26 February 1851): on suicide - *Empire* (26 February 1851); on insanity - *Sydney Morning Herald* (6 November 1861).
6. John West, edited by A.G.L. Shaw, *History of Tasmania* (Sydney: Angus and Robertson, 1971), 533.
7. Henry Parkes, edited by D. Blair, *Speeches on Various Occasions Connected with the Public Affairs of New South Wales* (Melbourne, 1876, 81.
8. *Empire* (25 August 1859).
9. B. Quaife, *The Intellectual Sciences*, Vol. 1 (Sydney, 1872), 137.
10. *ibid.*, 169.
11. *ibid.*, 178.
12. W.J. Stephens, 'Biological Science: A Necessary Factor in University Work', *Sydney University Review*, No. 4 (December 1882), 396.
13. *ibid.*, 394.
14. Mitchell Library, Harpur Manuscripts, A92.
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Department of History,
University of Melbourne,
Parkville, Vic. 3052, Australia.

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The Architecture of Scientific Sydney*

JOAN KERR

A special building for pure science in Sydney certainly preceded any building for the arts - or even for religious worship - if we allow that Lieutenant William Dawes' observatory erected in 1788, *was* a special building and that its purpose was pure science.[1] As might be expected, being erected in the first year of European settlement it was not a particularly impressive edifice. It was made of wood and canvas and consisted of an octagonal quadrant room with a white conical canvas revolving roof nailed to poles containing a shutter for Dawes' telescope. The adjacent wooden building, which served as accommodation for Dawes when he stayed there overnight to make evening observations, was used to store the rest of the instruments. It also had a shutter in the roof. A tent-observatory was a common portable building for eighteenth century scientific travellers; indeed, the English portable observatory Dawes was known to have used at Rio on the First Fleet voyage that brought him to Sydney was probably cannibalised for this primitive pioneer structure.

The location of Dawes' observatory on the firm rock bed at the northern end of Sydney Cove was more impressive. It is now called Dawes Point after our pioneer scientist, but Dawes himself more properly called it 'Point Maskelyne', after the Astronomer Royal. Dawes was simply a naval lieutenant who volunteered for service under Phillip. The Reverend Dr. Maskelyne was the man who obtained astronomical instruments on loan from the Board of Longitude so that this particular naval marine could make observations useful for English shipping in the Pacific (the applied science aspect of the place), and in order to record an expected comet for British scientists (the pure science role, and the explanation for the building's speedy erection).

Dawes used the observatory for four years, but when he returned to England in 1791 he took his borrowed instruments back with him and the structures were abandoned. The observatory apparently collapsed, but by the end of the year Collins reported that the wooden building was being used as a guardroom, a platform for a flagstaff and a cannon having been erected beyond it. Science rapidly gave way to a modest display of military strength and the original purpose of the building was soon forgotten.

The only visual evidence we have of the appearance of the observatory is Dawes' own rough sketch in a letter. At Old Sydney Town at Somersby, the scientific building has been chosen for recreation rather than the military one, despite its short life and this somewhat meagre evidence of its appearance. Some of the instruments used in the original building still exist at the National Maritime Museum, Greenwich and have been reproduced for this replica, so we can now pride ourselves on Sydney's scientific, rather than militaristic origins - an aspect of the colony then only of interest to a few gentlemen in England.

Governor Brisbane's observatory at Parramatta of 1822 was the next observatory in Australia.[2] It was a private gentleman's whim, erected at Brisbane's own expense, although the British government took it over and continued to maintain it after he left - until 1847 when it was dismantled. The instruments would have been sold off the following year had it not been for Captain Philip Gidley King's intercession, although - largely because of disputes about a site - no building to house the stored instruments eventuated for another ten years. In 1858 a temporary building at South Head was used by the government astronomer (Reverend William Scott) for a short time until the new permanent building in Sydney was completed later in the year. Philip Parker King had suggested that the best point for a permanent observatory in Sydney would be on Fort Phillip; its time ball would then be visible from all parts of the Harbour. This was the site

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ultimately selected. The new observatory replaced Fort Phillip, a defence work erected under Governor King in order to provide a defence for the town after the Castle Hill convict rising. So the new observatory stood (and still stands) on an old rampart and the wheel turned full circle. Defence replaced science; then science replaced defence.

By June 1858 the new building was complete enough to allow meridian observations, and the instruments from Brisbane's old Parramatta observatory were placed in it. It was designed by the Colonial Architect, Alexander Dawson[3], and consisted of an astronomer's residence, a library, a 'computer's room' and a centre square tower 58 feet high carrying a time ball that dropped every day at 1.00 p.m. A 12 foot square shed was erected in 1865 to the south of the main building, for thermometers, and a small magnetic observatory was also added then. A Government Printer photograph of c.1870 shows the building we see today - now restoration has been competed.[4]

Yet in 1907 demolition of the Sydney Observatory seemed inevitable. A writer in the *Sydney Morning Herald* commented:

> Let us hope that though the Observatory may go to a better site, the present generation will not rudely sweep away the historic remains where our pioneers built forts and raised guns to protect their small town [against their own people, I might interpolate], and science stepped in and raised her tower and aided the work of the community with her astronomical and meteorological observations and formed her base for shipping reports and signalling.[5]

The tower with its time ball was always the major public identification for this building and its visual importance helps explain the style of the architecture. The Observatory was, I believe, the first important building in New South Wales of this asymmetric Italianate villa form - a style normally confined to stately residences. (It had, for instance, already been used for the Governor's house at Toorak, Melbourne). But in Sydney, The Governor was lodged in the English architect Edward Blore's Tudor castellated towers; Italianate classicism was mainly employed for commercial buidlings, especially the large number of banks erected in the 1850s. The Observatory's major role was to represent science as the handmaid of commerce, helping to guide ships taking gold back to England. Hence the general style of the building was appropriately allied to a fashionable commercial style. The unique tower on the hill nevertheless proclaimed a special importance for science in the landscape - particularly when it was seen from Darling Harbour, the commercial shipping area. (Sotherby's Australia has recently discovered a romantic evening oil painting of the Observatory from Darling Harbour by Frederick Garling, dramatically reinforcing this image.)

When less obviously allied to commercial shipping interests or to London's gentlemenly obsessions, scientific building was less dominant in both form and location. The Australian Museum building is a case in point. Although the natural history of Australia was also of vital interest to gentlemen scientists in Britain and New South Wales, the commercial possibilities of flora and fauna were more limited. Collections had been formed ever since Sir Joseph Banks arrived in 1770, but the possibility of a public museum to house them locally did not develop beyond an odd room in somebody's house or government building until 1846. Passionate advocacy by people such as Alexander Macleay had resulted in a paid Government Zoologist for the colony (paid from England of course); but voices, such as Governor Bourke's in 1835 raised in favour of putting the natural sciences into an independent building were countered by equally persuasive tongues against such extravagance. William Charles Wentworth was one opponent; another was the editor of the *Sydney Monitor* who wrote:

> Zoology and Mineralogy, and Astronomy, and Botany, and the other sciences, are all very good things, but we have no great opinion of an infantile people being taxed to promote them .... We might as well give salaries to painters, sculptors, and chemists, as to botanists, astronomers, and Museum collectors[6]

- an opinion apparently still current in Canberra.

Nevertheless, powerful advocacy from above won out. In 1845 the Colonial Architect, Mortimer Lewis, was asked to design a museum building to cost no more than 3,000 pounds.[7] Construction began in January 1846 on the corner of William and College Streets, facing William Street. Lewis' building was modestly domestic in appearance and Greek in style. As well as the exhibits (to be housed in the large hall under a dome and behind a portico in antis), the building also had to house the museum staff and their families and provide a proper board-room for the committee of management.

From the end of the first year of building - when only the foundations had been put in for a third of the voted money - it was clear that Lewis had greatly underestimated both the time and cost involved in realizing his design. In August 1849 Lewis resigned, to avoid being dismissed. He left an unroofed shell; 7,416 pounds had been spent and there was evidence of considerable fraud in the costs of materials and labour. The chaste Colonial Greek building was investigated by an independent firm of architects (Robertson and Duer), who discovered that materials had cost more than their contracted prices and then had not been incorporated into the building but directed off site after delivery dockets had been signed. Wages were paid to non-existent workmen and Lewis' accounts about these transactions were suspiciously confused. The dome - as well as Lewis - was removed from the incomplete building. A plain hipped roof went on by 1850 and the building was ready for occupancy by the museum staff and committee of management by March 1852. The exhibition hall for the specimens and the public, however, remained useless, since it still had no gallery or showcases. Enough extra money was granted from the public purse in 1853 and 1854 to construct these, but no access staircase to the gallery was built. The gallery remained a storage space, reached only from the private quarters of the Museum until 1857. Then it was completed, only to prove quite inadequate in size for the increased demands that had arisen during the twelve years it had been building, despite by then having cost some 16,000 pounds. Four months after completion, Alexander Dawson produced plans for a major extension of more monumental Palladian form. Hardly surprisingly, the government refused to fund this.

So the Australian Museum remained a modest Regency building, domestic in its exterior appearance and major use, and limited in its interior public space. This single exhibition room was, nevertheless, the grandest public hall the city could boast. In 1854 it housed an 'Exhibition of the Natural and Industrial Products of New South Wales' prior to selected exhibits being sent to the Paris Universal Exhibition of 1855 - the first major representation of colonial products to be seen overseas. (A few exhibits from the colony had been sent to the 1851 London International Exhibition, the first of these gigantic collections of objects from all parts of the world, but these had not been officially organised through a local committee and were not on the scale of the 1854 effort for Paris.)

Colonial rivalry may have had something to do with N.S.W.'s brave display, since Victoria was also sending a major contribution to Paris for 1855. Sydney's exhibition was opened by the Governor General, Sir Charles Fitzroy, who arrived in suitable splendour to dwarf the building. Fitzroy read his opening speech in the exhibition hall in front of William Nicholas' gigantic plaster statue of Captain Cook - a statue that was never cast in bronze and subsequently disappeared.[8] The classical statues adorning the exhibition when Fitzroy opened it were plaster casts owned by Sir Charles Nicholson. They had been included only for local artistic ambiance, casts of antique statuary (probably imported) being quite unsuitable for export to Paris, of course. What did go to France were lumps of gold, samples of wood, models of buildings and photographs of Sydney's progress - the whole vastly more 'natural' than 'industrial'.

The same sort of colonial rivalry that helped inspire Sydney's 1854 exhibition also seems to account for the fact that extensions to the museum were provided a mere four years after completion. By 1856 Melbourne had a grand Italianate building (by Reed and Barnes) to house its 'national' museum, art gallery

and library. Sydney then had only the private Australian Subscription Library erected in the 1840s at the corner of Bridge and Macquarie Streets (by Henry Ginn), Lewis's simple Greek museum, and no sign or hope of a public art gallery. (Nicholson's statues continued to be displayed at the museum as a slight sop to the arts, and occasional art exhibitions continued to be held there with borrowed works from private collections.) Dawson's successor, the Scottish architect James Barnet[9], designed an immense Renaissance-style, domed and porticoed combined Museum, Library and Art Gallery that would utterly annihilate Melbourne's; it was never built, although it remained a fixed ideal until well into the twentieth century. A somewhat grander version of Dawson's more modest 1857 proposal to extend the Museum was, however, preferred.

By 1866 one of Barnet's wings following this modified design - the facade to College Street - was finished. As a contemporary newspaper noted: 'Sydney was greatly impressed by its large sandstone bulk resting on a stylobate twenty feet high and with its Corinthian piers forty feet high bearing flowery capitals caved by Walter McGill.' The interior was, however, less overwhelming, as a Legislative Assembly Select Committee noted in 1872:

> The edifice is too high and too narrow; the approaches from the street are incommodious; the windows are wrongly placed and faulty in design; the interior is crowded with heavy pillars which waste the space and obstruct the light; the internal walls are broken by angles and recesses; there is a useless gallery above the second floor; and there is in every part of the building abundant evidence of the architect's desire to subordinate utility to ornament.[10]

Most of the faults seem to have been due to this continuing desire to outdo Melbourne without sufficient revenue to complete anything but the facade of one wing. Yet, despite this report, which also stated that 'The fittest kind of ornamentation is that which is accomplished by the judicious arrangement of the exhibits themselves' and proclaimed that 'The interior of a Museum should be as nearly as possible rectangular', the public purse only opened for competitive facadism, not functional display. A building that looked impressive when visitors drove past it, or reproduced well in engravings and photographs, was more important evidence of local support for the natural sciences than one that actually housed collections and specimens adequately.

In 1890 funds were voted to add a third floor over the original building and bury Lewis' design under Roman splendour matching Barnet's. But the only real exhibition space then added was an extra gallery above the old one. External homogeneity was all that really mattered. The newly-appointed Colonial Architect, Walter Liberty Vernon, provided an accurate and careful continuation of Barnet's design and, although Lewis' core is still buried in the building, this is now very difficult to discover from the outside.

Unless commercially viable, public homage to science in Sydney remained similarly skin deep. Allegorical statues and reliefs carefully labelled 'Science' - so one could tell them from Agriculture, Industry or the Arts - appeared as occasional tributes to its valued role in the community, although normally placed on public buildings dedicated to quite unscientific purposes. For instance, the former Colonial Secretary's Office of 1879 on the corner of Bridge and Phillip Streets still bears a large marble lady by Simonetti[11], labelled 'Science', along with other female figures representing Labour, Art, Wisdom, Justice and Mercy, while the George Street side of the G.P.O., built in 1866-74 (the first section of a proposed great building over a whole city block) also incorporates a female allegorical relief figure of Science on one of the ground floor spandrels. She was probably carved by Walter McGill, the sculptor who did the Museum's capitals. Like the statue on the Colonial Society's Office, she is simply one figure among several: the Arts, Commerce and Literature.

Yet, even this sort of superficial and unspecific public acknowledgement of the role of science in the community remained rare. Scientific achievements remained largely dependent on the work of a few dedicated individuals capable of funding their 'hobby' When they did so, they were not publicly commemorated.

Figure 1. Connelly, Statue of Thomas Mort, Macquarie Place, Sydney.

Captain Cook and Governor Bourke were remembered in bronze at public expense, while Governor Phillip received a giant monument by Simonetti at the end of the century. Political and commercial success was also given public acknowledgement in the form of statues to its great men - including the anti-museum Wentworth, whose marble life-size statue by Italian Tenerani[12], paid for by public subscription, now stands in the Great Hall of the University of Sydney. Thomas Mort[13], the great wool auctioneer and exporter, was commemorated by a bronze statue by the English sculptor Connelly in Macquarie Place. (see Figure 1) But the only naturalist to be publicly commemorated was Alan Cunningham[14], who was given an obelisk in the Botanic Gardens and a statue (by Tomaso Sani)[15] amongst the host on the Lands Department building - tribute to his role in opening up the land rather than homage to his less financially profitable natural history discoveries. (see Figure 2). Australian plants may be depicted beside him, but Cunningham's colleagues on the building are the explorers (such as Hume, Hovell, Mitchell and Bass) and the politicians - including Robinson and Parkes who opened up New South Wales for settlement. (see Figure 3).

The most disinterested public sculpture to a man of science I know of in Sydney is that on the Pitt

Street side of the G.P.O., completed with the second phase of the building in 1883. It is, reputedly, of Archibald Liversidge, Professor of Chemistry at the University of Sydney[16], and was carved by the Italian, Tommaso Sani, under James Barnet's general direction. It is, however, only a section of one of four spandrel sculptures.

All the G.P.O. spandrel sculptures caused a great public outcry when they were completed. Their naturalistic style and their semi-comical references to real people were considered most inappropriate for the permanent medium of architectural carving. For instance, the postman appears to be a portrait of the post-master general, Francis Wright, delivering a letter to a servant girl who is flirting with him. The architect of the building was also there, Barnet being depicted as a Michelangelsque God still dreaming of his combined museum, gallery and library building (in the background). Liversidge was included in the spandrel representing Sydney's professions - as 'the Professor' - along with Sir James Martin as 'the Judge'.[17] They formed a pair with Commerce and Mining. (see Figure 4)

Figure 2. Tommaso Sani, Statue of Allan Cunningham, Department of Lands, Sydney.

Figure 3. S.T. Gill, 'Cunningham's Monument Botanic Gardens Sydney', from *Sydney Illustrated*, Sydney, Allan and Wigley, 1856.

Questions were asked in Parliament about these relief sculptures and a Select Committee was set up to decide whether they should be removed. The President of the English Royal Academy, Lord Leighton - a life-long Classicist - announced on seeing photographs of the controversial works: 'You have indeed an uphill fight where such things are possible'. Such a furore over modestly realistic representations in stone implies that the sculptures somehow posed a real threat to establishment values. Like the buildings that housed them, scientific pursuits were moving away from exclusively British interests, from being the province of the governor or resident gentlemen of means or even from being allied with privileged institutions such as the Australian Museum and Sydney University. At the G.P.O. the ordinary person was being publicly invited to view the various activities of the colony - including science - depicted in a style he or she could understand, although, as yet, no building allowing significant participation in such activities was being contemplated by 1883.

Sydney University's architectural monument to science was next on the scene. The Macleay Museum, designed by George Allen Mansfield[18], Australia's first native-born architectural member of the R.I.B.A. and the architect of History House, was erected in 1885-86. As a monument the Macleay Museum is, I think, singularly unsuccessful. Its liver-brick is totally out of harmony with Edmund Blacket's Main Building and its location ensured it was always overshadowed by its predecessor. The Macleay's style is debased Tudor, with thin crenellations, modest towers and coarse proportions- a mean imitation of Blacket's careful pattern-book Anglomania. Its display areas consisted of an uninspired piling up of display galleries on the Australian Museum Model (now destroyed). Altogether it came a very poor second to the sensitive, stylish and sophisticated Medical School by James Barnet - also begun in 1885 on the other side of the Main Building at Sydney University. At least the Macleay was finished first. It was a quick job, completed in a year in order to satisfy conditions for a major benefaction, whereas Thomas Anderson Stuart, Professor of Medicine, was determined to have a great architectural monument, even if this was not able to be completed for five years.

Figure 4. Tomasso Sani, 'The Professor', (Archibald Liversidge) spandrel sculpture on General Post Office, Sydney, completed 1883.

The Macleay Museum had to be quick and cheap because the University was not going to receive the great Macleay collection of insects, plants and books unless it provided housing for it. But this undistinguished architectural solution appears to have affected the public perception of the collection, and the Macleay Museum has never quite attracted the audience it deserves. Nevertheless, by 1886, Science was at least publicly commemorating the increased accessibility of its activities and collections for all scholars, not just for gentlemen of means. Sir William Macleay's gift is at least an appropriate symbol of this voluntary trnsference of scientific power and activity. A final stage would be to make scientific pursuits available to everyone although working-class science, of course, had to be labelled Applied rather than Pure. Still, practical applicability had been an essential condition for the creation of Sydney's scientific monuments from the first.

The erection of the Sydney Technical College and its adjacent Museum of Applied Arts and Sciences in Ultimo - a group of grand buildings designed by William Kemp in 1891 indicates this particular coming of age. (see Figure 5). Architecturally, these buildings seem to me to be the most significant amongst all I have mentioned. Earlier buildings had been attempts at exact emulation of models established elsewhere. The M.A.A.S. and the Technical College, although inspired by overseas' examples, have not as simple emulative a relationship to Britain or to Melbourne as the Observatory or Museum had. Nor were they a cheaper and less technologically innovative echo of London's 1851 and 1862 International Exhibition buildings (with a dash of Philadelphia's 1875 creation) as Barnet's short-lived International Exhibition

Figure 5. William Kemp, Sydney Technical College, designed in 1891.

Figure 6. Detail of carving over main door, Sydney Technical College.

Figure 7. Lucien Henry, dado design, late 19th century. From R.T. Baker, *Australian Flora in Applied Art*, Sydney, Government Printer, 1915.

building in the Domain (1879-1882) had been. The American Romanesque style, with its round arches, heaviness and modern mixed materials, was diluted with a strong dose of English neo-Norman, inspired particularly by the example of Alred Waterhouse's Natural History Museum at South Kensington, London (1873-81). But the extended Palladian facade of the 'Tech.', in particular, seems characteristically local in form.

There were also strong and obvious assertions of nationalism in the Australian plants and animals in the capitals of the pilasters and columns of the Technical College building, carved by McIntosh and Fillans: kangaroos, wombats and echidnas and, over the main door, Australian lizards. (see Figure 6) Like the architectural style, such ornamentation was a mixture of traditional and local forms, the national motifs themselves probably being determined by an extremely influential French teacher at the Tech., Lucien Henry, who inspired a whole generation of decorative artists and sculptors to use Australian flora and fauna in their designs. (Henry himself was particularly fond of the waratah.)[19] (see Figure 7)

One of the people most influenced by Henry was his colleague, Richard Baker, who organised a permanent display of Australian decorative arts at the Museum and subsequently published *The Australian Flora in*

Applied Art: Part 1 The Waratah (Sydney), 1915). The book was illustrated with designs by Henry and his students. More notably, perhaps, Baker also collected specimens of Australian marble for exhibition and wrote a pioneer book about them. (He wrote other important books on the trees, woods and grasses of Australia.) Such national awareness, intended for a local rather than British audience, seems to have emanated almost exclusively from the Sydney Tech. and it is therefore appropriate that its buildings echo its preoccupations at the time. Michael Dysart's concrete monster on Broadway for the Tech. of the 1970s (NSWIT), done under the auspices of the Government Architect's Office, Philip Cox's NSWIT extension into the old Fruit and Flower Market buildings in the Sydney Haymarket, and the Powerhouse Museum now completing for 1988 are, I think equally obvious indicators of social values. (see Figure 8)

Figure 8. Philip Cox, New South Wales Institute of Technology extension, incorporating the old Fruit and Flower Markets, Haymarket.

There is no escaping the mixture in scientific architecture - as in everything else - of money and mind: of crass commercialism and high-minded disinterested research. Certainly, when we examine the architectural monuments we have created for scientific purposes, both motives and achievements are very mixed. Science in Sydney has not only remained the poor relation of commerce, bureaucracy or government, it also pales into insignificance against the monuments to Medicine or Education. The only time Science begins to look good is when we compare its buildings to our nineteenth-century monuments to Art and Culture.

One wing of the thin, but impressive, facade of the Australian Museum was completed in 1868; the N.S.W. Government Architect, W.L. Vernon, finished the equally thin facade of the National Gallery of N.S.W. in 1902. Until 1969 the Art Gallery was Sydney's supreme example of skin-deep public homage - a one-room deep temple in front and a low shed behind. Science may not then have scored so badly in comparison, but where is its Opera House today? It seems unlikely that the Government Architect's office will provide either external glory or internal revelations at the new Powerhouse Museum. Nuclear reactor stations such as Pine Gap seem likely to remain the most dramatic, expensive and revealing architectural monuments to Science we now create.

NOTES

The Editor expresses his thanks to Dr. Terry Smith, Candy Bruce and Sarah Workman for their help in securing these footnotes and the illustrations in Dr. Kerr's absence.

1. William Dawes (1762-1836), naval officer astronomer and surveyor, arrived Sydney with the First Fleet. He laid out many of the first streets of Sydney and explored the Upper Nepean area. His papers are held at the Mitchell Library.

2. Sir Thomas Brisbane (1773-1860) built at this family home at Brisbane House the second observatory in Scotland. At Parramatta he made the first observations of stars in the southern hemisphere since the mid-eighteenth century. He built a third observatory at Makerstoun in 1826 and later became president of the Edinburgh Astronomical Institution.

3. Alexander Dawson (b. 1817) first worked in Hobart before being invited to Sydney in 1856 by Governor Denison to replace Weaver as Colonial Architect.

4. Sotheby's (Sydney) 17th October, 1984. (Schooner at Anchor against Sydney Panorama).

5. *Sydney Morning Herald*, 20 April 1907.

6. *Sydney Monitor*, 20 July 1833.

7. Mortimer William Lewis (1796-1879) was at first town surveyor under Sir Thomas Mitchell before becoming Colonial Architect in 1835. Fifteen years later he was forced to resign from the post when an official enquiry into the cost of the museum placed the fault with Lewis. He consoled himself by building the Gothic revival Richmond Villa.

8. The statue was exhibited by Sir Charles Nicholson and is presumably the same as that exhibited by Nicholl in London at Westminster Hall in 1844 and then dismissed by the press as a "tame crabbed looking person".

9. James Johnstone Barnet (1827-1904) held the position of Colonial Architect from 1865 to 1890, during which time he was responsible for the design and construction of close to 1500 projects, including the G.P.O., the Colonial Secretary's Office, the Lands Department, the Public Library, the Medical School at the University of Sydney, and the Exhibition Building in the Botanical Gardens.

10. Report of the Select Committee on Sydney Museum, *Votes and Proceedings of the Legislative Assembly of N.S.W. 1873-4*, Vol. 5, 828.

11. Archille Simonetti (1838-1900) came to Australia from Rome in 1871. He was appointed instructor of sculpture and modelling at the New South Wales Academy of Art in 1875 and later, in the early 1890s, ran an "Atelier" from his studio in Balmain. He is best known for his portrait busts of prominent colonial society and for the Memorial Fountain to Governor Phillip in the Botanic Gardens Sydney which was executed between 1889 and 1897 at the cost of $13,000.

12. Pietro Tenerani (1789-1869) was an Italian neoclassical sculptor with an international reputation. An extract from a letter from Wentworth to Thomas Barker dated 11 August 1858 was published in the *Sydney Morning Herald* (22 October 1858) for the benefit of the subscribers to the 'Wentworth Testimonial': "I think I wrote you from Rome to say to you that I have given my Statue to Tenerani, the most eminent sculptor of Rome, who is to finish it in three years from the date he commenced it, last May."

13. Thomas Mort (1816-1878) wool auctioneer and businessman. He gave the land for St. Mark's Church, Darling Point, and commissioned Blacket to design it. As well, he contributed generously to the building of both St. Andrew's Cathedral and St. Paul's College, Sydney University. The bronze statue of Mort was executed in 1883 at a cost of 3,000 pounds.

14. Alan Cunningham (1791-1839) botanist and explorer was a protégé of Sir Jeseph Banks at Kew Gardens before being appointed to the Sydney colony in 1816. For the next fourteen years he explored much of the eastern coast of Australia, always collecting and cataloguing botanical specimens. A writer for *The Month* (undated journal c.1839 M.L.) wrote of the obelisk: "The pillar has been placed in the dirtiest little puddle of stagnant water it would be possible to find in the entire colony."

15. Tommaso Sani (1839-1915) came to Sydney from Italy in the later 1870s. The Postmaster General involved himself in the G.P.O. controversy on Sani's behalf and the carvings were saved. The affair however had a dampening effect on Sani's career and he was declared a bankrupt in 1889 and again in 1895.

16. Archibald Liversidge (1846-1927), Professor of Chemistry and Mineralogy at the University of Sydney. Active in almost every area of science in the colony, Liversidge was at the peak of his career when Sani chose him to represent Science in the G.P.O. sculptures.

17. Sir James Martin (1820-1886), journalist, politician and chief justice. Martin spent a small fortune

beautifying 'clarens', his mansion at Potts Point. He commissioned Walter McGill to make a life-size replica of the Choragic monument of Lysicrates (now in the Botanic Gardens).

18. Mansfield had a prospering architectural practice and it was said that at one time his annual income rose to 10,000 pounds. Not everyone was pleased by his success and J. O'Davey, a former employee of Mansfield, wrote in his 'Reminiscences': "Batty Langley was his textbook. Smugness was his style and respectability his manner." Due to Mansfield's improvidence and his drinking, in later life his wife was forced to take in boarders.

19. Lucien Felix Henry (1850-1896), artist and teacher, was a political exile who came to Sydney in 1880 and taught at the Mechanics' School of Arts and the Sydney Technical College. Henry produced work in sculpture, architecture and design and was one of the first to advocate the use of Australian flora and fauna in design, and was particularly drawn to the waratah as a motif. His best-known work is perhaps the designs for the stained-glass windows in the Sydney Town Hall.

Power Dept. of Fine Arts,
University of Sydney,
N.S.W., 2006, Australia.

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The Agricultural Society of New South Wales and its Shows in Colonial Sydney*

BRIAN H. FLETCHER

Agricultural shows were a feature of life in New South Wales from early days. The first was held at Parramatta in October 1822 when settlement was confined mainly within the Cumberland Plain and the colony was a mere thirty four years old. Organised by the recently established Agricultural Society of New South Wales the show became an annual event during the 1830s. Unfortunately the Society failed to retain support and by the time it became defunct in 1836 the show had already ceased to exist.[1] By the time this occurred, settlement had spread far into the interior and along the coastal plain. Over the next twenty years a number of regional agricultural societies were founded. Some, like the Hunter River Vineyard Association, the later New South Wales Vineyard Association and the Australian Floral and Horticultural Association had a specialised function.[2] But most were more wide ranging and were designed to serve the needs of all farmers and graziers in a particular district, regardless of what they produced. Each of these societies held annual exhibitions of livestock and farm produce, perpetuating the tradition that had grown up earlier in Britain. Similar events were also organised in the other Australian colonies. Interestingly, the first Agricultural Society was formed not in New South Wales but in Van Dieman's Land. Everywhere, however, in all of the colonies shows were regularly held and were featured in the press.[3]

For the most part, the shows of the 1830s were designed to serve purely regional interests and needs. They were usually held on a vacant piece of land and lasted for one or two days. They provided a focal point for the local community and a forum for the exchange of ideas. By competing for prizes farmers and graziers could be encouraged to raise standards. All could benefit from the opportunity to be brought into contact with the latest innovations in the fields of husbandry and stockraising. Not everyone, however, was satisfied with necessarily fragmented approach that was alone possible in the absence of a central body spanning the whole of New South Wales. As far back as 1838 the *Sydney Gazette* had put forward proposals for a 'general society' which would 'embrace the whole community' and link the activities of local bodies. The editor returned to this theme three years later and in a series of editorials developed the idea more fully.[4] The times were scarcely propitious for drought and depression struck hard in the early 1840s and settlers were interested more in surviving than in forming a colony-wide agricultural society. Not until late in the following decade when further expansion had occurred and the initial disruptive effects of the gold rushes had been overcome were fresh moves made.

On 9 March 1857, following a meeting at the Terminus Hotel, Liverpool, the decision was made to establish the Cumberland Agricultural Society.[5] The impetus behind this meeting came from leading settlers and members of the Legislative Council who sought in part to improve the level of farming in the county where rural enterprise originated in New South Wales. Attempts had earlier been made to found a Society here but, partly owing to the lack of homogeneity among the settlers of the region, these attempts had failed. By 1857 this particular county lagged behind others in possessing no formal organisation devoted to the interests of the man on the land and the new society was partly intended to overcome this problem. Yet, its founders soon came to see their responsibilities as extending over the whole colony. They spoke of their objectives as being 'national' and on 14 March 1859 council agreed that the society should

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'henceforth be designated the Agricultural Society of New South Wales'. It was also decided that the president should be chosen each year from a different part of the colony 'so that effect could be given to its national aims'.[6] A twenty acre site for a showground was obtained on the west side of Parramatta Park adjacent to what became Westmead railway station.[7] This enabled produce and stock to be brought from other regions and exhibited at the annual show which formed the centrepiece of the Society's activities. Some writers have seen this Society as a direct descendant of earlier Agricultural Society of New South Wales that had existed between 1822 and 1836. Whether this was so is a matter of some dispute, but at least the two bodies shared a common desire to promote rural interests throughout the whole colony rather than those of just one part. It was this which distinguished them from the numerous other agricultural societies that existed by the middle of the nineteenth century.

Between 1860 and 1867, the Agricultural Society of New South Wales continued to make its headquarters at Parramatta. Each year, with the exception of 1863 when there was a severe drought, a show was held at the Westmead ground, and at first aroused substantial interest.[8] In scope and range it was a considerable advance on the shows that had been held under the auspices of the 1822 Society. The earlier body had no ground and its shows consisted of little more than a display of livestock and produce, arranged in a corner of the market-place at Parramatta, to coincide with the periodical government sponsored Fair. The show at Westmead, in contrast, was held over two days on a large ground that contained permanent buildings in the shape of cattle yards, cereal and poultry sheds and pens for sheep and pigs. Visitors came from many parts to see the wide range of exhibits, to enjoy the entertainments and witness the ploughing matches that were a regular feature. While lacking 'that intensely exciting character' possessed by other forms of competition, these matches strongly appealed to those who enjoyed demonstrations of skill. It was, observed the *Sydney Morning Herald*, 'the regularity of the furrow, and not speed, that was the chief care of the ploughman'.[9]

Despite the initial enthusiasm which greeted its foundation the Society failed for long to fulfil its promise. Membership remained small and attendance of council members at committee meetings gradually declined. Once the novelty of the early shows had gone crowds fell off and although exhibits were sent from the Hunter region and from Port Macquarie, other districts proved unwilling to co-operate. Increasingly it became evident that the Society's original name of 'Cumberland' fitted reality rather more than did that of 'New South Wales'. Part of the trouble was that during the 1860s the Society faced increased competition from the growing number of local bodies that were formed. Their presence diverted attention away from the New South Wales Society and reduced the prospects of exhibits being sent to Parramatta. The show at Westmead was no better than those in other regions and its awards carried no particular significance. Far from developing in the way that had been anticipated, the Society lost ground and by the late 1860s was merely one of a number of bodies that possessed little more than local importance.

It was in response to this situation that there ensued a series of moves which culminated in the reform and reconstitution of the Agricultural Society. According to a later account the initiative was taken by the entrepreneur and adventurer, Jules Joubert, whose family played a major role in the history of Hunters Hill. In an autobiographical work, appropriately entitled *Shavings and Scrapes from Many Parts* Joubert described how he had attended a meeting of the Agricultural Society in February 1867 called to find means of overcoming a serious financial deficit. A member, whose name Joubert did not reveal, moved that the Society be wound up, but Joubert recommended its reconstitution on a broader basis. He urged that it be given a new council and be moved to Sydney were a more elaborate show could be organised. Although taken aback by the audacity of these proposals, those present, he claimed, accepted them.[10] The reality was rather different. No meeting was recorded on the date he gave and his name was not among the committee members for 1867. Nor was the financial plight of the Society as grave as he made out. The impetus for

reform came not from Joubert, a mere outsider, but from longstanding members of the Society who saw the need to overcome its failings.[11] Particularly important were leading settlers from the Hunter River, like J.R. Nowlan, and from the Mudgee area, like G.H. Cox member of an important pioneering family.[12] Such men were enlightened farmers and graziers who believed in the necessity for a central society to raise standards throughout the colony. To succeed, such a society had to be based in the metropolis which was a focal point for the transport system and which possessed port facilities. Early in 1868 steps were taken to move the Society from Parramatta to Sydney.

This move marked the beginning of an association between Sydney and the Agricultural Society that has lasted until the present day. The relationship between City and Society, however, was initially somewhat different to what it later became. Some time was to elapse before a permanent ground could be found and in the meantime the Society lacked a base. Office accommodation was obtained in Lyons Buildings, George Street and for exhibition purposes a lease was secured from the City Corporation allowing the use of the Cleveland Paddocks, or Prince Alfred Park as it is better known, for two months of the year. At first the ground, although close to the railway terminus at Redfern, had little to recommend it. Joubert described it as 'a quagmire with a filthy drain running across it - a plague spot'. In 1869, however, the City Council erected a permanent exhibition building which the Society was allowed to rent for the duration of the show. Here, over the next twelve years the Society's exhibitions were held, temporary pens, marquees and other structures being erected to supplement the main building.[13]

The move to Sydney formed only part of attempts to breathe fresh life into the activities of the Society. A revised set of rules, issued early in 1868 opened with the statement that it was,

> An association of persons desirous of disseminating such information amongst those who derive their sustenance directly from the land, as may enable them to obtain the highest results from the application of skills, energy and capital to farming and pastoral pursuits.[14]

The overriding objective was to promote husbandry, 'including improvements in the breeding and treatment of livestock, by the enlightened combination of Practice with Science'. This last phrase became the motto of the Society and echoed that of the Royal Agricultural Society of England which had been founded in 1842.[15] Members of the New South Wales body were to be encouraged to develop,

> A friendly and parental relation towards the kindred societies of the colony, to increase their number, and to use their best endeavours to unite and strengthen these local bodies for harmonious and progressive action.[16]

To secure these ends there was to be established at Sydney a centre for meetings, lectures, a library and a collection of specimens. A close association with local and overseas societies was to be fostered, transactions were to be published, district reports were to be issued and members were entitled, on payment of a moderate fee, to obtain scientific advice. Gratuitous information was to be made available on 'the geological distribution and character of soils, the secrets of insect life and atmospheric phenomena'. Experiments were to be conducted into the growth of plants and prizes were to be offered to young people so as to develop particular skills.[17]

It was recognised that if these objectives were to be attained, an improved administrative structure and enlarged membership were necessary. 700 copies of the rules were distributed and recipients were invited either to become ordinary members at one guinea a year, or to become governors on payment of three guineas a year. The society was to be run by a president and ten vice presidents elected from among the governors, and council of fifty members, twenty five of whom were to be elected annually. The council was empowered to appoint from within its ranks a number of subcommittees. These included: finance, exhibitions, publications, lectures and scientific matters.

These reforms were the work of a group acting under the chairmanship of Sir William Macarthur, the youngest son of John Macarthur and a wealthy settler with interests in practically every branch of farming

and grazing. He had been elected president of the Society in January 1868 and amongst those who assisted him were men of great talent and skill.[18] They included the former Colonial Secretary, Edward Deas Thomson, politicians and leading settlers such as John Hay, John Lackey, R.L. Jenkins and James Pye. There was also a sprinkling of businessmen and scientists. This last group counted amongst it number, Professor John Smith, who held the Chair of Chemistry and Experimental Physics at the University of Sydney, A.M. Thompson, Reader in Geology and Charles Watt who was an analytical chemist.[19] For such experts to be associated with the Society represented a new departure of considerable significance. Another member of key importance who joined later was Howard Reed, formerly agricultural editor of the *Illustrated London News* and currently on the staff of the *Sydney Morning Herald*.[20] Overall, the composition of the office-holders and the council reflected the determination of the Society to broaden the basis of its activities and become a colony wide institution.

Over the next decade or so, the Society had remarkable success in fulfilling its objectives. Numerous scientific investigations were conducted, papers and lectures were regularly delivered and a substantial library was developed. Beginning in July 1868 a *Journal*, the first of its kind in Australia, was published and this soon came to occupy an important place in the annals of Australian agricultural literature.[21] It kept members informed about the activities of the Society and took up issues of importance to the man on the land. It contained both the reports of investigations carried out by the scientific committee and articles on practical subjects. Metereological information obtained from stations set up all over the colony was also issued. In addition, the Society did much to assist local societies. Publicity was given to their activities in the *Journal*, prizes were offered at the metropolitan exhibition and medals were provided for award at local shows. From 1875 the Society administered an annual grant which the government made available for local bodies.

Central to the activities of the Society, however, was the annual Sydney show which generated the finance that made all else possible. It is important when considering the shows of the late 1860s and the 1870s to remember that they occurred during an age of exhibitions. London had set the scene in 1851 with the Great Exhibition which reflected Britain's position as the world's leading industrial power. A second such occasion was organised in 1862, by which time Paris had hosted the first of three exhibitions to be held in the capital between 1854 and 1878. Vienna provided the venue for an exhibition in 1873 and Philadelphia followed suit three years later to celebrate the centenary of American Independence. These occasions, as Professor Graeme Davison has recently noted, were the outgrowth of earlier and more modest provincial exhibitions. Their popularity derived in part from the fact that at a time of limited educational opportunities they enabled the masses to gain first-hand knowledge of technological change. 'Only if we appreciate the capacity of objects to stir the curiosity and imagination of nineteenth centurey people', observed Davision, 'will we understand the popular appeal of the international exhibition'.[22] Each of these events aroused considerable interest in the Australian colonies. Press coverage was detailed and exhibits were sent from New South Wales and elsewhere. Reports of their reception attracted much attention and patriotic feelings were stirred.

All of this created an atmosphere highly favourable to the efforts of the Agricultural Society. Indicative of its desire to capitalise on the situation was the decision early in 1868 to appoint Jules Joubert as secretary. He was known to be a man of energy and enterprise with a flair for showmanship. His hand was discernible in all of the exhibitions that were held in Sydney over the next few years and he helped to lead the Society away from too exclusive an involvement in the interests of the man on the land.[23] Backed by a council that contained a solid core of Sydney based manufacturers, retailers and merchants he developed the show into something more exciting and wide ranging than had ever before been seen in the colony.

The first clear revelation of Joubert's talents became evident at the second of the Society's Sydney shows in 1870. The first had been a relatively uneventful affair that occurred too soon after the move to the capital for the Society to have put its act in order. 1870, however, was the centenary of the discovery and naming of New South Wales by Captain Cook. It was universally agreed that major celebrations should be held, drawing on the resources of other colonies as well as New South Wales. The Society was at the forefront in promoting this idea and its plans found ready support. Preparations were conducted at several levels. First were the arrangements for the layout of the ground. These were undertaken principally by the Municipal Council. Plans for the construction of an exhibition building, 250 feet long, 140 feet wide and 70 feet high, were laid before the Society's own Exhibition Committee in October 1869 and subsequently approved. Delays occasioned by wet weather resulted in the building being completed only just in time, but the result was seen as justifying the effort and the money. The largest structure of its kind in the Australian colonies and equalling the best in Britain and Europe, the Hall was seen as a masterpiece of design. Impressive from the exterior, it was capable of housing large numbers of exhibits and a vast crowd of visitors, who could not only wander around the ground floor, but gain an overview from the galleries. The Municipal Council, which bore the cost of the building, also erected in the grounds a variety of other structures, including a 'commodious refreshment pavillion'. A thousand trees were planted and the ground was surrounded with a fence in which were set three 'ingenious' gates each with a mechanism for counting entrants. Amidst all this splendour and a range of marquees stood, somewhat incongruously, a 'mean-looking shed' from which the secretary of the Society was to conduct his business in a way that commanded widespread admiration.[24]

Practically all of the details concerning the exhibition itself were handled by the Society - a fact which distinguished it from similar celebratory events in other colonies. Elsewhere the practice was for governments to appoint 'paid and responsible agents, acting under the guidance of a paid director'. Arrangements for the 1870 celebrations, however, were handled by voluntary assistants drawn mainly from the Society.[25] Immense effort went into the preparations and these were rewarded by the response to the Exhibition which opened on 31 August 1870.[26] Nothing like it had ever before been seen in Sydney. A division had been made between the agricultural side, in which there were thirteen sections, and the non-agricultural side in which there were seven sections. Every species of livestock was on display with the exception of merino sheep which could not be brought from the interior because of the quarantine regulations embodied in the Scab Act. On the other hand, an abundance of fleeces was exhibited. There was also every kind of farm produce and a strong horticultural section, which included articles from Queensland. There was machinery of all kinds, some horse-drawn but much steam-powered. In addition, there was an impressive array of articles ranging from precision made astronomical and surveying instruments to chemical and pharmaceutical products. For those with cultural interests there was a fine-arts gallery which was elegantly decorated and 'adorned with a superb collection of works of art'. Nor was the history of the colony neglected. Portraits of Captain Cook and a picture showing how he was killed, along with other mementoes, were arranged in such a way as to remind visitors of their past. For the most part, the display was static, but ploughing matches and demonstrations of machinery were arranged. In 1869 these had been held on the Society's land at Parramatta, but this was too far away from the main centre of attraction. Accordingly, matches were arranged in Victoria Park, after the University Senate had decided that the deed of grant prevented the use of the University paddocks. In addition to the normal horse-drawn contest there was an exhibition of a steam-driven road traction engine which had been brought from Brisbane. It had won a prize at Edinburgh and although driven by men unskilled in the art, showed a combination of 'great power with a fair amount of speed'.[27]

According to the *Herald* the exhibition 'has been to this colony what the Great Exhibition of 1851 was to England - what the Exposition Universelle of Paris was to France in 1862'.[28] Besides commemorating the

discovery of the colony it publicised its products and resulted in the placing of substantial numbers of orders. Visitors came from all over Australia and from other parts of the world and it was estimated that close to half the population of New South Wales attended. The Agricultural Scoeity benefited greatly. Its cattle show alone attracted immense interest. 'Had there been nothing else...', commented the *Herald*, 'the stock would have been sufficient to draw crowds of people interested in pure blood, symmetrical forms and high condition. Last year was good, but this year excelled'. The author of these remarks was convinced that here was proof that the Society, far from being 'merely a Sydney affair' had much to offer the colony:

> It is plain now that nowhere could those who are interested in the breeding and grazing of livestock find so good a place of meeting or so good a market as at Sydney.[29]

The sales which followed the exhibition were so successful that Sydney was bound to become 'the great exchange for cattle as well as ideas'.

Scarcely had the exhibition closed than preparations began for the next show, which was to be devoted exclusively to products of the land. This was a deviation from previous policy which possibly resulted from a belief that other branches of industry had received sufficient attention in 1870. The show of 1871 provided no space for non-agricultural exhibits. Even the fine-arts display was dropped, prompting the *Herald* cynically to remark that,

> Those who retain pleasant visions of the pictures in the galleries will find instead the feathered realities of many paintings, together with a confusing medley of clarion and cackle, quite incapable of being transferred to canvas, but most musical to a fancier of poultry.[30]

The array of livestock, farm produce and machinery was impressive, exhibits were sent from all over the colony and from Queensland, Western Australia and Tasmania, while specimens of timber arrived from as far away as California. Other attractions included a series of lectures on agricultural subjects delivered in Saint Paul's schoolroom adjoining the park. Perhaps because a reaction had set in since 1870, perhaps because the range of exhibits was limited, the show was not a success. The new council elected at the end of the year was confronted by a critical financial situation with prizes unpaid and heavy liabilities. Emergency measures were introduced to reduce expenditure.[31] These included the imposition of additional charges on exhibitors and the subletting of two of the rooms at present occupied in Sydney by the Society. The government was also asked for financial assistance and it was decided that future exhibitions 'be not exclusively agriculture but should combine a judicious selection from other industries, fine arts etc.'[32] The show of 1872 was marked by the presence of significant range of industrial goods. Novel attractions included workers demonstrating their crafts and displays of machinery in motion. The results were gratifying. Attendances rose and the increased receipts enabled the Society to cover expenses and repay the debts incurred during the preceding year.

The pattern established at the show of 1872 was followed during the next six years. The emphasis was on attracting a wide range of livestock, farm produce and farm machinery and there were regular ploughing matches as well as deomonstrations of equipment. Combined with this were sections devoted to the retail trade and secondary industry. Each year an attempt was made to have some special feature that might appeal to a larger audience. In 1872 it was decided to focus trienially on intercolonial exhibits.[33] In 1875 an array of articles was sent from the United States.[34] A year later Canada was added to the list of overseas contributors.[35] Particular attention was paid to improving the layout and arrangement of exhibits. In 1875, for example, a large annexe was added to the main building which in itself was laid out in a novel manner. Intercolonial agricultural produce was arranged side by side with that from New South Wales so that comparisons could be made. The normal display of local stained glass was situated so as to heighten the effects of other exhibits and a balcony was constructed featuring an apartment containing costly furniture. A re-disigned display of botanical decorations gave added lustre to the customary 'flowers and foliage and sparkling jets of water'. A further change, this time in organisation, was reflected in the decision to exhibit livestock during the second week, thus providing an additional attraction at a time when attendance might otherwise have fallen off and easing the burden on organisers during the opening

week. Attention was also paid to entertainment.[36] There were two rings, one for judging horses and the other for judging cattle. The space for a large ring was lacking, but the horse ring was used for jumping and other events. Side shows were not as yet in evidence; on the other hand, there were musical events, including a band, evening concerts, balloon ascents and floral shows.

The Society's exhibitions were, therefore, well planned, varied, innovative and of considerable importance. They added a new dimension to the life of Sydney and attracted the attention of other colonies and countries. 'The enterprise of the Agricultural Society and its value to the colony will hardly be questioned', observed the *Herald* after witnessing the ninth Sydney show. It has,

> Rendered most valuable service to the colony by stimulating enterprise, by affording opportunities for comparison, by creating a wholesome rivalry, and by bringing into bold relief the advantages to be derived from improving the breed of horses and cattle, and farming on scientific principles, from the use of labour-saving appliances, and generally from teaching how mind can best operate upon matter, and turn the beneficience of nature to the most profitable account.[37]

Paradoxically, the very achievements of the Society were to prove its undoing. Success had bred a spirit of optimism which gave rise to the belief that there were no limits to what might be undertaken. When, in April 1877 Jacob Levi Montefiore and Samuel Aaron Joseph, two Jewish business partners, proposed that the Society stage an international exhibition in Sydney during 1879, the opportunity was eagerly grasped.[38] Approaches were made to the government by a section of the Executive Committee which assured the Premier that all costs would be borne by the Society. Unfortunately, no attempt had been made to determine whether the resources of the Society were adequate and the Executive Committee had acted in the absence of some members and without informing Council When news leaked out concern rapidly spread among councillors and fears were expressed that the Society would be ruined. There ensued a series of angry meetings and approaches to the government which was in the embarrassing situation of having committed itself to the exhibition. Reluctantly it was eventually forced to assume full responsibility in order to save face after proposed compromises had failed.[39]

The International Exhibition which lasted for eight months from September 1879 until April 1880 was the most impressive event of its kind to be held in colonial Sydney.[40] A grandiose Garden Palace, covering eight and a half acres and designed by the Colonial Architect, James Barnet, was erected in the Domain. It was surrounded by subsidiary buildings and ornamental gardens and became a showpiece until destroyed by fire in 1882. Exhibits were sent from all over the world as well as from other Australian colonies and they covered practically every aspect of life. Some 1,117,536 visitors, including many from overseas, came to gaze in admiration at the spectacle and enjoy the amusements. Financially, the Exhibition was considered more successful than any other except that of 1851 in England. 'The verdict of the great majority of the country seemed to be', observed the Executive Commissioner, 'that it was a substantial gain, and a huge stride in the path of progress, and that the success of the experiment justified what was undoubtedly a bold step'.[41]

For the Agricultural Society, however, the Exhibition was a disaster. The Society lost face as a result of the bitter wrangling on Council that received widespread publicity in the press. The fact that some members of the Executive had misled the government as to the capacity of the Society to shoulder costs, soured relations with ministers and cast doubt on the competence of office bearers. Stockowners were angered by the initial decision to become involved in an International Exhibition which they thought favoured city at the expense of country interests. For a time it looked as though pastoralists might break away from the Society and form an organisation of their own.[42] This step was averted by tension still remained and in country circles there were suspeicions that took some time to die down. Added to all this, the Society was unable to hold a show of its own in 1880 because of competition from the International Exhibition and as a result the financial situation deteriorated.[43] Much of the blame for these

setbacks was placed on Joubert who was forced out of office. Some of the responsibility for the fiasco of 1879 must be placed on his shoulders. Yet at the same time he had helped to ensure that earlier shows were such a success and the advances made by the Society during the 1870s owed much to initiative and enterprise.

The 1879 Exhibition brought to a close an era in the history both of the Agricultural Society and of its shows in Sydney. For some time there had been doubts as to the desirability of the present arrangements under which the Society leased Prince Alfred Park for a mere two months of the year. Increasingly councillors had come to appreciate the need for a permanent base on which facilities could be constructed for use throughout the year. The lease to Prince Alfred Park was due to expire on 30 June 1880 and there was no guarantee of renewal on the same terms as before. Matters were brought to a head by the setbacks of 1879 which left the Society with inadequate funds to cover rental in the coming year. In July 1879 a deputation approached the Mayor of Sydney seeking immediate cancellation of the lease and exemption from half the rental for which the Society was liable. The City Corporation agreed to these terms thus relieving the immediate financial situation. On the other hand, the Society was now without a ground and urgent steps had to be taken to find one. After searching for some months a site was located at Moore Park adjacent to the Cricket Ground on land that had been vested in the City Corporation. In May 1881 a lease was signed giving the Society control of a block some twenty five acres in extent. Additional portions were later added and the form of tenure was changed to give the Society greater security. For the first time since moving to Sydney it had a permanent base on which capital works could be constructed.[44]

The immediate advantages of the move to Moore Park, however, were only slight. Prince Alfred Park had occupied a central position within reach of the railway terminus at Redfern. The new ground was located on the outskirts of Sydney in an area which, although serviced by trams had no train line. Livestock had to be driven on foot along Cleveland Street and produce had to be freighted by road transport. The ground itself was in a primitive state. One observer referred to it as a 'lumpy stretch of sand covered with undergrowth and low bush', while another dismissed it as 'a scrub-covered unprofitable sandy patch.'[45] Inevitably much time and money had to be spent before the ground could be put to effective use and this necessarily meant that fewer funds were available for other purposes. During the 1880s, although the objectives of the Society remained unchanged, it was in no position to pursue so many of them as before. The *Journal* was dropped as were other services including climatic reports and scientific investigations. Nor were lecture programmes held any more. Indeed, almost the only activity in which the Society engaged was that of holding an annual show.

The fact that the Society, for the first time since moving to Sydney, had its own ground was a considerable advantage. At Prince Alfred Park it had been given the use for part of the year of a large building which the City Council had constructed and at show time temporary structures and pens were erected to house exhibits. At Moore Park, however, the Society had access to the ground for the whole year and was able to embark on a programme of planned development involving the construction of a number of permanent buildings. Within months of taking possession of the land construction work had begun and by April 1882 a substantial number of structures had been completed. These included stalls for 150 horses, a three roomed cottage for the grooms, two large sheds for cattle and one for poultry, a dog pavilion and pens for pigs. In addition there was a 'simple yet graceful' pavilion over one hundred feet long and forty feet high, to provide cover for exhibits. These buildings were arranged around the main ring which was surrounded by a fenced and railed trotting track, 689 yards in circumference and thirty six yards wide, 'well ballasted, top-dressed, watered and rolled'. Within this oval were judging rings for cattle and horses and stout fences for horse jumping events.[46] Over the next few years other improvements were made, culminating in the ambitious undertakings begun in 1887 in preparation for celebrating the centenary of white settlement in Australia. By 1888 the ground contained two grandstands capable of seating nearly

5000 visitors, a number of substantial pavilions and a range of well-constructed sheds, pens and other necessary buildings. A loop had been constructed from the Randwick tram line, enabling visitors to be brought to the main entrance where gates and turnstyles had been constructed. Provision had also been made within the ground for a press room and a post and telegraph office.[47]

The Centennial Intercolonial Agricultural Exhibition, which opened on 25 January 1888, catered for 'almost everything connected with ... pastoral and agricultural industries, manufactures and mineral wealth'. Governors from every colony attended the opening ceremony and visitors poured in from all over New South Wales. There were numerous livestock entries, an impressive array of farm machinery including some from Victoria, South Australia and New Zealand, and a working dairy. One novel feature arose from the decision to offer 'valuable trophies' for the best collection of vegetables, farm produce, fruits and flowers from each of four districts into which the colony was divided - western, southern, northern and metropolitan. Here was the forerunner of the later and justly renowned district exhibits competition that has continued to the present day. Interesting too was an advertisement which the Society placed in the *Herald* inviting applications to run side-shows at the exhibition. What response was received is not known, but this was the first reference to what in later years became an important feature of the Easter show. Other attractions, this time dating from earlier in the decade were also in evidence. The existence of a ring had enabled the Society, from the time of the move to Moore Park, to expand equestrian events. Jumping contests were held in the ring and so too were horse parades. On the track that surrounded it there were the ever-popular trotting and bicycle races.[48]

Impressive though the Centennial Exhibition was, it failed to reach the heights attained by the best of the Society's Exhibitions during the 1870s. The *Town and Country Journal* criticised the arrangement of exhibits and claimed that the impression left by the festivities was 'mostly those of headache and indigestion'.[49] Similar comments could have been made of some of the other shows held since 1882. Admittedly, considerable efforts were devoted to making them attractive. The agricultural and livestock side were developed and so too were the ever-popular equestrian events. Advances in the dairying industry were brought before the public and attention was drawn to improvements in methods of transport. The *Herald* commented favourably on the firm of Sage, Glencross and Company of Macdonaldtown for demonstrating,

> A new style of brougham hansom, fitted with such novelties as sliding doors, an electric bell, and a carriage clock. They also display "the lady's Parisian phaeton", a handsome vehicle with extended hood, and excellently arranged springs; and the Carrington dogcart, a very neat turnout.[50]

Yet, in general the shows of the 1880s failed to attract as many entries from the business, commercial and manufacturing world as had been the case earlier. They were also more provincial in character and lacked the inter-colonial dimension. All this reflected the failure of the Society to overcome problems that had their roots in the debacle of 1879. It took some time for the disillusion generated by the events preceding the exhibition of that year to dissipate. Meantime, Council remained weak and divided. The departure of Joubert also left a gap which was filled only after there had been a succession of three nondescript secretaries. Moreover, local societies had strengthened themselves and by 1890, thanks partly to the provision of a government subsidy, their number totalled 103.[51] Their presence, as the *Sydney Morning Herald* noted, diminished the 'interest taken in the metropolitan association' and its shows.[52] Finally, the 1888 show, which might have been expected to give the Society a fresh start in fact created new difficulties. In addition to a financial deficit, doubts were cast by the *Evening News* on the legality of charges to the public for entry to the ground. An article published on 26 January 1888 claimed that the land occupied by the Society formed part of a common and that the Municipal Council lacked the power to lease any part of it under conditions that impeded free access. On 28 January a crowd of spectators forced their way through the gates without paying and when brought before a magistrate the case was dismissed as being beyound his jurisdiction.[53]

Some six years of complicated negotiations were necessary before the government passed an act resolving the problems associated with the Society's tenure of its ground. Meantime, it had passed through a difficult time, made worse by the economic depression of the early 1890s and by public opposition arising from a decision to allow the regular use of the trotting track by the Driving Park Trotting Club. This move was prompted partly by the need to secure additional revenue, but it angered other clubs, prompted the racing fraternity to use its influence in parliament to create trouble for the Society and angered those sections of community opinion which saw gambling as a moral evil.[54] Uncertainties associated with the tenure of the ground deterred the Society from making further improvements, while the criticism to which it was exposed meant that it lost face with the public. All this had a bearing on the show which continued to be held annually. According to one parliamentary critic, however, the metropolitan society was

> ... not in any way equal to some of the country societies in regard to the exhibition of stock and agricultural produce. It is more in the nature of a show of trotting and things of that kind - an endeavour to attract the public by a kind of show business instead of fulfilling the real purpose of an agricultural or pastoral association.[55]

Another observer remarked that,

> On the occasion of the last show there were about four or five bags of wheat, half a dozen bags of maize, two or three kegs of butter, a cheese or two and half a dozen hams. It was the most wretched exhibition of the kind I ever saw.[56]

That these descriptions were unduly one-sided clearly emerges when they are compared with newspaper accounts of the shows of these years. Nevertheless, the Society's exhibitions still lagged behind those of the 1870s both in terms of their scope and level of attainment.

Matters greatly improved during the second half of the 1890s. The Act of 1894 removed doubts as to the Society's tenure of the ground and forbade trotting outside showtime. Economic conditions improved as the depression came to an end and the Society's council was strengthened by the acquisition of leading figures who acted in a united way. With an outstanding President, Sir John See, Colonial Treasurer and later Premier, and an able, dedicated secretary, Frederick Webster, the Society was well placed to progress. Symptomatic of the desire to improve its standing was the decision to apply to Queen Victoria for the right to become 'Royal' - a request that was granted in 1892. The real medium of advance, however, was the show which promised to bring funds and kudos. During the closing years of the decade a concerted effort was made to improve its organisation and widen its scope. A capital works programme designed to improve the ground, extend facilities to exhibitors and cater more effectively for visitors, was begun. Fresh innovations were introduced into the show itself and existing features were extended. The district exhibits competition was given a new form at the turn of the century.[57] Earlier, a wine kiosk had been erected to cater more fully for the needs of the wine industry.[58] Additional classes were added to the cattle competitions and exhibits drawn from the state run experimental farms were put on display under the aegis of the recently established Department of Agriculture.[59] To capitalise on developemtns in the field of transport a special prize was offered for the best motor cars and cycles. In 1901 part of the ground was set aside for their use, prompting the *Herald* to comment that,

> The advent for the first time in the history of the Society, of an imposing array of automobiles on the asphalt track was a most interesting event. Eight or nine automobile carriages, buggies and a post-office delivery van appeared on the track, around which they were driven a great many times. The carriages and buggies, besides the drivers contained passengers who were treated to an exhilarating ride at a rapid rate. Two of the carriages were sent around at a 30 miles an hour speed and the display was warmly cheered.[60]

Nor were the needs of other secondary industries neglected. The number and range of exhibits in this category increased and, in addition, the products of the mining industry were displayed. The entertainment side of the show was also promoted. Equestrian events continued to prove popular as did trotting which was permitted during show time. Woodchopping contests which had originated in the forests of Tasmania during the 1870s, were introduced at Moore Park in 1899.[61] Side shows, which had a somewhat mixed reputation, became permanently established giving the show the character of a fair as well as an

exhibition of practical import.[62] By the turn of the century the show had acquired most of its modern features and was regaining the position it had lost at the end of the 1870s.

Since first being held in Sydney some three decades earlier, therefore, the show had experienced a somewhat chequered history. Yet, despite intermittent setbacks it had survived and established firm traditions. Held over the Easter break it had become one of a number of attractions, including the A.J.C. Races, that drew country visitors in large numbers to Sydney. In some circles Easter had come to be more closely associated with the show than with religious ceremonies - a fact deeply disturbing to the Protestant churches and especially the Church of England which regularly, but unsuccessfully, sought to prevent the holding of the show on Good Friday. 'Easter in Sydney without its agricultural show', commented the *Sydney Mail* in somewhat sacriligous terms, 'would be the play with the chief scene left out'. After overcoming its problems, the show had during the closing years of the century established a premier position in New South Wales. Its prizes were highly valued and their award conferred recognised distinction on the recipient. In terms of its size and scope it also outweighed that of the metropolitan societies in other colonies - a fact which reflected the greater wealth and diversity of New South Wales.

How important the show had been in promoting the objectives of the Society is more difficult to evaluate. Contemporaries, however, believed that the spirit of competition which it generated did produce improvements in the breeds of livestock and the scope and quality of farm products. Every branch of rural production was represented, if not in every year then in most. The distribution of prizes for livestock favoured the larger landholders, but this was not so true of other sections and, in any event, all elements in the rural community benefited from the opportunity to see what could be done by the best farmers and breeders. Not until December 1889 when agriculture was added to the responsibilities of the Minister for Mines did the government do much to promote scientific methods. Meantime the Society, particularly through its show had played a leading role in keeping the man on the land abreast of new developments. It continued to do so in co-operation with the new sub department of Agriculture after 1889. Given that the rural sector of the economy contributed most to export earnings the Society's efforts to increase productivity must have brought benefit to the whole community.

The show was important in other ways too. Although rural exhibits were its main freature the products of other branches of industry were on display in most years. Indeed, no industry lacked representation at some time. The show thus became a focal point for producers of all kinds. Commentators during the later nineteenth century in particular, frequently drew attention to the importance of the show in bridging gaps and in establishing points of contact between town and city. This extended to all levels of society for, by 1900, the show had become a major source of attraction to spectators as well as participants. Attendance receipts varied but rose considerably from £454.0s.6d in 1882 to £4712.2s.0d in 1900 by which stage close to 200,000 people were coming to the ground.[63] This falls well short of the present-day figure but it does represent a higher proportion of the total population than comes nowadays. Country people rubbed shoulders at Moore Park with city folk and all age groups from young to old were represented. The show had already become a central feature of life in New South Wales and helped to provide a bond which linked town and bush. During the next centrury it was to build still further on the foundations that had thus far been laid.

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Department of History,
The University of Sydney,
N.S.W., 2006, Australia.

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"Scientific Sydney"

APPENDIX I

ARTISANS AND MANAGERS: EXPLORING TECHNICAL EDUCATION IN NEW SOUTH WALES, 1884-1984

Saturday 24 November 1984

ABSTRACTS

TECHNICAL EDUCATION IN NEW SOUTH WALES: THE INSTITUTIONAL BACKGROUND

Associate Professor Kenneth J. Cable,
Department of History, University of Sydney, N.S.W., 2006.

This paper discussed the development of the institutional structure of education in the colony in the second half of the nineteenth century. After considering primary, secondary and tertiary education, it indicated the structural place of technical education up to the educational reforms of the first decade of the present century.

TECHNICAL EDUCATION AT THE CROSSROADS, 1900-1914

Miss Joan Cobb,
TAFE, Sydney, N.S.W., 2000.

This paper looked at the foundation of the technical college system from its origins in the Mechanics' School ot Arts and its development into a network of colleges, up to its eventual takeover by the Department of Public Instruction. It traced some of the changes that took place in courses, and examined the structure and objectives of the courses themselves, paying attention also to the most important factors contributing to changes in objectives, and the development of the system under the Department until the outbreak of the First World War.

THE SYDNEY MECHANICS' SCHOOL OF ARTS: A SESQUICENTENARY RETROSPECT

Mrs. Jean Riley,
St. Agnes High School, Evans Road, Rooty Hill, N.S.W., 2766.

The year 1983 was the 150th anniversary of the founding of the Sydney Mechanics' School of Arts. This paper considered the history of the movement, examining the philosophy behind it, the aims of the Sydney Mechanics' School of Arts, the committee's efforts to keep the movement viable, and the movement's contribution to education, both technical and non-technical, until the formation of a government Board of Technical Education.

The paper also reviewed the history of the construction of and renovations to the Sydney Mechanics' School of Arts, based on the minute books of the Sydney School of Arts, records and plans of the architect John Bibb, and newspapers of the day.

THE TECHNOLOGICAL IMPULSE IN COLONIAL NEW SOUTH WALES

Dr. Louise Crossley,
Power House Museum, Ultimo, N.S.W., 2007.

From its earliest beginnings as a penal colony, the technological impulse in New South Wales sprang largely from the need to win first subsistence, then sufficiency and ultimately profit from an alien and unresponsive environment. Much of the technology required to exploit the country's natural resources was imported, but gradually indigenous products and prcesses began to make a significant and often unique, contribution to the economy.

Some of these developments were explored, highlighting the 'colonial' status of technology in New South Wales compared with overseas - a situation still prevalent today.

MAKING HISTORIES: TECHNICAL EDUCATION IN THE LATER 19th CENTURY

Dr. Ann Hone,
Canberra College of Advanced Education, P.O. Box 1, Belconnen, A.C.T., 2616.

This was a work-in-progress paper intended to raise problems in writing histories of technical education. Some of the problems raised were central to the'making' of any historical account; others specific to the task of illuminating past and present education arrangements. Dr. Hone discussed briefly

some histories of technical education and then sketched in some of her own approaches to the technical education in New South Wales in the later 19th century.

'APPROPRIATE TECHNOLOGY' IN COLONIAL DEVELOPMENT

Dr. R.S. Bhathal,
Assistant Director, Power House Museum, Ultimo, N.S.W., 2007.

Most historians and economists have tended to ignore the role of technology in the developement of Australia in the 19th century. This paper explored, through examples in the agricultural, pastoral and mining industries, the role that appropriate technology played in the social and economic development of Australia. It attempted to use staple theory which had been so successfully used in the Canadian and American cases to set the scene to discuss appropriate technology in Australian development.

RECREATING OUR TECHNICAL HERITAGE: THE 'NEW' SYDNEY TECHNICAL INSTITUTE

Mr. John White,
Deputy Director General, Technical Education, TAFE, Sydney, N.S.W., 2000.

This paper discussed some of the major historical developments in technical education in New South Wales as a rationale for establishing an Historical Unit within the New South Wales Department of TAFE. The paper discussed the objectives of such a unit and its role in the collection, presentation and classification of historical matter, and the ways in which such information assessed. Brief reference was made to a possible location for th e unit and its establishment.

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Appendix II

CULTURE AND LEARNING IN THE COLONIAL METROPOLIS

Saturday, 18 May 1985

ABSTRACTS

BENEFICENT PROVIDENCE AND THE QUEST FOR HARMONY: THE CULTURAL SETTING OF COLONIAL SCIENCE 1850-1890

Mr. G. Melleuish,
Department of History, University of Melbourne, Parkville, Vic., 3052.

This paper dealt with the cultural and philosophical context in which science and scientists operated in colonial Sydney. Concentrating on the conceptions of natural religion, Beneficent Providence and harmony prevailing among the educated and articulate colonists, it argued that during this period such men had a coherent view of the world in which "Science" played a leading role. Finally it examined some of the implications of this world view for the understanding of the nature of colonial science.

THE ARCHITECTURE OF SCIENTIFIC SYDNEY

Associate Professor Joan Kerr,
Power Department of Fine Arts, University of Sydney, N.S.W., 2006.

This paper concentrated on a few scientific public buildings in nineteenth century Sydney: the Australian Museum, Sydney Observatory, the Macleay Museum at the University of Sydney, and the Museum of Applied Arts and Sciences. It was shown how public appreciation and awareness may be seen in the erection of architectural monuments specifically dedicated to the Sciences, and in their significance within the local landscape.

Dawes Point Observatory was erected in 1788, before any thought had been given to a church building for the new colony, but this initial enthusiasm for pure science was not sustained from the public purse. The Australian Museu, erected in the late 1840s, was the first notable public monument for scientific purposes; it inaugurated a new interest in scientific pursuits during the 1850s. Yet the heyday of buildings for exclusively scientific purposes did not dawn until the 1880s and 1890s; even then, none was a pivotal work in Sydney's architectural history.

How did such public architectural tributes to the place of science in the community relate to its major applied role in creating and changing the Sydney scene?

GENTLEMEN SCIENTISTS: THE MACLEAYS OF ELIZABETH BAY HOUSE

Ms. Shar Jones,
Curator, Elizabeth Bay House, Elizabeth Bay, N.S.W., 2011.

This paper examined the activities of the Macleays of Elizabeth Bay House as leaders and patrons of science in New South Wales from 1826-1891 and attempted to place colonial scientific development in a broader context.

Alexander Macleay (1767-1848) public servant, entymologist plantsman and scholar was influential in British learned societies before coming to N.S.W. in 1826. The development of his 54 acre garden at Elizabeth Bay provided an impetus for horticulture and botanical study in the Colony and as Colonial Secretary he was well placed to provide leadership in the establishment of cultural and scientific institutions.

His son, William Sharp Macleay (1792-1865), public servant, entymologist and distinguished scientific theorist who influenced both French and English scientific thinking, continued his father's patronage of colonial science. During his lifetime, Elizabeth Bay House was a venue for scientific discourse.

After his death a cousin, William John Macleay, grazier, M.P., and enthusiastic naturalist, amalgamated and augmented the Macleay Collections which were greatly englarged after his expedition to New Guinea. He played a vital role as a leader and patron of colonial scientific societies. He was an important benefactor of the University of Sydney and, in 1891, was knighted for his gift of the family collection to the University. It formed the basis of the Macleay Museum, a collection containing rare specimens which continue to excite scholarly interest.

The heritage of the Macleays of Elizabeth Bay House remains however, not only in their own collection, but at the basis of the entire antipodean scientific tradition.

APPENDIX II

NINE ACRES IN CORN AND THE GOVERNOR'S DEMESNE: THE ESTABLISHMENT AND EARLY DEVELOPMENT OF THE ROYAL BOTANIC GARDENS OF SYDNEY

Dr. Lionel Gilbert,
Dept. of History, Armidale College of Advanced Education, Armidale, N.S.W., 2350.

Although no doubt difficult to justify in a penal colony, it seems strange that Joseph Banks did not contrive to send a botanist with the First Fleet. He was, after all, one of the chief advocates of establishing a Colony in N.S.W. However, Banks bided his time, and once the settlement was established he ensured that a succession of collectors and scientific botanists worked in New South Wales to the advancement of science in general and to the advantage of Kew Gardens in particular. These workers included George Caley, Robert Brown and Allan Cunningham.

Sydney's Botanic Gardens began as a farm described by Governor Phillip as comprising "9 Acres in Corn". When better prospects for agriculture were found elsewhere, the old farm, as part of an extensive Governor's Domain, was used as an entreport for the acclimatisation of exotics and the despatch of indigenous plants during a vigorous exchange programme undertaken by Banks and the early Governors.

Governor Macquarie made provision for Botanic Gardens to be conducted on more formal lines within the "Demesne" he rather fiercely defended. About the time Mrs. Macquarie's Road through the Domain was completed in June 1816, he appointed a "botanical soldier", Charles Fraser, as Colonial Botanist.

Thereafter the old Government Farm and the surrounding area on either side of Botanic Gardens Creek, were developed as landscaped gardens by Fraser and his successors, who included Richard and Allan Cunningham, James Anderson, Nasmith Robertson, James Kidd, Charles Moore and Joseph Henry Maiden. These Colonial Botanists, Superintendents and Directors were, in their respective times, obliged to deal with the help and the hindrance offered by Governors, administrators in both London and Sydney, politicians and members of a rising influential local scientific fraternity. Despite criticisms, parliamentary enquiries, stringent budgets and diverse views about the purpose - and even the need - for such an institution, Sydney's Botanic Gardens have become world-renowned for their site and extent, the richness of their contents, the good taste of their planning, their educational impact and their promotion of scientific investigation.

AGRICULTURAL SHOWS IN COLONIAL SYDNEY

Associate Professor Brian Fletcher,
Department of History, University of Sydney, N.S.W., 2006.

Agricultural shows were held in Sydney from 1869, when the Agricultural Society of New South Wales moved its headquarters to the city. First conducted in Prince Alfred Part the show was later centred at Moore Park were the Society obtained ground in 1882. This paper discussed the background and objectives of the Society and the extent to which these were reflected in a succession of shows, the character of which changed substantially over the years 1869 to 1900.

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Doctoral Thesis Abstract: Pharmacological Properties of Himbacine: An Alkaloid from *Galbulimima* Species

SYED ANWAR-UL HASSAN GILANI

This study was undertaken to investigate the pharmacological properties of himbacine, an alkaloid isolated from Galbulimima species native to Australia and Papua-New Guinea. In vitro experiments were performed on various smooth muscle and cardiac preparations as well as in vivo experiments on anaesthetized cats.

In smooth muscle preparations, himbacine (0.1 - 30 μM) was found to inhibit muscarinic receptor activation. The antagonism of acetylcholine or carbachol was competitive and a similar pA_2 value (around 7.2) was found in all smooth muscle preparations studied irrespective of whether the tissues responded to cholinomimetics by contraction (guinea-pig ileum, trachea and oesophagus , rat uterus and stomach) or by relaxation (rabbit aorta). When compared with atropine, himbacine was found to be 70 - 110 times less potent and its potency was similar to that of homatropine.

At concentrations greater than 30 μM, himbacine caused inhibition of smooth muscle contractions induced by histamine, 5-hydroxytryptamine, Ba^{++} or K^+ and was 6 - 10 times less potent than papaverine.

In guinea-pig atria, pA_2 values of himbacine for the antagonism of cholinomimetics were around 8.3 which represent at least 10 times greater potency than that found in smooth muscle preparations. In contrast, pA_2 values for atropine or homatropine in atria were similar to those determined in smooth muscle preparations.

Gallamine, a known cardio-selective muscarinic receptor antagonist was studied for comparison and had 11 times greater affinity for cardiac muscarinic receptors than for muscarinic receptors of ileal smooth muscle. However, antagonism of acetylcholine or carbachol, at higher concentrations was less than expected for a competitive antagonist and exhibited non-linear Schild plots with slopes progressively departing from unity. The degree of antagonism of acetylcholine was less than for carbachol at higher concentrations of gallamine.

In contrast, himbacine was a competitive antagonist over a wide range of concentrations, exhibiting linear Schild plots with slopes not significantly different from 1. The Schild plots for the antagonism of both acetylcholine or carbachol were similar and himbacine was approximately 500 times more potent than gallamine.

At higher concentrations (> 1 μM), himbacine caused dose-dependent decreases in spontaneous rate of atrial contraction, similar to the effects of quinidine and papaverine.

The effective refractory period of guinea-pig atria was prolonged by himbacine in a similar concentration range to that causing bradycardia. The effect was qualitatively similar to that of quinidine but himbacine was 3 times less potent.

In vivo, himbacine (1 - 4 mg/kg), produced dose-dependent falls in blood pressure and heart rate in anaesthetized cats. These effects were similar in atropinised areflexic cats suggesting that no central nervous component was involved. Himbacine at these doses was found to be devoid of any effect on responses of cat

nictitating membranes to pre-ganglionic stimulation in contrast to hexamethonium.

It is concluded from these results that himbacine may be the prototype of a novel class of competitive antimuscarinic agents with cardio-selectivity. Because of its selective action for cardiac muscarinic receptors, himbacine has clinical potential in the therapeutic correction of the brady-arrhythmias currently treated by atropine.

Department of Pharmacology,
University of Sydney,
N.S.W., 2006, Australia.

(Manuscript received 10.12.1985)

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75th Birthday

Seminar

held in honour of

JOHN A. DULHUNTY

at the Department of Geology and Geophysics,
University of Sydney

4th April, 1986

Papers Read:

Session 1. Chairman: David Branagan

R.A.L. Osborne:	Caves and landscape chronology at Timor Caves, N.S.W.
R. Helby *et al*:	The age of the Permian sequence in the Stroud-Gloucester Trough
H.A. Martin:	Tertiary stratigraphy, vegetation and climate of the Murray Basin in N.S.W.
D.J. Swaine:	Rapid weathering of a siltstone.
D.K. Tompkins:	Academic coal studies and the coal industry. The sampling of coal as a bulk commodity.

Session 2. Chairman: Barry Webby

D.F. Branagan:	The Sydney floods of November, 1984.
A.A. Day:	A geophysical study of the Gulgong and Upper Goulburn River District, N.S.W.
R.L. Stanton:	The influence of sedimentary environment on the development of stratiform ore type.
J.A. Dulhunty:	Mesozoic Garrawilla lavas beneath Tertiary volcanics of the Nandewar Range, N.S.W.

Dr. John A. Dulhunty

Journal and Proceedings, Royal Society of New South Wales, Vol. 119, pp. 3-6, 1986
ISSN 0035-9173/86/010003 — 04 $4.00/1

John A. Dulhunty — An Appreciation

B. D. WEBBY

This issue of the Journal and Proceedings is published in honour of Dr. John A. Dulhunty, now in his seventy-sixth year. The contributions were first presented at a seventy-fifth birthday seminar held on 4 April, 1986, under the auspices of the Department of Geology and Geophysics of the University of Sydney and the Royal Society of New South Wales, to recognize the significant scientific achievements of John Dulhunty, formerly Reader, and now Honorary Research Affiliate, in Geology at the University of Sydney. The contributions by his former colleagues, associates and friends cover a wide range of geological topics which reflect John's own broad spread of research interests.

John Dulhunty was educated at the Cranbrook School in Sydney, and at the University of Sydney where he graduated in 1938 with a Bachelor of Science degree with First Class Honours in Economic Geology. He was awarded the Deas Thompson Scholarship for Mineralogy in 1938, and then supported in research appointments by a Linnean Macleay Fellowship from 1940 to 1944, and a Senior Commonwealth Research Fellowship from 1945 to 1949. His research culminated in the completion of his Doctor of Science thesis on the 'Classification and Origin of New South Wales torbanites' in 1946, and through the 1940s and 1950s the publication of an impressive series of thirty two papers on aspects on Australian coals. In addition to 'landmark' papers on the nature of torbanites and the metamorphic development of coal, he published three pioneering contributions (one jointly with his wife Roma) in the field of Australian Permian palynology. In 1951 he was appointed to a Senior Lectureship in Geology in the University of Sydney, and in 1957 promoted to Reader. In addition he was Acting Head of the Department of Geology and Geophysics on three occasions, during 1955, 1961 and again in 1967.

His association with the Royal Society of New South Wales has been an active and fruitful one. Since he first joined the Society in 1937 he has contributed thirty papers to the Society's Journal, served as President in 1947, acted as councillor and advisor on publications at various times. He presented the Clark Memorial Lecture in 1964 and received the award of the Medal of the Royal Society for service to Science in 1970. His other activities include a period as President of the Geological Society of Australia from 1964 to 1965.

Since his retirement in 1973, John has undertaken a most ambitious programme of research. With his wife Roma he has carried out a series of arduous expeditions to Lake Eyre, to document the major sequence of wet-dry cycles of sedimentological and geomorphological change in the lake. This has resulted in a number of important research papers to scientific journals, especially those of the Royal Societies of South Australia and New South Wales. Secondly he has continued to work towards establishing stratigraphical relationships using radiometric dating of Mesozoic-Cainozoic lavas in the central west of New South Wales also with significant research publications.

Colleagues and friends in the Department of Geology and Geophysics and among the wider geological community have an affection and admiration for John, not just because he reminds us of our links with the illustrious line of Sydney University geologists of the past, Edgeworth David, Cotton and Browne, but because we recognize his outstanding geological talents - his stimulating and entertaining lectures, his wise counsel and administrative expertise during periods as Acting Head of Department, his pioneering spirit of adventure, when no longer a young man, to undertake expeditions with Roma to Lake Eyre, and his lasting research contributions on a variety of aspects of Australian geology. For these reasons we were delighted that the Council of the Royal Society agreed to publish the proceedings of the John Dulhunty birthday seminar in the Society's Journal.

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B.D. Webby

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The Sydney Floods of November 1984 and Engineering Geology

D. F. Branagan

ABSTRACT. In the period 5 - 12 November 1984 Sydney experienced some of the heaviest rainfall ever recorded. This paper considers specific localities (Waverton, Bellevue Hill and Dover Heights) where geological conditions contributed in various degrees to the damage by flooding and erosion and consequent human suffering.

At Waverton, where massive Hawkesbury Sandstone outcrops, contributing factors were (a) man-made structures which diverted water from natural drainage paths, and (b) the failure of artifical embankments.

In the Eastern Suburbs the irregular boundary between the Hawkesbury Sandstone and overlying sand makes it difficult to predict the paths of water which sinks into the sands. Once the sands are saturated the potential for failure and heavy flows along the irregular sand/sandstone faces is considerable.

However the main control on flow paths is the artificial topography created by urbanisation. This contributed to major flooding at Rose Bay, after a number of separate sand-water slurries combined to form a single deep stream.

While similar flooding is to be expected again and cannot be entirely prevented, some measures can be taken to reduce likely damage.

The extreme conditions may have influenced the attitude of engineers involved in the discussion on the Maximum Possible Flood concept which, in 1985, led to the decision to carry out major strengthening of Warragamba Dam.

INTRODUCTION

'In the period 5th to 12th November, 1984 Sydney experienced some of the heaviest rainfall on record, and many of the short duration rainfalls were the heaviest ever recorded at many sites' (Riley 1985).

The Urban Flooding Conference held at MacQuarie University in March 1985 (Riley et al., 1985) dealt in considerable detail with the meteorological aspects of the storms, and with some aspects of damage, particularly flooding, (see also Soil Conservation Service, 1985).

Damage to public facilities alone in the Ku-ring-gai and Ryde Municipalities amounted to close to $1 million. These are areas where shale is the predominant surface rock. While there were some places where the surface rock was severely scoured, the major damage seems to have been by high overland surface flows carrying fine silt. Despite the considerable rainfalls over nearly a week, few significant slumps occurred in these areas (Luscombe, 1985).

The damage to these northern areas was largely sustained on the morning of Thursday, 8 November. As this region was the focus for discussion at the Urban Flooding Conference it will not be further considered in this paper.

This paper considers specific localities where geological conditions contributed, in various degrees, to the damage and consequent human misery. The localities studied were (1) Waverton; (2) Bellevue Hill and (3) Dover Heights (Figure 1). At each of these localities major damage, including erosion was caused on the night of Thursday 8th to Friday 9th November.

GEOLOGY OF THE SYDNEY REGION

The geology of the Sydney region is outlined in Branagan (1985). More detailed information can be found in Herbert (1983) and Pells (1985).

For the purpose of this paper the significant factors of the region discussed are (i) the presence of massive Hawkesbury Sandstone, overlain in the northern suburbs by Ashfield Shale or in the eastern suburbs by a variable thickness of essentially unconsolidated sands and/or fill and (ii) the variable topography caused by erosion, weathering, possible uplift and sea-level changes mainly during the Cainozoic era.

WAVERTON

The Waverton locality gives a good idea of the flooding problems met with in much of the Mosman-North Sydney area. However there are local variations of some significance.

This area would be regarded under most circumstances as being unlikely to be affected by heavy rainfall. It is topographically steep, below the plateau surface about Crows Nest (approx 60 m elevation), and consists of resistant Hawkesbury Sandstone with generally adequate drainage, via stormwater channels, to the nearby Berry Bay.

Modifications to the topography consist of a moderately deep road cutting towards the southern end of Carr Street, railway embankments east of Waverton Station near Euroka and Woolcott Streets, each crossed by an overhead bridge, and two levels of fill in Waverton Park, the higher area adjacent to Woolcott Street filled to form bowling greens, the lower to form an oval adjacent to the sea wall, several metres above average highwater level (Figure 2).

Based on interviews with residents, and personal inspection, there were two periods of major damage, one close to midnight, the other about one and a half hours later (Friday morning).

The first of these involved the erosion and slumping of the main railway line embankment which formed an artificial dam blocking the underpass at Woolcott Street (Figure 3). Excess runoff flowing down Carr, Euroka and Union Streets formed a reservoir behind the dam, up to 2 m deep.

Shortly after this dam failed, a considerable amount of coarse embankment debris and fine silt was carried down the eastern end of Waverton Park; this scoured natural loose rock and soil and severely damaged the public footpath as well as causing water damage to several adjacent houses. Much of the debris was deposited on the eastern end of the oval (most if not all did not reach the bay) (Figures 4, 5, 6).

There may have been several periods of blockage and failure as there were overlapping layers of sand and debris deposited across the eastern side of both levels of Waverton Park.

The second major pulse took a different course; water accumulated adjacent to Waverton Station before plunging over a relatively narrow section of the railway embankment and through the lower portion of several home units. (Figure 2).

Sandstone blocks and fragments together with a considerable amount of sandy and silty mud were transported through and around the units on the north side of Woolcott Street and across one of the bowling greens (and adjacent park). The steep southern end of the bowling green, consisting of fill, was severely eroded and this additional material was carried to the oval below (Figures 6, 7). Several individual deltaic lobes could be recognised on the lower oval and it was interesting that relatively coarse coke, because of its lightness had been carried to the distal end of the delta lobes (Figure 8).

Waverton Discussion

Waverton received 209 m.m. in the twenty four hour period from 9 a.m. Thursday to 9 a.m. Friday, the majority during the evening storms. These storms had been preceded by some days of heavy rain, Waverton receiving 47 mm in the previous 24 hrs, North Sydney 76 mm, so that all soils were completely saturated and most drains were close to capacity prior to the events discussed.

A factor in the inability of the drains to cope with the stormwater was the relatively high tide which coincided with this storm.

What was the cause of failure of the railway embankment adjacent to the bridge-underpass? Unfortunately some clearing had already been done and repairs were underway by the time the author was able to inspect this site.

Sandstone underlying the fill of basalt and sandstone cobbles and gravel contained a thin shale lens dipping some 5° towards the south east (i.e. to the free face). There was some evidence of slickensiding here, suggesting that there may have been sliding of the overlying in-situ sandstone and the embankment fill on this surface. Unfortunately any loose sandstone and shale had been bulldozed away in preparation for placing coarser embankment material (at a lower angle than previously).

The flood damage problems at Waverton involved the geological factors of concentrated runoff and ponding due to the presence of steep topography, artificial and restrictive pathways, placing of engineering structures and the juxtaposition of resistant sandstone against relatively weak fill, the latter being at the end point of paths of concentrated flow.

EASTERN SUBURBS

As outlined earlier geological conditions in the Eastern suburbs show several significant differences from those encountered at Waverton.

Though Hawkesbury Sandstone is again the major rock type present, the surface of this rock, east of the city of Sydney, is no longer covered by Ashfield Shale. The sandstone has in fact been subject to a long period of weathering and erosion so that the near-surface sandstone shows considerable variation in hardness. The solid rock surface varies considerably in altitude over short distances, (maximum elevation is 108 m near Bondi Junction), has been cut by basaltic dykes (now weathered to clay) and is covered by a variable thickness of wind-blown and water-deposited sand.

The area between Bellevue Hill and Dover Heights consists largely of fixed dune-sands (solid rock at depths from zero to approximately -65 m) and there are various areas of fill, many near sea-level, others at considerable elevations.

The presence of large surface areas of sand at both high and low elevations permits considerable infiltration, thus reducing, under normal conditions, the volume of overland flow to be expected.

On the other hand, once saturation is exceeded the potential for sudden failure of non-cohesive soils and the potential for heavy flows along the irregular sandstone/sand interfaces is considerable. Such failures did, in fact, occur.

An interesting feature of the eastern suburbs flooding was the restricted courses it took (Figure 9). The irregular nature of the sand/sandstone surface led to a considerable variation in the elevation at which erosion took place. However this irregularity has been only slightly taken into account in the man-made storm drainage system constructed over many years which is essentially related to the natural topography - whatever the underlying conditions.

Be that as it may, when these drains are full and flooding commences, the main control on flow paths is the artificial topography created by urbanisation. While this is to a large degree dependent on the natural topography, the building of stairways, fences, retaining walls, sewers etc. restricts and directs flow into variously-shaped channels in which the flow patterns are poorly understood, if at all. In steep parts of the eastern suburbs the placing of man-made structures appears likely to create turbulent or even jet flow conditions and laminar flow might well be the exception.

Several examples of the problems will be discussed.

Bellevue Hill

At Bellevue Hill erosion occurred on the north-east side of Scots College Oval (Figures 10, 11). Here thick sands have been reworked to provide a suitable surface for the oval.

Following the collapse of the bank considerable damage was caused by sediment-laden water which undercut a house, severely eroded the supports of a second house and deposited a thick layer of sand in front of a lower house.

A newspaper report of this event (Sun 16.11.84) includes a sketch which is somewhat misleading. It shows the bank failure as occurring directly opposite the steep and narrow College Lane and suggests that a powerful jet of sand-laden water from the bank caused the severe damage.

However this was not so - the bank erosion occurred some 20 m north of College Lane, the erosion path on the oval being initially south to north before swinging north east to break the bank some 4 m above Cranbrook Lane and dump a considerable amount of sand on to the road towards the northern end of No.19 (Figure 10).

Despite the relative steepening of Cranbrook Lane towards the north at this point this sand seems to have remained for some time forming a dam, (Soil Conservation Service, op cit). Water running down Cranbrook Lane would then have been diverted to the right down the steps of College Lane. Drainage and sewerage pipes beneath the oval, and essentially along the eroded N-S trench appear to have failed at this time, and the major cause of failure then began to operate - a swift tunnelling beneath the road surface of Cranbrook Lane opposite College Lane which undermined the southern end of the foundations of No. 19 by extremely rapid flow, (Figures 12, 13, 14, 15). The sediment flowed rapidly down College Lane, the bulk of the sand cascading over Cranbrook Road to fill the front of No. 24, two separate flows then continued down Cranbrook Road (Soil Conservation Service, op cit) and Beresford Road (rather less) to New South Head and the western end of Rose Bay. The strength of flow can be gauged by the extensive scouring to concrete forming the base of the lane wall (figure 15).

Less extensive but similar damage occurred by erosion of the edge of Cranbrook Oval (New South Head Road). Rainfall must have been intense here as this site has a restricted catchment and is only a few metres above sea level. Sand-laden flow from this site may have contributed in part to severe erosion of the roadway at the south end of Wunulla Road near New South Head Road (figure 16).

According to a newspaper article (Wentworth Courier, Jan 1985) the failure in Wunulla Road was due in part to blocking of drains with cement slurry which had been washed in by workers after nearby construction operations were completed.

Dover Heights - Rose Bay

The erosion-flood pattern here consisted of a number of separate channels which had their heads west of Military Road with elevations slightly over 80 m, Portland Street (Dudley Page Park) being the highest (Figures 17 & 18). These channels met at the junction of Newcastle Street and New South Head Road, Rose Bay, causing extreme flooding, although erosion here was relatively minor. The main escape way was the narrow North Arcade lane and water unable to enter this funnel inundated nearby shops (Figure 19).

Flow paths were complex, again being largely controlled by man-made structures (Figure 20). Major erosion was confined generally to the initial one third of each stream course and usually occurred where there was a sharp change in gradient (Figure 21). While sand was the major component of the eroded material, some quantities of fill and construction material naturally became involved.

An independent flood path drained south and west from MacLeay and Hardy Streets (Dover Heights) to North Bondi. Major damage occurred adjacent to Dover Heights Squash Club where there is a marked break in slope (Figure 22). This stream seems to have dissipated its energy about Murriverie Street. I did not inspect damage at Vaucluse (Hopetoun Avenue, draining to Parsley Bay, and Vaucluse Road draining to Hermit Bay) reported to me by C.V.G. Phipps, which was severe, although the distance of flow was relatively short. Doubtless numerous other small areas were also affected by concentrated flows.

The Dover Heights - Bondi - Rose Bay area was affected at the time of flooding by a garbagemen's strike, and considerable quantities of garbage

(both putrescent and non-putrescent) had accumulated on the streets. Some of the storm-water drains were rapidly clogged by this material, causing the water to seek alternative paths. The presence of the garbage certainly contributed to the high peak flows which concentrated at some places, but it was probably of minor, rather than major, significance.

CONCLUSIONS

Flooding and erosion of similar magnitude can certainly happen again, and some of the same flood paths are likely to be used.

The concentrated flow paths high in the catchments which passed through individual buildings or along narrow paths need study so that modifications to drainage can be made to divert water before it reaches such points.

The peculiar circumstance of several independent flood flows combining at Rose Bay at peak levels is perhaps remote, but prudence suggests that some modification of the man-made surface topography be undertaken at Rose Bay to divert the flow into the open area west of the main shopping area.

It is clearly not practical to build drains capable of carrying such large volumes of water (and sand), but some improvements could be made to the system as the opportunity offers.

It is interesting that during 1985 major attention was given by the Sydney Water Board (M.W.S. & D.B.) to the concept of the Maximum Possible Flood and the safety of its major dams.

The result has been a decision to increase the spillway capacity at Warragamba Dam and to carry out other protective measures there.

While too much cannot be made of the coincidence, the knowledge of the Sydney floods of November 1984 probably played a part (albeit subconscious) in the discussion of a concept which is now seriously regarded as of great practical significance.

Acknowledgements

Discussion with Professor K.H.R. Moelle and Mr. A. Norman on visits to the sites were greatly appreciated, and Professor C.G.V Phipps drew my attention to some problem areas. Mr. L. Hay of this department drew the figures and typing was undertaken by Helen Young. The University's Department of Illustration prepared the photographs.

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Dept. of Geology & Geophysics
University of Sydney NSW 2006
Australia

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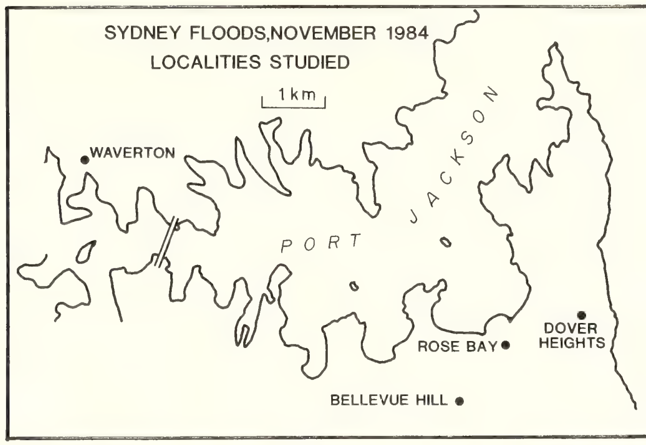
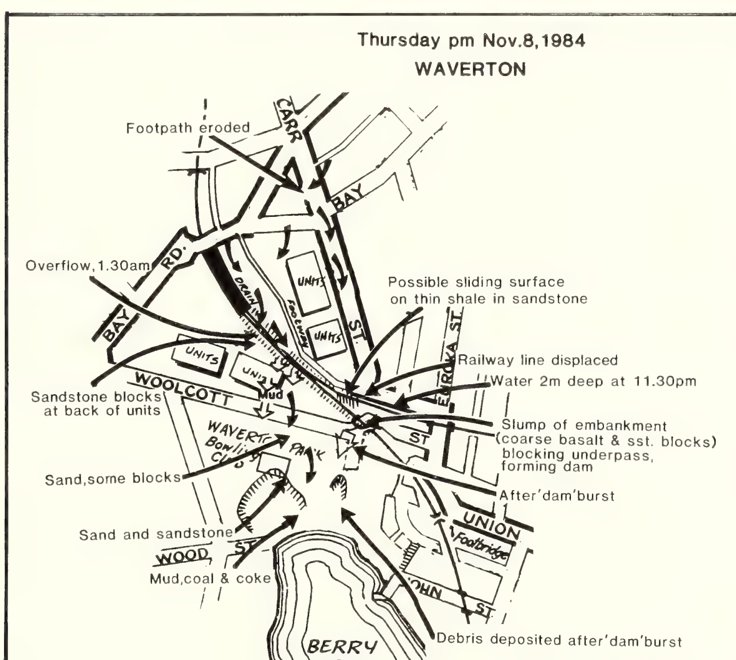


1. Locality map.



2. Waverton area flood and erosion paths.

3. Woolcott St. Underpass on left, embankment restored after slumping.

4. Coarse debris in Waverton Park carried from slumped embankment in right background.

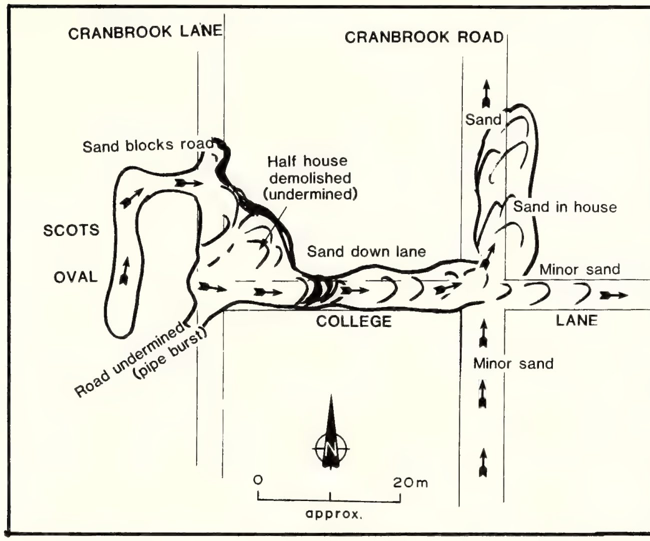
5. Erosion of footpath, east end of Waverton Park, after embankment slump 'dam' failure.

7. Coarse debris on lower Waverton Park mainly derived from undercutting of bowling green fill.

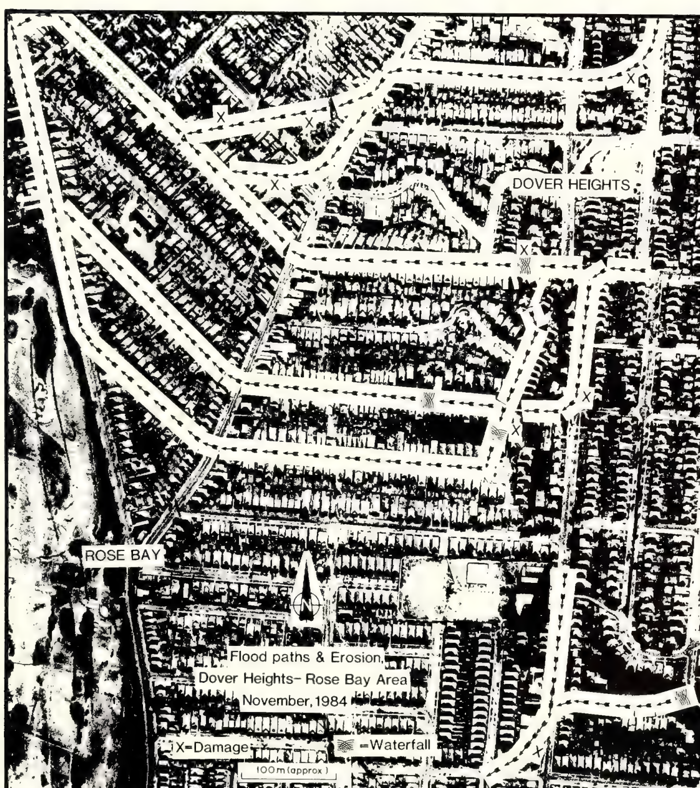
9. Eastern Suburbs flood and erosion paths.

6. Lower Waverton Park covered by debris (mainly sand) from two major erosional events, the earlier in the background.

8. Coarse, but light, coke at distal end of flood delta, near Harbour wall, lower Waverton Park.



10. Cranbrook Avenue area erosion paths.



17. Dover Heights - Rose Bay erosion and flood paths.

11. Scots College scour.

12. Collapse of part of no. 19 Cranbrook Avenue by undercutting.

13. Erosion of Cranbrook Avenue by drain failure and scour.

14. Undercutting along College Lane by sand-laden water from pipe outburst.

15. View up College Lane to Scots Oval. Note narrow, straight, stepped flood course. Concrete along left side of path has been severely eroded.

18. Erosion of thick sand layer at head of Dover Heights catchment (Portland St.).

19. Failure of beach wall and erosion of park fill Percival Park, Rose Bay, the constricted end point of the Dover Heights - Rose Bay flood paths.

20. Collapse of structures built immediately over sandstone along a major flood path (off Liverpool St.).

21. Protective measure adopted below a major waterfall on flood path (Dover Road).
The supporting wall of this house suffered considerable damage.

22. Heavy erosion in Hardy Street caused largely by restricted flow derived from Macleay St. (first right) which has a steep cliff at its eastern end.

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Mesozoic Garrawilla Lavas Beneath Tertiary Volcanics of the Nandewar Range

J. A. Dulhunty

ABSTRACT. Basic alkaline volcanic rocks of Mesozoic K-Ar ages crop out from beneath Jurassic Pilliga Sandstone on the western side of the Nandewar Range, and underlie Tertiary alkaline flows in the western foothills of the range. The lowest Nandewar Tertiary flows lying on Pilliga Sandstone, are about 2 Ma older than those previously dated at higher levels. Thinning of the Mesozoic and Tertiary lavas to the west of the range suggests that both rose, possibly from the same magma reservoir, along fracturing of the Hunter-Mooki Thrust in each of the two periods.

INTRODUCTION

The purpose of this paper is to record results, and discuss implications, of K-Ar dating of post-Palaeozoic volcanic rocks cropping out at low levels in the western foothills of the Nandewar Range between Narrabri and Courada (Fig. 1).

Well known outcrops of Jurassic Pilliga Sandstone occur in Forestry Reserves for 42 km along the western side of the Narrabri-Courada road, as shown on the 1:250 000 Geological Series Narrabri Sheet SH 55-2. East of the road alkaline volcanic rocks crop out and extend to the foothills of the Nandewar Range, where they occur at low levels below erosional remnants of Pilliga Sandstone above which high-level Tertiary alkaline Nandewar volcanics crop out in profusion. Previously it was assumed that the low-level volcanic rocks were part of the high-level Tertiary volcanics, some 17 to 22 Ma old, which had flowed down over outcrops of sandstone probably of Triassic age. The low-level and high-level volcanics were alkaline and could always be distinguished in hand specimens or microscopically.

FIELD STUDIES, RADIOGENIC DATING AND CONCLUSIONS

In 1978, field studies along the Narrabri-Courada road suggested that low-level volcanic rocks occurring to the east, may dip to the west and pass beneath the Pilliga Sandstone on the western side of the road. If so, the volcanics would be Mesozoic and equivalent to Garrawilla lavas. To resolve this possibility, one specimen (K 40) was collected from an outcrop of volcanic rock close to the eastern margin of Pilliga Sandstone at Twelve Mile Hill, 20 km from Narrabri on the Courada road. It was K-Ar dated and found to be 218 Ma old (Table 1), which confirmed the Mesozoic age of Middle Triassic for that particular occurrence of low-level volcanics. In 1985 three more specimens (K 41, K 42 and K 43) were collected from low-level volcanics in the area between the Courada road and the Nandewar Range (see Fig. 1 and Table 1). Ages obtained were all Mesozoic, from 161 Ma to 217 Ma or Late Jurassic to Middle Triassic. Of these, specimens K 42 and K 43 were collected from volcanic rock cropping out from beneath sandstone in the western foothills of the Nandewar Range. Their ages confirmed the Jurassic age of the overlying sandstone, and that it is Pilliga Sandstone.

Two specimens (K 44 and K 45) of high-level volcanics were also collected from previously established Tertiary Nandewar Volcanics, immediately overlying Pilliga Sandstone in the western foothills. Their ages were 22.1 Ma and 20.2 Ma, (Table 1), indicating that they were extruded early in the building of the range, as ages of 17.5 Ma to 18.2 Ma were reported by Wellman and McDougall (1974) for flows higher in the range. Thus, volcanism which built the Nandewar Range, would appear to have extended from 22 Ma to 17 Ma, as originally suggested by Stipp and McDougall (1968).

Three sketch sections (Fig. 2) along lines A-B, C-D and E-F in Fig. 1, were constructed to illustrate general relations between Jurassic and Triassic sediments, and Mesozoic and Tertiary volcanics on the western side of the Nandewar Range. Very little sub-surface geological information was available apart from logs of coal exploration bores drilled some 3 km west of the Narrabri-Courada road, and petroleum exploration bores some 25 km west of the road, kindly supplied by the N.S.W. Department of Mineral Resources. If loss or gain in radiogenic argon has not occurred, extrusion would appear to have taken place from time to time over a period of almost 100 Ma (Embleton, Schmidt, Hamilton and Riley, 1985). In the present case, if the range of 57 Ma is factual or nearly so, the older Middle Triassic volcanics must have been subjected to some 57 Ma of surface weathering and denudation before the younger Late Jurassic lavas were extruded. This could have produced complex subsurface relations between younger and older Mesozoic flows, which is not shown in the three sections.

Bore cores indicate thinning of the Mesozoic lavas as they pass west from the Nandewar Range, as illustrated in Fig. 2. Similar thinning occurs in the Nandewar Tertiary lavas which were almost certainly extruded from the Hunter-Mooki Thrust fracturing above which the Nandewar Range is situated. So it is possible that release of orogenic compression following Permian time may have provided tensional conditions favouring rise and extrusion of lava from Middle Triassic to Late Jurassic. Similar rise and extrusion of lava,

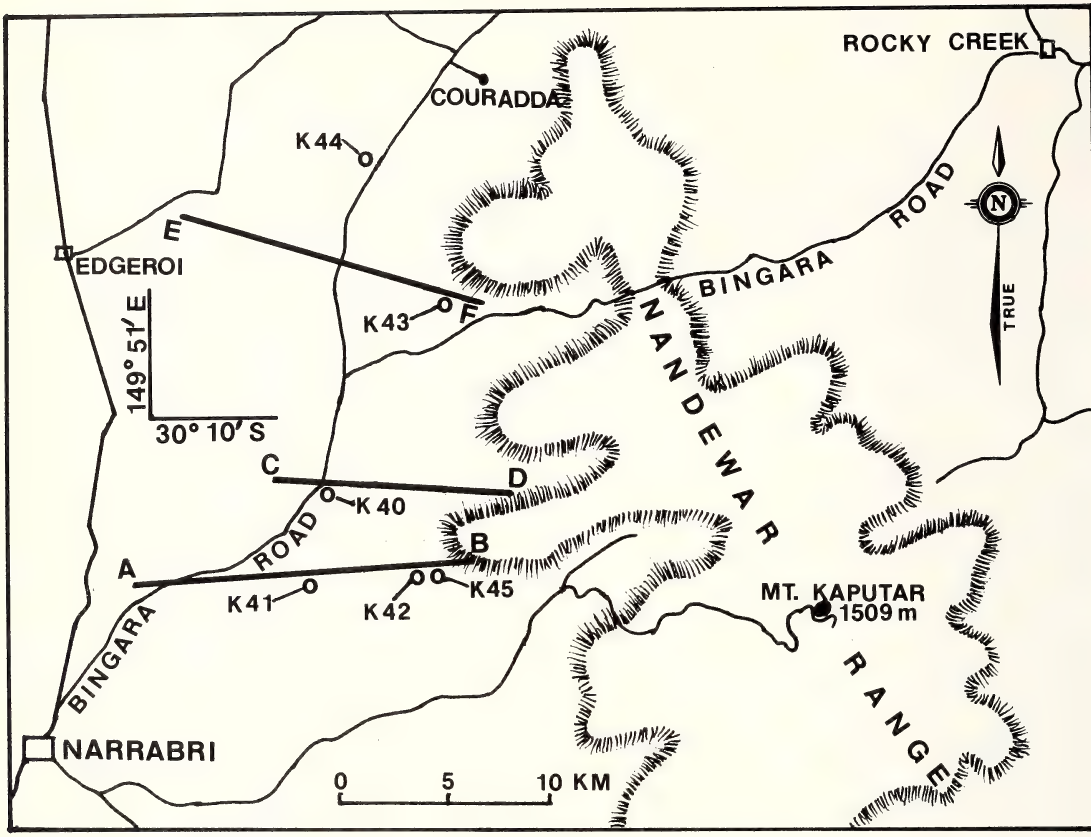

Fig. 1. Locality map of Narrabri-Courada area showing sites of specimens collected for K-Ar dating, and section lines for Fig. 2.

TABLE 1

K-ArAGES OF VOLCANIC ROCKS IN THE NARRABRI-COURADA DISTRICT

Locality	Specimen No.	% K	K^{40} p.p.m.	Rad Ar^{40} p.p.m.	Rad Ar^{40} / Tot Ar^{40}	Rad Ar^{40} / K^{40}	Calc. Age Ma
Twelve Mile Hill	K 40	1.435 1.441	1.754	.02324 .02294	.588 .676	.01316	218 ±9
Mountain Valley	K 41	1.238 1.229	1.472	.01901 .02045	.819 .681	.01341	217 ±9
Gowrie, Horsearm Creek	K 42	1.372 1.342	1.619	.01594 .01577	.711 .741	.0009794	161 ±6
Fernlea	K 43	1.546 1.463	1.795	.02262 .02399	.648 .805	.01298	211 ±8
Moema	K 44	0.616 0.667	0.765	.001000 .000977	.266 .454	.001292	22.1 ±1.5
Gowrie Hillside	K 45	2.397 2.426	2.877	.003225 .003576 .003394	.580 .383 .578	.001181	20.2 ±1.0

Constants Used: $\lambda_\beta = 4.962 \times 10^{-10}$/year $(\lambda_e + \lambda'_e) = 0.581 \times 10^{-10}$/year $^{40}K/K = 1.193 \times 10^{-4}$ g/g

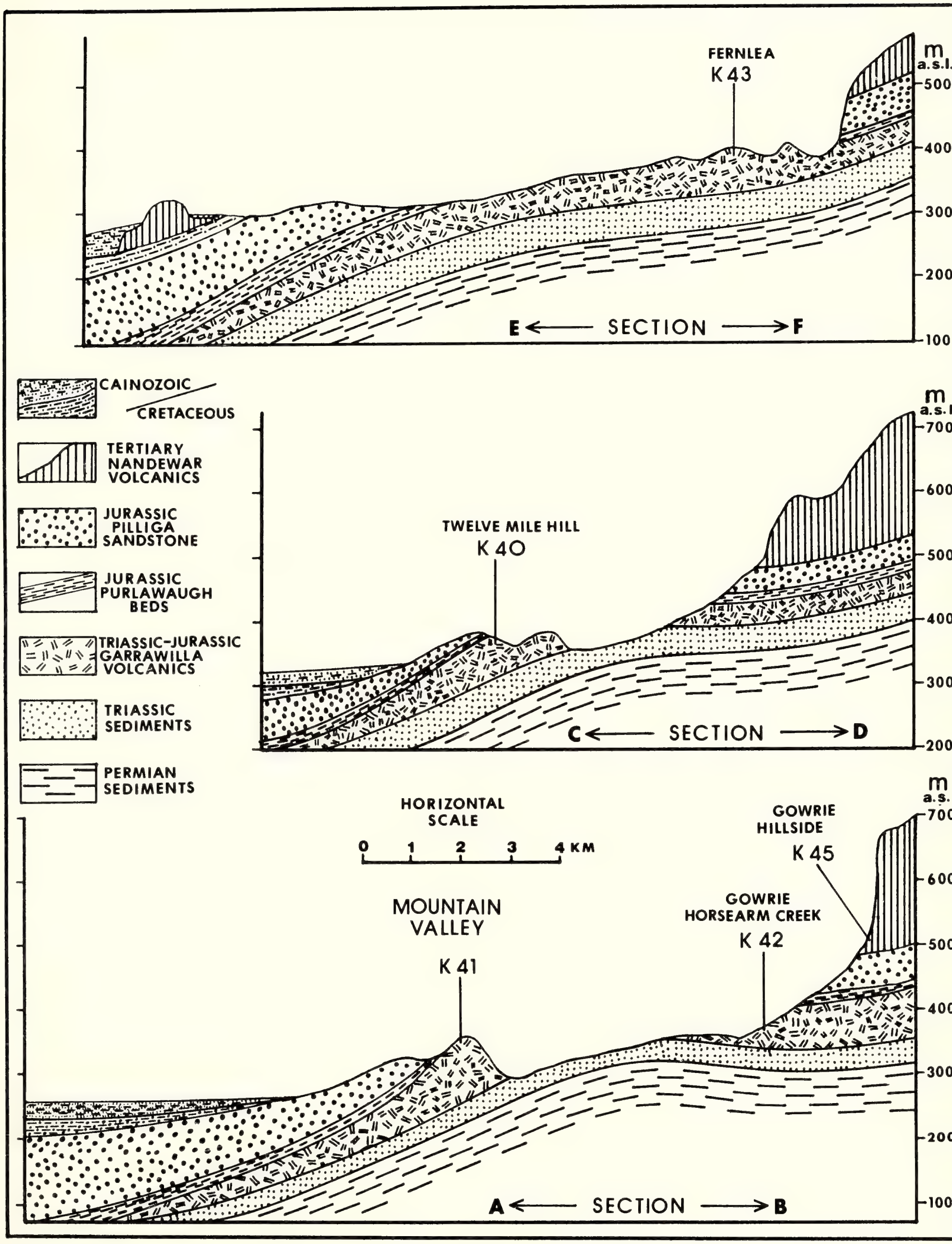

Figure 2. Sketch sections showing occurrence of Mesozoic lavas beneath Tertiary alkaline volcanics of the Nandewar Range.

possibly from the same magma source, occurred again in Miocene time to build the Nandewar Range, under conditions of tension associated with epirogenic elevation of the Eastern Highlands of New South Wales.

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In conclusion it is wished to acknowledge research facilities of the Department of Geology and Geophysics, University of Sydney, helpful co-operation of landholders over whose properties investigations were carried out and K-Ar dating of rock specimens by Geochron Laboratories, U.S.A.

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Department of Geology & Geophysics,
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The Age of the Permian Sequence in the Stroud-Gloucester Trough

R. HELBY, M. LENNOX AND J. ROBERTS

ABSTRACT. Two microfloras from the Stroud-Gloucester Trough indicate Permian ages for two important stratigraphic units within the succession. That from the base of the Alum Mountain Volcanics conforms with the *Pseudoreticulatispora pseudoreticulata* to *Granulatisporites trisinus* Interval Zone of Price (1983) and Stage 3a of Kemp *et al.* (1977), and is Early Permian in age. The second microflora from the Thirty Foot Coal in the Weismantels Formation conforms with the *Dulhuntyispora granulata* to *Didecitriletes ericianus* Interval Zone of Price (1983) and Stage 5a of Kemp *et al.* (1977), and is probably early Middle Permian in age.

INTRODUCTION

The Stroud-Gloucester Trough, located about 200 km north of Sydney (Fig. 1), is a relatively small (*ca.* 38 x 10 km), essentially elongate, north-south oriented, fault controlled structure containing a sequence of Permian rocks about 2000 m thick. The contents of the trough were folded during the Late Permian and Triassic and now form the prominent Stroud-Gloucester Syncline (Roberts & Engel, in press).

Aspects of the geology of the area have been considered by Sussmilch (1922), Osborne (1938, 1950) and Osborne & Andrews (1948), whilst the most comprehensive published account remains that of Loughnan (1955). Loughnan formally subdivided the coal-bearing sequence (see Fig. 2), noting that the sequence unconformably overlay 'Kuttung volcanics'. He suggested that the coal-bearing sequence was developed '..not later than Muree times' (1955, p. 112), and postulated that the initiation of the Stroud-Gloucester Trough and the Cranky Corner Basin was coincident. Voisey & Packham (1969) reviewed Loughnan's data, and suggested (1969, p. 267) that the coal-bearing sequence conformably overlay the 'Carboniferous'.

George (1975) summarised the results of an extensive drilling (432 drill holes) and surface mapping programme. The stratigraphic sequence outlined by George (1975, pp. 255, 256 and Fig. 1) closely resembled Loughnan's, although he extended the Craven Coal Measures to encompass the Spring Creek Conglomerate and Broad Gully Formation, the latter being subdivided into the upper Crowthers Road Conglomerate and a lower unit termed upper Craven Coal Measures. Loughnan's Avon Coal Measures were renamed the Gloucester Coal Measures, and the basal ('Carboniferous') volcanic sequence was assigned to the Alum Mountain Volcanics.

More recently Lennox & Wilcock (1985) summarised a comprehensive study of the geology of the basin. They presented a detailed subdivision of Loughnan's sequence, and also integrated new palynological data indicating a Stage 3 age for the Stroud Volcanics (Alum Mountain Volcanics), and Stage 5 ages for the Dewrang Group and younger strata. The units outlined by these authors will be formalised by Lennox (*in* Roberts & Engel, in prep.). An interpretative comparison of the stratigraphic sequences of Loughnan (1955), George (1975) and Lennox & Wilcock (1985) is presented on Fig. 2.

PALYNOLOGICAL DATA

With the exception of informal references to palynological dating in Lennox & Wilcock (1985), there are no published palynological data concerning the Permian sequence in the Stroud-Gloucester Trough. Indeed, until the mid 1970's only one unpublished palynological report concerning the basin existed.

The first, and perhaps the most significant, contribution to the dating of the sequence in the Stroud-Gloucester Trough was an unpublished report by Hennelly (1961) to the Australian Oil and Gas Corporation Limited, dealing with seven outcrop samples from the Gloucester-Myall area. Specifically, four coal samples including one from each of the McInnes Formation, the Thirty Foot Seam, the Avon Coal Measures and one from the Craven Coal Measures, relate to the dating of the basin. Hennelly interpreted the McInnes Formation sample as Carboniferous. This sample was later examined by Helby (1970, Fig. 3, sample 700). Hennelly considered the other three samples to be no older than the 'upper coal measures' and, by implication, of Stage 5 age.

The present paper records palynofloras from two samples. Sample A (GSNSW 5969) was collected from a thin, carbonaceous unit at the base of the Alum Mountain Volcanics, on the bank of the Karuah River, two km north of Stroud Road. Sample B (GSNSW 5970), was collected from loose surface coal, immediately adjacent to a creek outcrop of the Thirty Foot Coal, 50 m east of the Durallie Road, two km northwest of Dewrang Siding. The location of the samples is illustrated on Fig. 1. In addition, a palynomorph from a Wandrawandian Siltstone sample, located 16 m below the base of the Nowra Sandstone at Pointers Gap, in the southern Sydney Basin, is illustrated and its significance is discussed. Abundant, moderately well preserved palynomorphs were recovered from each sample. The distribution of selected taxa in the samples is outlined on Table 1. Selected taxa from samples A

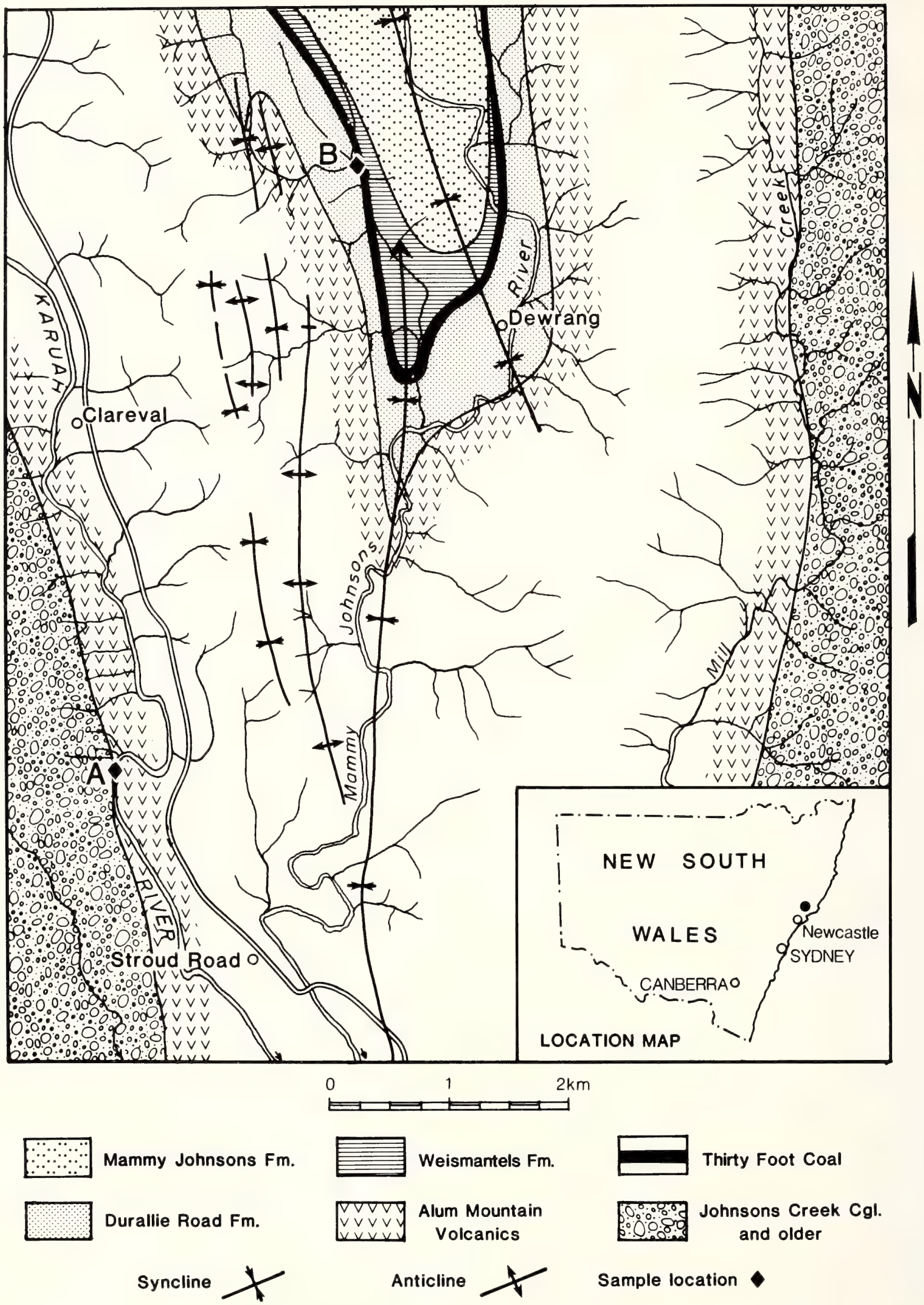

Fig. 1. Selected geology of the southern end of the Stroud-Gloucester Trough showing location of samples.

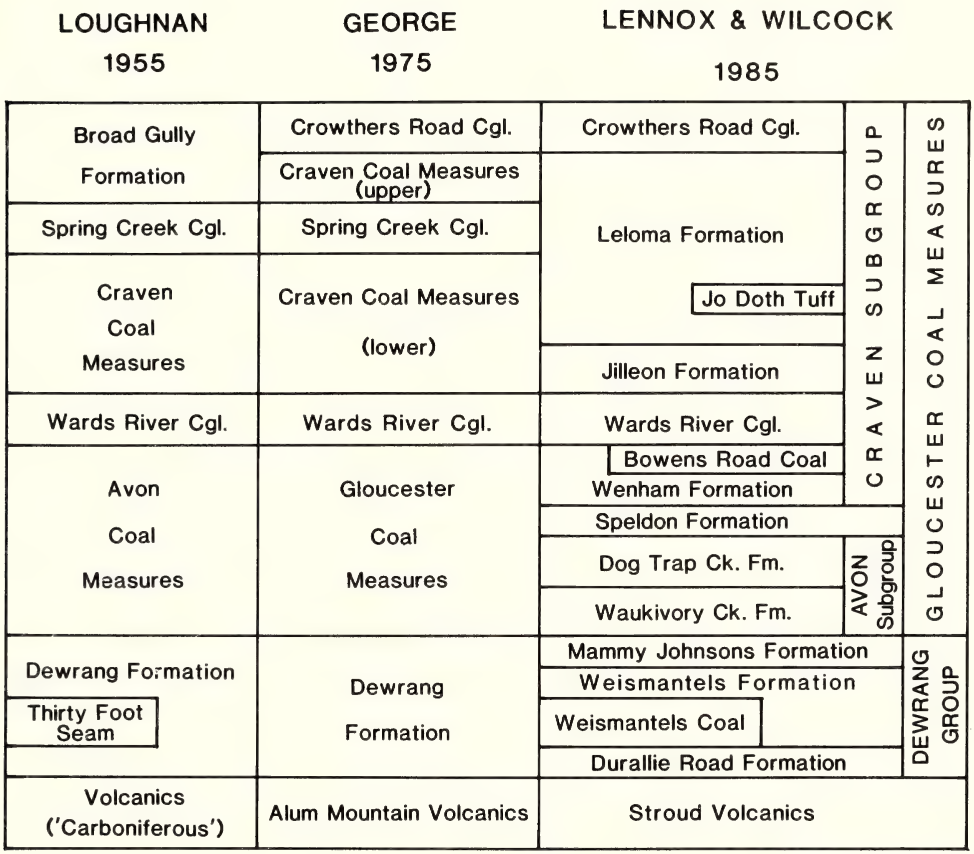

Fig. 2. Comparison of stratigraphic nomenclature applied to the Dewrang Group and the Gloucester Coal Measures.

and B are illustrated on Plate 1 and 2 respectively.

Sample A, from the base of the Alum Mountain Volcanics, yielded a moderately diverse assemblage characterised by the prominent occurrence of *Pseudoreticulatispora pseudoreticulatus* (Balme & Hennelly) Bharadwaj & Srivastava 1969 and the apparent absence of *Granulatisporites trisinus* Balme & Hennelly 1956 and *Phaselisporites cicatricosus* (Balme & Hennelly) Price 1983. This association conforms with the *Pseudoreticulatispora pseudoreticulata* to *Granulatisporites trisinus* Interval Zone of Price (1983 = Stage 3a of Kemp *et al.* , 1977). The apparent absence of *Diatomozonosporites townrovii* Segroves 1970 suggests that the association is marginally younger than basal Stage 3a. McMinn (pers. comm. to ML, 1985) recorded a similar assemblage from samples at higher levels in the Alum Mountain Volcanics.

Kemp *et al.* 1977, p. 204) and Balme (1980, p. text fig. 4) suggested that the Carboniferous-Permian boundary possibly occurs within the lower part of Stage 3a, whilst Powis (1984, Table 1) indicated that the lower part of Stage 3a is equivalent to the upper Gzhelian. Foster (1983) considered that these conclusions are not supported by faunal data. Truswell (1978 *in* Calver *et al.*, 1984) recorded apparent Stage 2 assemblages

TABLE 1

DISTRIBUTION OF SELECTED PALYNOMORPH TAXA

PALYNOMORPHS	SAMPLES	
	A	B
Acanthotriletes tereteangulatus Balme & Hennelly 1956	+	
Anapiculatisporites concinnus Playford 1962	+	
Apiculatisporis cornutus (Balme & Hennelly) Hoeg & Bose 1960	+	
Barakarites rotatus (Balme & Hennelly) Bharadwaj & Tiwari 1964	+	+
Bascanisporites undosus Balme & Hennelly 1956		+
Brazilea plurigenus (Balme & Hennelly)Foster 1979	+	+
Brevitriletes levis (Balme & Hennelly) Bharadwaj & Srivastava 1969	+	+
Cannanoropollis janakii Potonie & Sah 1960	+	+
Cannanoropollis talcherensis Srivastava 1970	+	
Circulisporites parvus (de Jersey) Norris 1965	+	
Crucisaccites monoletus Maithy 1965	+	
Cycadopites cymbatus (Balme & Hennelly) Segroves 1970	+	
Dulhuntyispora granulata Price 1983		+
Ephedripites sp.		+
Granulatisporites trisinus Balme & Hennelly 1956		+
Illinites sp. cf. *I. unicus* Kosanke 1950	+	
Interradispora versus Price 1983		+
Leiotriletes badhamensis (Venkatachala & Kar) Foster 1975	+	
Leiotriletes directus Balme & Hennelly 1956		+
Limitisporites rectus Leschik 1956	+	
Marsupipollenites striatus(Balme & Hennelly) Hart 1965	+	+
Marsupipollenites triradiatus Balme & Hennelly 1956	+	+
Microbaculispora indica Tiwari 1965	+	
Microbaculispora micronodosa (Balme & Hennelly) Anderson 1977	+	
Microbaculispora tentula Tiwari 1965	+	
Microbaculispora villosa (Balme & Hennelly) Baharadwaj 1962		+
Osmundacidites spp.		+
Peltacystia monile Balme & Segroves 1966	+	+
Phaselisporites cicatricosus (Balme & Hennelly) Price 1983		+
Pilasporites calculus Balme & Hennelly 1956	+	
Plicatipollenites densus Srivastava 1970		+
Plicatipollenites malabarensis (Potonie & Sah) Foster 1975	+	+
Plicatipollenites spp.	+	+
Potonieisporites methoris (Hart) Foster 1975	+	+
Potonieisporites spp.	+	+
Praecolpatites sinuosus (Balme & Hennelly) Bharadwaj & Srivastava 1969		+
Praecolpatites ovatus Anderson 1977		+
Protohaploxypinus amplus (Balme & Hennelly) Hart 1964	+	+
Protohaploxypinus limpidus (Balme & Hennelly) Balme & Playford 1967	+	+
Protohaploxypinus rugatus Segroves 1969		+
Pseudoreticulatispora pseudoreticulata (Balme & Hennelly) Bharadwaj & Srivastava 1969	+	
Punctatisporites gretensis Balme & Hennelly 1956	+	
Punctatisporites spp.	+	+
Retusotriletes nigritellus (Balme & Hennelly) Bharadwaj 1962	+	+
Scheuringipollenites ovatus (Balme & Hennelly) Foster 1975	+	+
Striatoabieites multistriatus(Balme & Hennelly) Hart 1964	+	+
Striatopodocarpites cancellatus (Balme & Hennelly) Hart 1964	+	+
Striatopodocarpites gondwanensis Lakhanpal, Sah & Dube 1960	+	
Triadispora sp. cf. *T. epigona* Klaus 1964		+
Tuberculatisporites modicus Balme & Hennelly 1956	+	
Tuberculatosporites sp.		+
Verrucosisporites naumovae Hart 1963	+	
Verrucosisporites spp.	+	+
Vitreisporites signatus Leschik 1955	+	+
Vittatina spp.	+	

associated with invertebrate faunas of Faunizone 1 of Clarke & Banks (1975), apparently supporting Foster's contention. More recently, Foster (in prep.) has proposed a new zone (to replace Stage 2), which is associated, at least in part, with a fauna which he accepts as Asselian equivalent. In any event, the occurrence of Stage 3a (and probably the upper part of Stage 3a) associations at the base and throughout the Alum Mountain Volcanics confirms the Early Permian age of the unit.

Sample B, from the Thirty Foot Coal in the Weismantels Formation, also yielded a moderately diverse microfloral assemblage. The palynoflora was characterised by the relatively common occurrence of *Dulhuntyispora granulata* Price 1983, together with *Praecolpatites sinuosus* (Balme & Hennelly) Bharadwaj & Srivastava 1969 and relatively prominent *Bascanisporites undosus* Balme & Hennelly 1956, *Granulatisporites trisinus* Balme & Hennelly 1956 and *Phaselisporites cicatricosus* (Balme & Hennelly) Price 1983. *Didecitriletes ericianus* (Balme & Hennelly) Venkatachala & Kar 1965 and other species of *Dulhuntyispora* were not observed, suggesting that the association conforms with the *Dulhuntyispora granulata* to *Didecitriletes ericianus* Interval Zone of Price (1983) which is equivalent to lower Stage 5a of Price (*in* Kemp *et al.* 1977). In the Sydney Basin this assemblage has been recorded only from the upper part of the Wandrawandian Siltstone (16 m below the Nowra Sandstone at Pointer's Gap). From these data, we suggest that the Thirty Foot Coal, a major regressive phase in the marginal marine Dewrang Group, is a sequential correlative and probable age equivalent of the Muree Sandstone in the Hunter Valley and of the Nowra Sandstone in the southern Sydney Basin.

A major discrepancy thus exists between the palynological sequence and the faunal sequence. In the Bowen Basin the lower Stage 5 palynofloras are associated with the upper *brevis* to lower *isbelli* zones of Runnegar & McClung (see Foster, 1983, Fig. 6). However, the lower Stage 5a association in the upper Wandrawandian Siltstone is associated with the lower part of the *ovalis* Zone, whilst the lower Stage 5c suites, implied by McMinn (1985, Fig. 3) to occur above the Mulbring and Berry Siltstones, are associated with higher levels in the *ovalis* Zone (McClung, pers. comm.). Clearly, resolution of this problem is beyond the scope of the present paper. Nevertheless, an early Middle Permian age is suggested for the assemblage (Foster, 1983, p. 118).

REPOSITORY

All palynological slides are lodged in the collections of the Geological Survey, New South Wales Department of Mineral Resources. The prefixes GSNSW and MMMC denote samples in the palynological catalogue and the figured microfossil catalogue respectively.

ACKNOWLEDGEMENTS

We are grateful to Esso Australia Limited and Len Hay for photographic and drafting assistance. Graham McClung and Peter Price discussed aspects of the relationship of invertebrate faunas and microfloras, and Andrew McMinn provided unpublished palynological data from his study of the area. Alan Partridge reviewed an early draft of the manuscript.

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Plate 1. Selected palynomorphs from Sample A (GNSNW 5969). Fig. 1, *Apiculatisporis cornutus* (Balme & Hennelly) Hoeg & Bose 1960, MMMC 01741. Fig. 2, 3, *Peltacystia monile* Balme & Segroves 1966, MMMC 01742. Fig. 4, *Striatoabieites multistriatus* (Balme & Hennelly) Hart 1964, MMMC 01743. Fig. 5, *Illinites* sp. cf. *I. unicus* Kosanke 1950, MMMC 01744. Fig. 6, *Vitreisporites signatus* Leschik 1955, MMMC 01745. Fig. 7, *Brevitriletes levis* (Balme & Hennelly) Bharadwaj & Tiwari 1969, MMMC 01746. Fig. 8, *Microbaculispora indica* Tiwari 1965, MMMC 01747. Fig. 9, *Microbaculispora tentula* Tiwari 1965, MMMC 01748. Fig. 10, *Striatoabieites* sp., MMMC 01749. Fig. 11, *Cannanoropollis* sp. cf. *C. talcherensis* Srivistava 1970, MMMC 01750. Fig. 12, *Protohaploxypinus amplus* (Balme & Hennelly) Hart 1964, MMMC 01751. Fig. 13, *Striatopodocarpites gondwanensis* Lakanpal, Sah & Dube 1960, MMMC 01752. Fig. 14, *Pseudoreticulatispora pseudoreticulatus* (Balme & Hennelly) Bharadwaj & Srivastava 1969, MMMC 01753. Fig. 15, *Barakarites rotatus* (Balme & Hennelly) Bharadwaj & Tiwari 1964, MMMC 01754. Fig. 16, *Plicatipollenites malabarensis* (Potonie & Sah) Foster 1975, MMMC 01755. All x500.

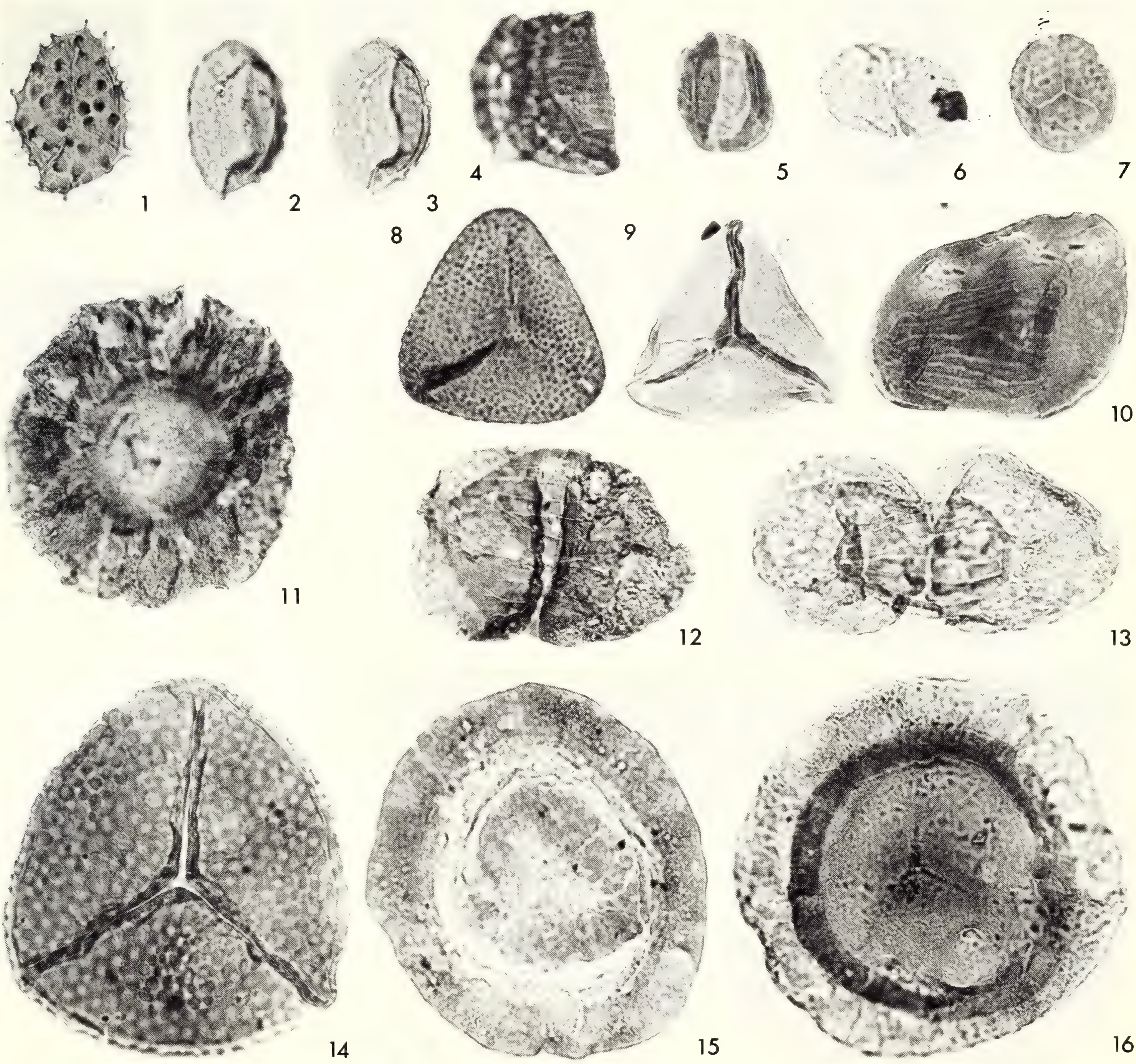
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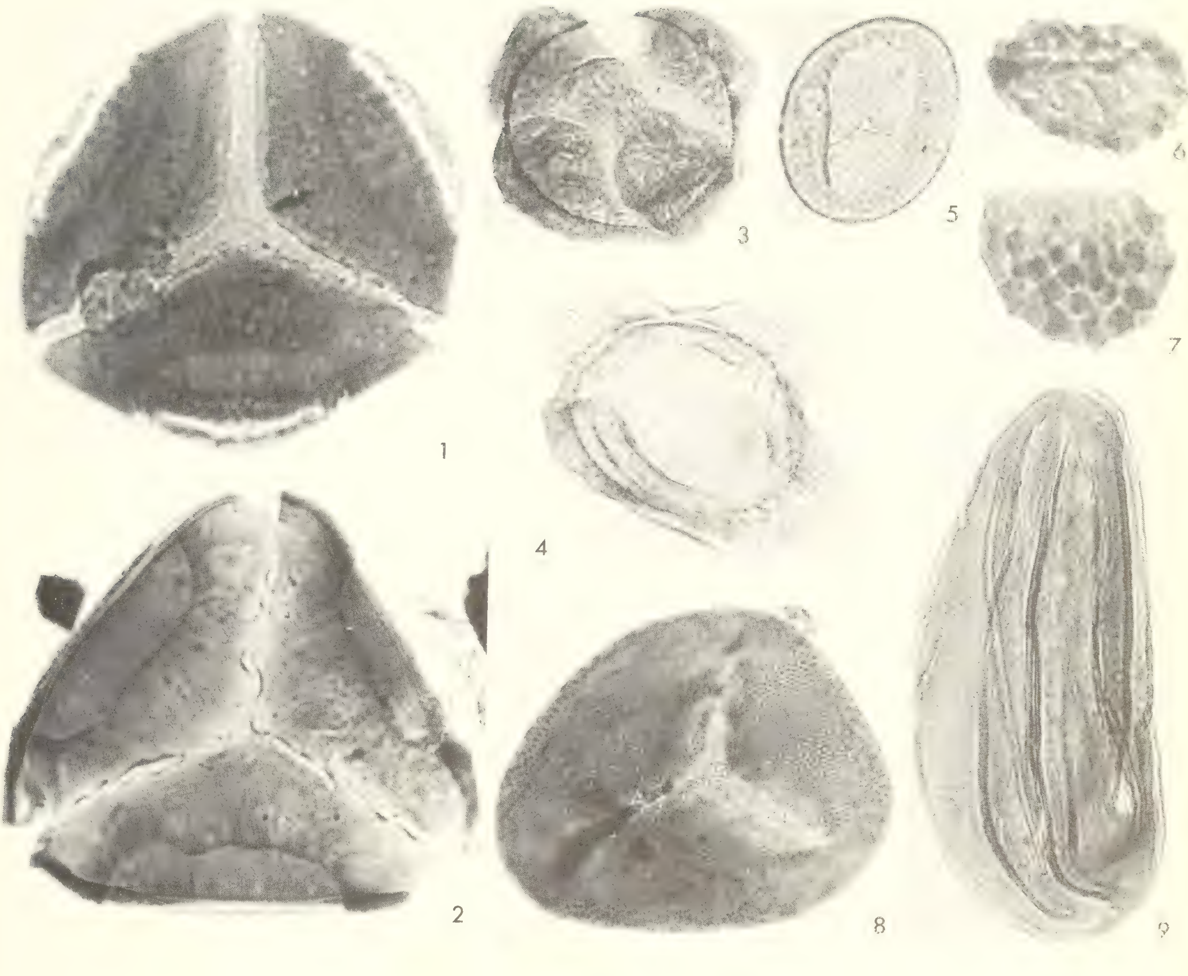
Plate 2. Figs 1, 3-9 selected palynomorphs from Sample B (GSNSW 5970). Fig. 1, *Dulhuntyispora granulata* Price 1963, MMMC 01756. Fig. 3, *Bascanisporites undosus* Balme & Hennelly 1956, MMMC 01757. Fig. 4, *B. undosus*, MMMC 01758. Fig. 5, *Marsupipollenites triradiatus* Balme & Hennelly 1956, MMMC 01759. Fig. 6, *Phaselisporites cicatricosus* (Balme & Hennelly) Price 1983, MMMC 01760. Fig. 7, *P. cicatricosus*, MMMC 01761. Fig. 7, *Praecolpatites sinuosus* (Balme & Hennelly) Bharadwaj & Srivastava 1969, MMMC 01762. Fig. 2, *D. granulata*, typically preserved specimen with scutula and sculptine stripped off, GSNSW 726 (slide 2), sample from Wandrawandian Siltstone. All x500.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1. 356A Burns Bay Road,
LANE COVE, N.S.W. 2066

2. Registrar's Office,
The University of Sydney,
SYDNEY, N.S.W. 2006

3. Department of Applied Geology,
University of New South Wales,
KENSINGTON, N.S.W. 2033

(Manuscript received 29.5.86)

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Tertiary Stratigraphy, Vegetation and Climate of the Murray Basin in New South Wales

H. A. Martin

ABSTRACT. The palynology has been done on some one hundred bores in the Murray Basin, including recent work in the western part of the basin. The oldest of the Tertiary sediments are late Eocene and the youngest containing pollen, middle Miocene. The upper 80 m to 100 m of sediment are barren.

The Oligocene through early Miocene *P. tuberculatus* Zone has been subdivided using quantitative events, or changes in abundance of some common pollen types. These events are reliable for local stratigraphic correlation. One event, the change to high Myrtaceae ratios, may be traced over the whole of the basin, but it is time transgressive, occurring earlier in northwest and later in the southeast.

The palaeovegetation throughout this time was rainforest and there was regional variation over the basin. Through most of the Oligocene, when *Nothofagus* was abundant, the climate was very wet, with constant high humidity. The change to high Myrtaceae, starting in the late Oligocene in the northwest, marked a drop in precipitation, probably to about 1500 mm. In the northwest, araucarians became abundant in the early Miocene and there was a further drop in precipitation, possibly with the development of a seasonal moisture deficit. The top of the pollen bearing sequence, probably mid Miocene, marked a further drop in precipitation to levels below that required to support the permanently wet sites necessary for pollen preservation.

It is thought that changes in the palaeovegetation prior to the late Oligocene are due mainly to the effects of changing sea levels. The late Oligocene changeover to high Myrtaceae and subsequent events are thought to reflect a drier climate which was the result of the developing circum-Antarctic oceanic circulation and glaciation on Antarctica. The events documented here show that the drying out process, which leads eventually to the present aridity, started in the late Oligocene in these inland areas.

INTRODUCTION

The Murray Basin is a shallow, relatively flat Cainozoic feature covering infra-basins of Early Cretaceous, Mid Triassic, Permian and minor Late Carboniferous sediments. Tertiary sediments comprise the bulk of the deposits (Bembrick 1975). The Cainozoic history of the basin is characterised by slow relative rates of subsidence and low rates of sediment supply (Brown 1983). Tectonics in the Cainozoic have been slight and have probably only maintained the relative configuration of the basin (Bembrick 1975).

Ground water is the most important resource of the Murray Basin. In the early days of settlement, shallow wells were sunk for domestic and stock water (Gibbons *et al.* 1972). The Water Resources Commission of New South Wales has now been engaged in a programme of exploration to assess the ground water potential of the basis for approximately twenty years. The Commission is also a participant

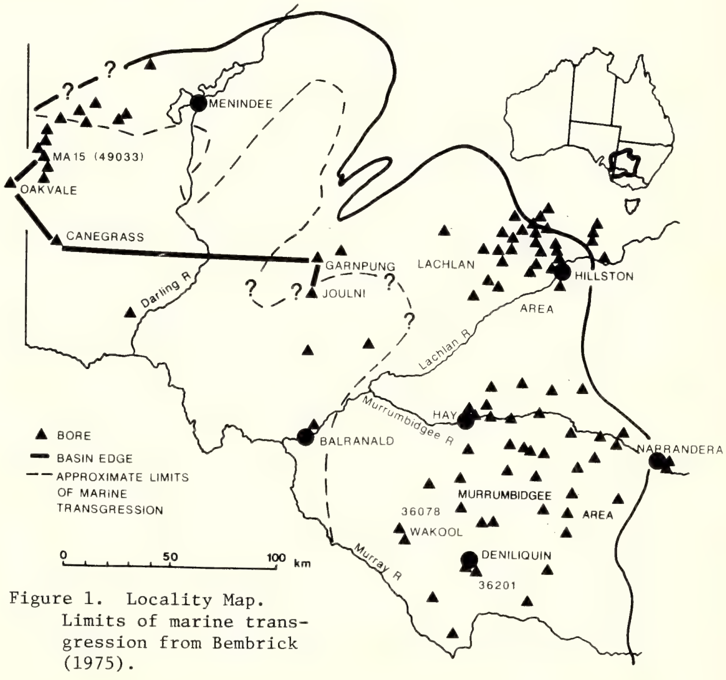

Figure 1. Locality Map. Limits of marine transgression from Bembrick (1975).

of the joint Commonwealth and States Murray Basin Project which will assess the hydrogeology of the basin as a whole. The materials of this study are samples from bores sunk mainly by the Commission. The palynology of approximately one hundred bores has been investigated for this report (see Fig. 1).

This paper presents the stratigraphic palynology and interpretations of the vegetation, palaeoecology and climate of the Oligocene to early Miocene. These latter interpretations add significantly to an understanding of the palynostratigraphy.

STRATIGRAPHY

A number of stratigraphic schemes have been proposed for different parts of the basin by various authors. The schemes most relevant to the New South Wales section are reviewed in Martin (1984b, in press) and are not repeated here. The palynostratigraphy has been described for the eastern part of the basin (Martin 1984b, 1984c). Recent work has extended to the western part of New South Wales which is not well known.

In the eastern part of the basin, the oldest Tertiary spore pollen assemblages may be placed in the late Eocene Middle *N. asperus* Zone (Stover and Partridge 1973, 1982; Partridge 1972). This zone forms a relatively thin veneer over the pre-Tertiary basement. The overlying Oligocene-early Miocene *P. tuberculatus* Zone forms thick sections of up to 170 m and is encountered in every bore. This latter zone is overlain by the latest early Miocene-middle Miocene *T. bellus* Zone which is relatively thin, with few occurrences. The uppermost 80 m to 100 m of sediment have not yielded pollen (Martin 1984b, 1984c).

The thick sections of *P. tuberculatus* Zone have been problematical. Stover and Partridge (1973) have subdivided this zone, but their diagnostic species are infrequent and not reliable in the Murray Basin. There is, however, a clear difference between the base and the top of the zone. Certain species are usually present in the base, becoming infrequent towards the top. A quantitative approach, which utilizes variation in abundance of common pollen types, has been suggested (Martin 1984a) for subdivision of the zone, in the Murray Basin. As well as giving some of the required stratigraphic control, this approach also provides valuable information for reconstruction of the vegetation and environment of the relevant time.

The quantitative method is, however, essentially empirical. High ratios of a selected pollen type with some relevant portion of the pollen count are used to define 'greater abundance' and these high ratios are applied consistently to each bore (Martin 1984a). Truswell *et al.* (1985) have divided the sequence in the Oakvale bore by comparing adjacent pairs of pollen assemblages, using a computer program. When the Oakvale bore is subdivided according to the high ratio method, the same zonation as that calculated by the computer program is obtained and a comparison of the two methods is presented in Martin (in press).

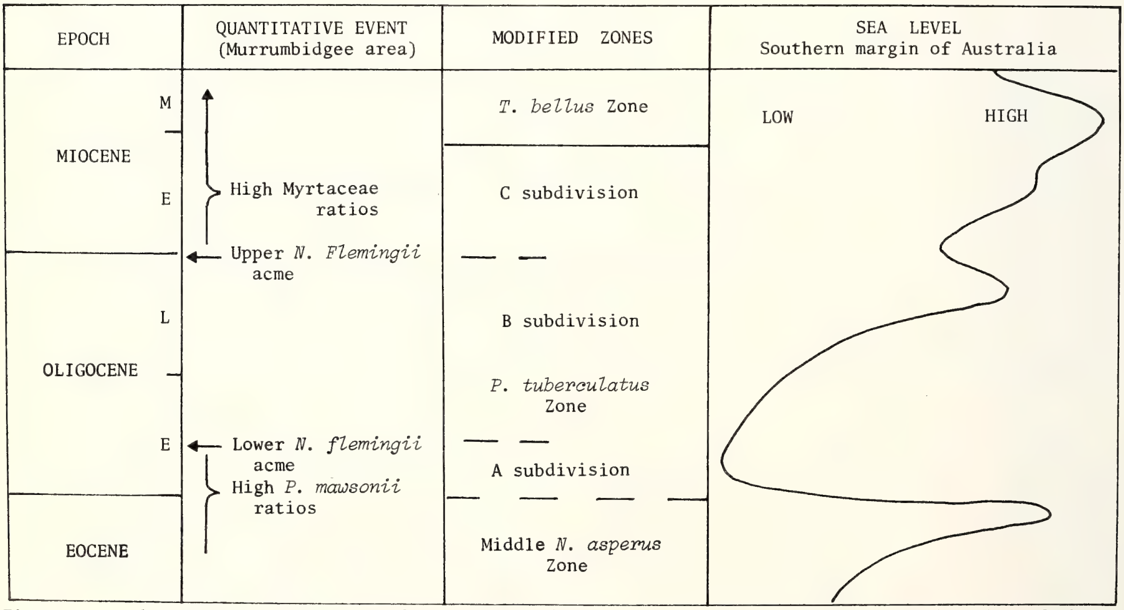

Figure 2. The quantitative events of the Murrumbidgee area and their use for the modification of the zonation of Stover and Partridge (1973, 1982). (From Martin 1984b). The sea level curve is from Loutit and Kennett (1981).

High ratios of three taxa have been shown to be reliable for stratigraphy in the Murrumbidgee area (Martin 1984b). These are:

1. The ratio of *Phyllocladidites mawsonii* to total gymnosperm count. Values above 0.25 are the high ratios which occur at the base of the *P. tuberculatus* Zone.

2. The ratio of *Nothofagidites flemingii* to total *Nothofagidites* count. Values above 0.25 are the high ratios which occur in two layers or acmes, one low down in the *P. tuberculatus* Zone either coinciding with the high *P. mawsonii* ratios, or just above it, and the other much higher up, in the *P. tuberculatus* Zone. The lower and upper acmes may be distinguished by associated species of the assemblage.

3. The ratio of total Myrtaceae count to total *Nothofagidites* count. Values above 0.8 are the high ratios which are consistent, high up in the *P. tuberculatus* Zone, either with the upper *N. flemingii* acme or above it. High Myrtaceae ratios continue to the top of the available section.

The subdivision of the *P. tuberculatus* Zone, using these high ratios is shown in Figure 2. The application of this scheme to the Murrumbidgee area is presented in Martin (1984b).

Extrapolation to the Lachlan area shows certain differences in most bores. With only one exception (see Fig. 7), high *P. mawsonii* ratios are not found there. High *N. flemingii* ratios are not restricted to two layers but may occur anywhere in the *P. tuberculatus* Zone. High Myrtaceae ratios, however, are usually present at the top of the section, as in the Murrumbidgee area. A comparison of the two sections is presented diagramatically in Figure 3. The most important difference between the two areas is the shallower basement in the Lachlan area. The three southern bores of this latter area are situated over the Ivanhoe Trough and the section there is much thicker. In these three bores, the *P. tuberculatus* Zone can be subdivided, in a similar way to those of the Murrumbidgee area.

Only the high Myrtaceae ratios are, however, found in the western area as shown in Figure 4. Indeed, the high Myrtaceae ratios is the most widespread event in the basin and occurs in most bores.

In the western area, other pollen types may be abundant. Truswell *et al.* (1985) record higher values of *Araucariacites australis* (araucarians) in the upper part of the Oakvale section and ratios have been computed from their data for comparison with a bore at Deniliquin in the southeast (see Fig. 5).

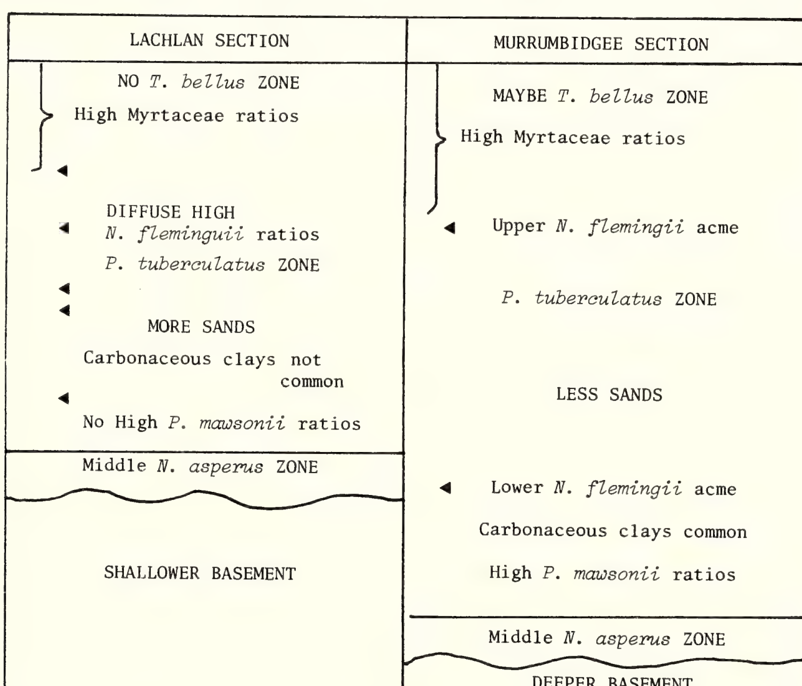

Figure 3. A diagrammatic comparison of the Lachlan and Murrumbidgee Sections. The Lachlan Section depicted here excludes the three southern deep bores which are more like those of the Murrumbidgee area (see text).

The comparison also includes the *N. flemingii* and Myrtaceae ratios. Figure 5 shows that there are no high *N. flemingii* ratios, but minor peaks register at the appropriate stratigraphic levels at the two acmes. Both bores show an increase in Myrtaceae ratios at or above the level of the upper *N. flemingii* acme, but the Myrtaceae ratios are much higher in Oakvale. Similarly, *Araucariacites* increases in the upper part of the section but the ratios are much higher in the Oakvale bore. Very similar patterns are seen in Figure 6 which compares the Canegrass bore in the northwest and a bore near Wakool in the southeast.

PALAEOECOLOGY

The differences in the high ratios between the Murrumbidgee and Lachlan areas may be attributed to differences in depositional environment. High *P. mawsonii* ratios are almost entirely restricted to the mid-dark grey clays in the Murrumbidgee area but such clays are not common in the Lachlan area, where sands are more common. The pre-Tertiary basement over most of the Lachlan area is shallower than that of the Murrumbidgee (see Fig. 3 and Martin 1984a).

The ecological tolerances of the living representatives of the three taxa (see Table 1) used for zonation in the eastern part of the basin form a series with *P. mawsonii in* the wettest habitat, *N. flemingii* intermediate and Myrtaceae (as a general group) in the driest habitat. The association of *P. mawsonii* high ratios with the mid-dark grey clays, discussed above, fits this series (see Table 1 in Martin 1984a). The distribution of the high *N. flemingii* ratios fits the hypothesis that it required good drainage (Martin 1984a). In the Murrumbidgee area the *N. flemingii* high ratios are most common along the Murrumbidgee River. The two acmes approximates the stratigraphic position of falls in sea levels (see Fig. 2). Lowered sea levels would result in down cutting of the rivers and improved drainage. On the other hand, the Lachlan area with sandier sediments and a slightly shallower basement was better drained the whole time. (Other environmental factors change with a fall in sea level and their likely effect is discussed below.)

Most of the clays associated with the high Myrtaceae ratios are less carbonaceous (see Table 1 in Martin 1984a). The high Myrtaceae ratios are thought to be associated with a change to a drier climate (discussed further, below).

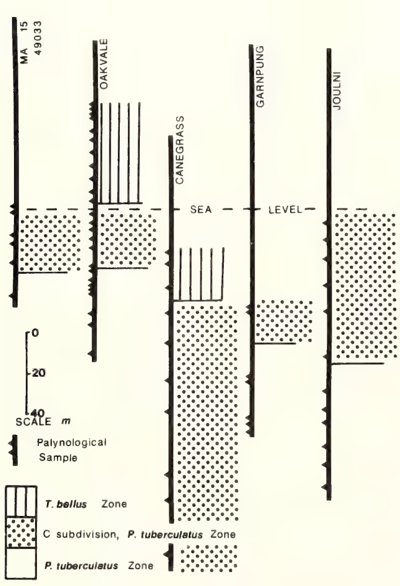

Figure 4. A section in the western part of the Basin (see Fig. 1). The lower boundary of the C subdivision of the *P. tuberculatus* Zone relies solely on high Myrtaceae ratios (see Fig. 6). The Canegrass bore is situated over the Tarrara Trough and the sedimentary sequence is much deeper here

THE PALAEOVEGETATION

Interpretations of the vegetation given here rely on general principles rather than comparison with some analagous, living vegetation. The changes wrought by the intervening millions of years have been so great that a good fit with some extant vegetation does not exist. For example, the three pollen types of *Nothofagus* are found together in the early-mid Tertiary assemblages but the living plants do not grow together anywhere in the world today. The *brassi* type alone is found in New Guinea, New Caledonia and the New Hebrides whereas the other two types may grow either alone or together in southeastern Australia, Tasmania, New Zealand and South America. Secondly, living representatives of the gymnosperm pollen types *Lygistepollenites florinii* and *Dacrycarpites australiensis* (see Table 1) grow today in New Guinea, New Caledonia and New Zealand. In the early-mid Tertiary, these pollen types are found with *P. mawsonii* whose living representative is restricted to Tasmania. There are other examples, but if a general, rather than specific approach is adopted, floristically, comparable vegetation is found today on the Australian mainland, Tasmania, New Zealand, New Caledonia and New Guinea.

The pollen assemblages consist largely of tree pollen. Pollen counts of low growing plants, such as Gramineae, Restionaceae and Cyperaceae is usually very low. Most of the sequence pre-dates the first appearance of Compositae. Two samples near the top of Oakvale sequence are an exception with up to 27% Cyperaceae and 10% Gramineae (Truswell *et al.* 1985), but even these assemblages contain a large proportion of tree pollen. Thus the pollen assemblages are interpreted as closed forest, i.e. rainforest, where the continuous tree canopy reduces light intensity on the forest floor to levels insufficient to support a good cover of low growing plants.

Experiments with surface samples, in which the pollen assemblage on the ground is compared with the composition of the vegetation, show that changes in the abundance of pollen may be interpreted as changes in abundance of the parent plant in the vegetation. For high pollen producing plants, the deposition of pollen decreases exponentially from the source. High 'local' values are found close to the source and low 'regional' values are found within a relatively short distance of 400 m to 500 m from the source (Birks and Birks 1980). Low values may be interpreted as pollen transported in from a distance or a low occurrence of the parent plant in the vegetation. Thus the high ratios indicate an abundance of the parent taxon in the vegetation.

Figures 7-9 show that the high ratios are concentrated in certain areas of the basin. Not all of the bores in an area show the appropriate high ratio, even though only those bores which were sampled over the stratigraphic intervals at which the high ratios may be expected are included. Sands and gravels may be encountered anywhere in the sequence and it may be that these lithologies, which are generally unproductive of pollen, occupy the level at which the high ratios should occur. In some bores, the ratios fall just below the empirical value used to define the high ratios, but these bores show a peak at the appropriate stratigraphic level. These peaks are not mapped. The concentration of a high ratio in an area is thought to indicate regional variation in the palaeovegetation over the basin. In the early Oligocene, *P. mawsonii* was abundant in the southeast (see Fig. 7). *N. flemingii* may be abundant throughout the Oligocene in the northeast, but it is restricted to two relatively short time intervals along the Murrumbidgee River and the region immediately to the south. *N. flemingii* is not abundant in the eastern edge and most southeasterly part of the basin (see Fig. 8). Myrtaceae subsequently replaces *Nothofagus* as the most abundant pollen type over the whole of the basin (see Fig. 9), but this event occurs earlier, late Oligocene in the northwest and later, early Miocene in the southeast. Myrtaceae is more abundant in the northwest than in the southwest and eastern edge, throughout the whole of the sequence (see Figs. 5 and 6). Araucarians become abundant in the early Miocene in the northwest (see Fig. 7).

Vegetation in which *brassii* type species of *Nothofagus* are dominant is found in the mid montane zone in New Guinea (Johns 1982). *Nothofagus* is generally associated with a high precipitation of 1500-1800 mm and considerable cloudiness which reduces light intensity and maintains a high humidity. It is generally absent from areas which suffer a regular and sustained water deficit (Ash 1982). In New South Wales, one species of the *menziesii* type of *Nothofagus* is present and may be dominant where precipitation exceeds 1800 mm. It is usually restricted to sites that are commonly fog bound. In general, *Nothofagus* requires a high precipitation and maintenance of relatively high humidities throughout the year (Baur 1957).

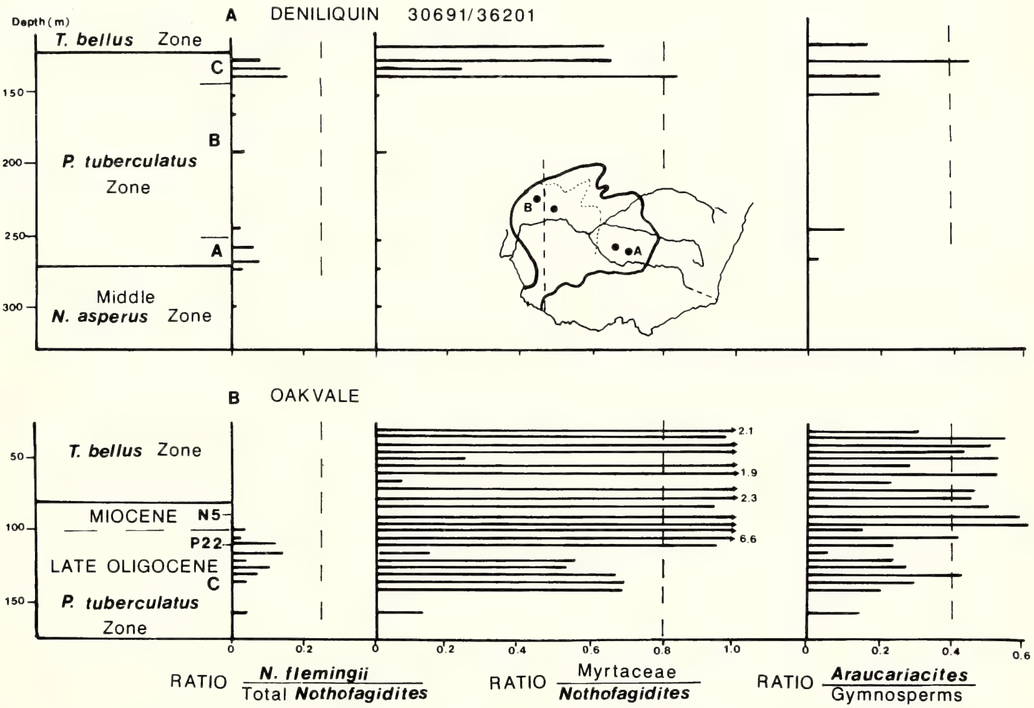

Figure 5. A comparison of the ratios of *N. flemingii*, Myrtaceae and *Araucariacites* for Deniliquin and Oakvale. The foraminiferal zones N5 and P22 are shown for Oakvale (from Truswell *et al.* 1985). Where the Myrtaceae/*Nothofagus* ratios are greater than 1, only some of the values are given.

In attempting to find some generally comparable vegetation with abundant Myrtaceae, the heterogeneous nature of this group which does not permit specific identification, must be borne in mind. The family Myrtaceae includes *Eucalyptus* and many other well known genera of open (sclerophyll) forests and woodlands. It also includes many genera and/or species confined to rainforests (see, for example, Francis 1981; Hyland 1983; Floyd 1973). In the bores of this study, the *Eucalyptus* type is present, but the majority of the pollen is unlike *Eucalyptus*. In the Oakvale bore, most pollen types can probably be related to rainforest taxa such as *Syzygium*, *Acmena* and *Tristania*, with minor amounts referable to *Decaspermum*, *Austromyrtus* and *Rhodamina* (Truswell *et al.* 1985).

In New South Wales, myrtaceous taxa are common in rainforests. A precipitation of 1500 mm is required for widespread development of rainforests (Baur 1957). Myrtaceae is often prominent in New Guinea forests, and becomes increasingly common in the upper part of the lower montane zone (of Paijmans 1976), but the precipitation requirements, relative to those of *Nothofagus* are not clear.

The family Araucariaceae has two genera, *Araucaria* and *Agathis*. The pollen morphology of both genera is very similar, but there is a size difference, *Araucaria* larger and *Agathis* smaller, although the size ranges of the two genera overlap (Cookson and Duigan 1951). From the size of the fossil pollen, most of the *Araucariacites* in the assemblages with high ratios probably represents *Araucaria*.

Araucarians are found in small, restricted areas along the northern half of the east coast region of Australia where the precipitation ranges from 820 mm to 1500 mm (Hall *et al.* 1970). In New Guinea, araucarians are found in wetter climates of the montane regions, with a precipitation of up to 4000 mm (Enright 1982). When compared with other types of Australian rainforest, araucarian forests have one of the lowest rainfall requirements (Webb 1978), and may be found in areas which experience a seasonal dry period (Webb and Tracey 1981).

The interpretation concentrates on a few selected taxa, selected primarily for stratigraphic purposes. These selected taxa provide valuable information about the palaeoecology. but not a balanced view of the palaeovegetation as a whole. The palynofloras as a whole are quite diverse with a number of pollen types found only in low frequences (up to 5%). The parent plants of these pollen types would have been low pollen producers, and hence little may be deduced about their abundance or rarity in the palaeovegetation. Collectively, they would have been significant and may even have accounted for a major part

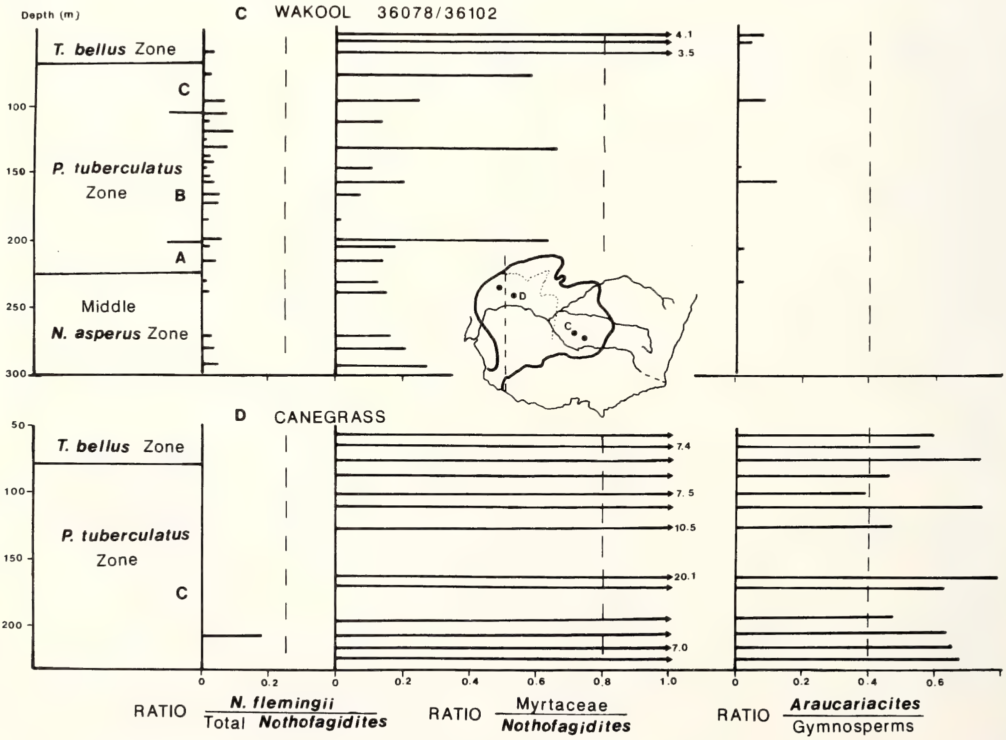

Figure 6. A comparison of the ratios of *N. flemingii*, Myrtaceae and *Araucariacites* for the Wakool and Canegrass bores. Where the Myrtaceae/*Nothofagus* ratios are greater than 1, only some of the values are given.

of the palaeovegetation. Their distribution over the basin has not been assessed, but it is likely that some, at least, may show regional differences.

PALAEOCLIMATE

The deduction of palaeoclimate from the climatic requirements of comparable present day vegetation can only be attempted at a very general level. The precipitation requirements of key taxa, discussed in the preceding section, are given for some New Guinea and Australian regions. It is difficult to compare montane regions in New Guinea, where topography and altitude assume an important role with the subdued relief of Australia and the almost flat Tertiary Murray Basin which is situated close to sea level. No great reliance can be placed upon absolute precipitation levels as such, but they do illustrate a clear trend, viz., *Nothofagus* in the wettest habitat, with the maintenance of high humidity, Myrtaceae probably intermediate and araucarians in the driest habitats, perhaps with a seasonal dry period.

The Oligocene assemblages with abundant *Nothofagus* indicate a wet climate with precipitation probably in excess of 1800 mm and the maintenance of high humidities the year round. The change to abundant Myrtaceae indicates a decrease in precipitation, probably to about 1500 mm. This change occurred in the late Oligocene in the northwest part of the basin and later, in the early Miocene in the southeast, and was not as well marked in the latter. When araucarians become abundant in the early Miocene in the northwest, there is a further decrease in precipitation, probably accompanied by a seasonal dry period. Truswell *et al.* (1985) consider that some seasonality was likely at Oakvale from the time of the change-over to high Myrtaceae. There was probably a precipitation gradient across the basin, drier in the northwest and wetter in the southeast, and this gradient parallels that seen today.

The above climatic interpretation is based on comparable living vegetation and is supported by evidence from the sediments. Lignites and dark grey carbonaceous clays, common in the Oligocene, indicate very wet conditions of deposition. Pale grey clays with a lower carbonaceous content become frequent towards the top of the non-marine sequence (Martin 1984a), possibly indicating that conditions of deposition were not as wet. The grey colour of the clays throughout the pollen bearing sequence probably indicates that there was no marked dry season, but this interpretation does not rule out a moderate seasonal moisture deficit for plant growth. The top of the pollen bearing sequence marks a further decline in precipitation to levels which were insufficient to support the permanently wet sites required for pollen preservation.

In this study, where precipitation is deduced from plant growth, 'precipitation' is more precisely 'effective moisture'. Effective moisture includes both rainfall and water transported in by the river systems. It also includes the influence of temperature through its effect on evaporation.

Temperature is also very important for plant growth but it is not considered here because the effect of moisture is thought to be the over-riding control. Temperatures in the Oligocene-early Miocene were probably somewhat higher than those of today. The mid-Miocene surface sea temperatures off south-eastern Australia were some 5° higher than those of today (Savin *et al.* 1975; Shackleton and Kennett 1975), hence land temperatures were probably somewhat higher also. Any variation in temperature during the Oligocene-early Miocene would probably have had more effect on the effective moisture than directly on plant growth. It has been stated previously that vegetation generally comparable to these Tertiary palynofloras, i.e. comparable for reasons of floristics and structure, may be found in Tasmania and New Zealand. However, the lower temperatures of these southern regions make them less suitable for climatic interpretations.

TABLE 1

Living representatives of the fossil pollen types (Martin 1978)

FOSSIL POLLEN TYPE	LIVING RELATIVE
Phyllocladidites mawsonii	*Lagarostrobus (= Dacrydium) franklinii*, huon pine.
Araucariacites australis	*Araucaria* and *Agathis*, e.g. hoop pine, kauri.
Lygistepollenites florinii	*Dacrydium*, Section B of the genus, e.g. rimu (of N.Z.).
Dacrycarpites australiensis	*Dacrycarpus (= Podocarpus*, Section *Dacrycarpus)*, e.g. kahikatea (N.Z.)
Nothofagidites	*Nothofagus*, all species of southern beech.
Nothofagidites flemingii	*Nothofagus*, *fusca* pollen type, similar to red beech (of N.Z.).
Myrtaceae (*Myrtaceidites spp*)	Most genera of Myrtaceae, e.g. eucalypts, lily-pily, bottle brush, tea tree, brush box, brush cherry, etc.

DISCUSSION

The major control of variation in the palaeovegetation and climate during most of the Oligocene may be linked with changing sea level. At times of lowered sea levels, the better drainage would lower the water table and produce drier growing conditions. As discussed previously, it is thought that this factor controlled the two *N. flemingii* acmes in the Murrumbidgee area. At times of high sea, the landscape would be the swampiest. *P. mawsonii* which requires the wettest habitat of the taxa was abundant in the early Oligocene, a time of high sea level (see Fig. 2). At times of subsequent high sea levels, e.g. in the late Oligocene, *P. mawsonii* did not return to its former abundance, probably because the climate had become too dry for it (see discussion below).

Changes in sea level also affect the climate directly. At times of high sea levels, shallow flooding of the continental shelves and low lying areas results in a warm, wet climate. At times of low sea levels, the low lying areas are drained and the continental shelves exposed, resulting in a climate that is cooler and drier. While these effects of changing sea levels caused some variation in the palaeovegetation, the precipitation remained sufficiently high to support abundant *Nothofagus* through most of the Oligocene.

Times of low sea level are usually regarded as times of erosion and Brown (1983) shows a mid Tertiary hiatus in sedimentation of the Murray Basin. This hiatus cannot be detected in the sediments of the totally non-marine area. There are soil horizons in the sequence but not one which may be identified

Figure 8. The distribution of high *N. flemingii* ratios. 1, high ratios in two discrete layers (see Fig. 2). 2, high ratios anywhere in Oligocene (see Fig. 3). 3, bores without high ratios.

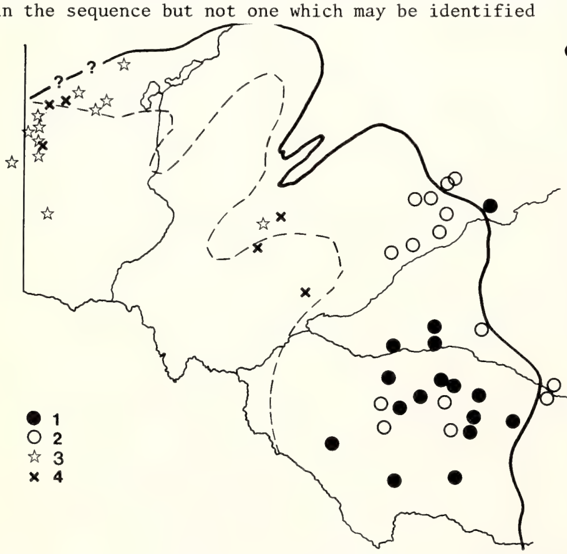

Figure 7. The distribution of high *P. mawsonii* and high *Araucariacites* ratios. 1, bore with and 2, bore without high *P. mawsonii* ratios in the Early Oligocene. 3, bore with and 4, bore without high *Araucariacites* ratios in the Early Miocene. Only the western and central regions have been mapped for high *Araucariacites* ratios For bores in eastern region which do not have high *Araucarites* ratios, see Fig. 1.

Figure 9. The distribution of high Myrtaceae ratios. 1, bore with high ratios in the *P. tuberculatus* Zone. 2, bore with high ratios starting in the *T. bellus* zone. 3, bore without high ratios.

with a mid Tertiary low sea level (R.M. Williams, pers. comm.). Given a high level of precipitation, an almost flat terrain of the forest cover, erosion would be minimal, other than that caused by down cutting of the river systems. In localised areas of impeded drainage, sedimentation would continue, though probably at a slower rate than at times of high sea level. The late Tertiary hiatus (Brown 1983), however, is well recognised in the non-marine area (R.M. Williams, pers. comm.). At this time, the climate was becoming drier and a good cover of vegetation would have been more difficult to maintain.

The late Oligocene changeover to abundant Myrtaceae is significant in that it is the first step towards a drier climate which leads, ultimately, to the present day climate. This changeover is unlikely to have been the result solely of fluctuations in sea level. Antarctica has had (and still has) a profound influence on world climate (Flohn 1978): particularly on Australia. Southern oceanic Tertiary palaeotemperatures (Savin *et al.* 1975; Shackleton and Kennett 1975) and the development of the circum-Antarctic circulation (Kennett 1977, 1978) are used to deduce the extent of Antarctic glaciation. This evidence is also used in models of Tertiary climatic change (see Kemp 1978; Flohn 1978; Bowler 1982). The changes in the palaeovegetation are examined in the light of these models. [There are other models of climatic change, based on different lines of evidence, e.g. Parrish and Curtis (1982), Parrish *et al.* (1982), but it is beyond the scope of this paper to assess these different view points.]

About the late Oligocene, the circum-Antarctic oceanic circulation was established. This factor probably reduced the efficiency of heat transfer from equator to pole, thus increasing the temperature gradient between these regions. The extent of ice cover on Antarctica is problematical, but all the evidence indicates high latitude cooling, which would have been reflected in increased baroclinity of the atmosphere and intensified circulation. A northwards movement of westerly winds would have influenced much of southern Australia (Kemp 1978; Flohn 1978). It is thought that these events initiated a drier climate which lead to the changeover to abundant Myrtaceae.

During the mid Miocene, a major climatic threshold was crossed when the Antarctic ice cap formed (Kennett 1977, 1978). The atmospheric circulation is thought to have intensified and general levels of precipitation fell, together with decreasing temperatures (Kemp 1978). It is likely that this event caused the cessation of pollen preservation in the Murray Basin. Decreased precipitation over the headwaters of the river systems, in the Eastern Highlands may have been more important than that over the basin itself. Such decreases over the headwaters would have had a significant impact on the water transported into the basin.

There are apparently two major steps of climatic change, but this does not rule out any change during the intervening periods. The failure of *P. mawsonii* to return to its former abundance at the time of the late Oligocene high sea level (see Fig. 2) indicates that there had been climatic change although a threshold which controlled the major aspects of the palaeovegetation had not been crossed.

Weathering subsequent to deposition may alternatively account for the lack of pollen preservation in the upper 80 m to 100 m of sediment, but it is unlikely to be the sole cause. Bore 25394 at Narrandera is on the basin edge, close to the Murrumbidgee River. Here, grey clays are found from the base of the sequence (late Eocene) up to 32 m and they have yielded Pliocene assemblages (see Martin 1984b). Thus subsequent to the mid Miocene, the river was able to maintain this small area permanently wet, but not the vast area further downstream, as it did prior to the mid Miocene.

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School of Botany,
University of New South Wales,
Box 1, P.O.,
Kensington,
Australia 2033.

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Cave and Landscape Chronology at Timor Caves, New South Wales

R. A. L. Osborne

ABSTRACT

Caves have existed at Timor Caves since before the Late Cretaceous and much of the present landscape of the area had its origins prior to the extrusion of Eocene basalts and tuffs.

Main Cave (TR 1) at Timor Caves was formed by the excavation of both bedrock and palaeokarst sediment below the water table under nothephreatic conditions. The palaeokarst sediment represents an extensive former cave filled with speleothem deposited under vadose conditions.

A tabular basalt body exposed in Main Cave is interpreted here as a flow filling a cave at a time intermediate between deposition of the palaeokarst and excavation of the present cave. The basalt has been dated at 73.5 Ma. This provides a maximum age for the present cave void and a minimum age for the palaeokarst.

Two similar basalt bodies exposed in Belfry Cave (TR 2) are also interpreted as flows.

The excavation of Main Cave corresponds with a significant and sustained rise in the local water table most likely associated with filling of the adjacent valley of Isaacs Creek by Eocene (circa 53 Ma) basalt of the Liverpool Range Beds.

Geomorphic evidence suggests that Isaacs Creek adjacent to the caves had a well developed valley before the Eocene basalt was extruded. Since the flows in Belfry Cave did not interact with groundwater, topographic relief during the Late Cretaceous was much the same as at present.

INTRODUCTION

Timor Caves have developed in the Timor Limestone Member of the Middle Devonian Yarrimie Formation (Crook, 1961a) which crops out along the valley of the Isis River, 25 km east of Murrurundi, in the Hunter Valley Region of New South Wales (Fig. 1).

The assignment of a Cretaceous age to Main Cave at Timor by Connolly and Francis (1979) represented a major change in thought about the likely age of caves and karst landscapes in eastern Australia, as previous workers had considred caves to be largely Pleistocene features.

Connolly has more recently retracted from this position (Connolly, 1983), suggesting that the caves must be younger than the Eocene basalts of the Mount Royal Ranges that form the watershed for Isaacs Creek. In a brief comment Francis and Osborne (1983) questioned Connolly's new conclusions and argued that Main Cave had more ancient origins.

Timor Caves have particular potential for studies in speleochronology as they occur close to basalts and intersect tabular basalt bodies. This offers the possibility of dating surface landforms by their relationships with the basalts and also of assigning maximum ages for speleogenetic events through dating of the basalts exposed in the caves.

This paper presents a new view of cave and landscape chronology at Timor Caves based on observations in Main and Belfry Caves, dating of basalt in Main Cave, and consideration of the relationships between the surface landscapes and basalts of Late Cretaceous and Eocene age.

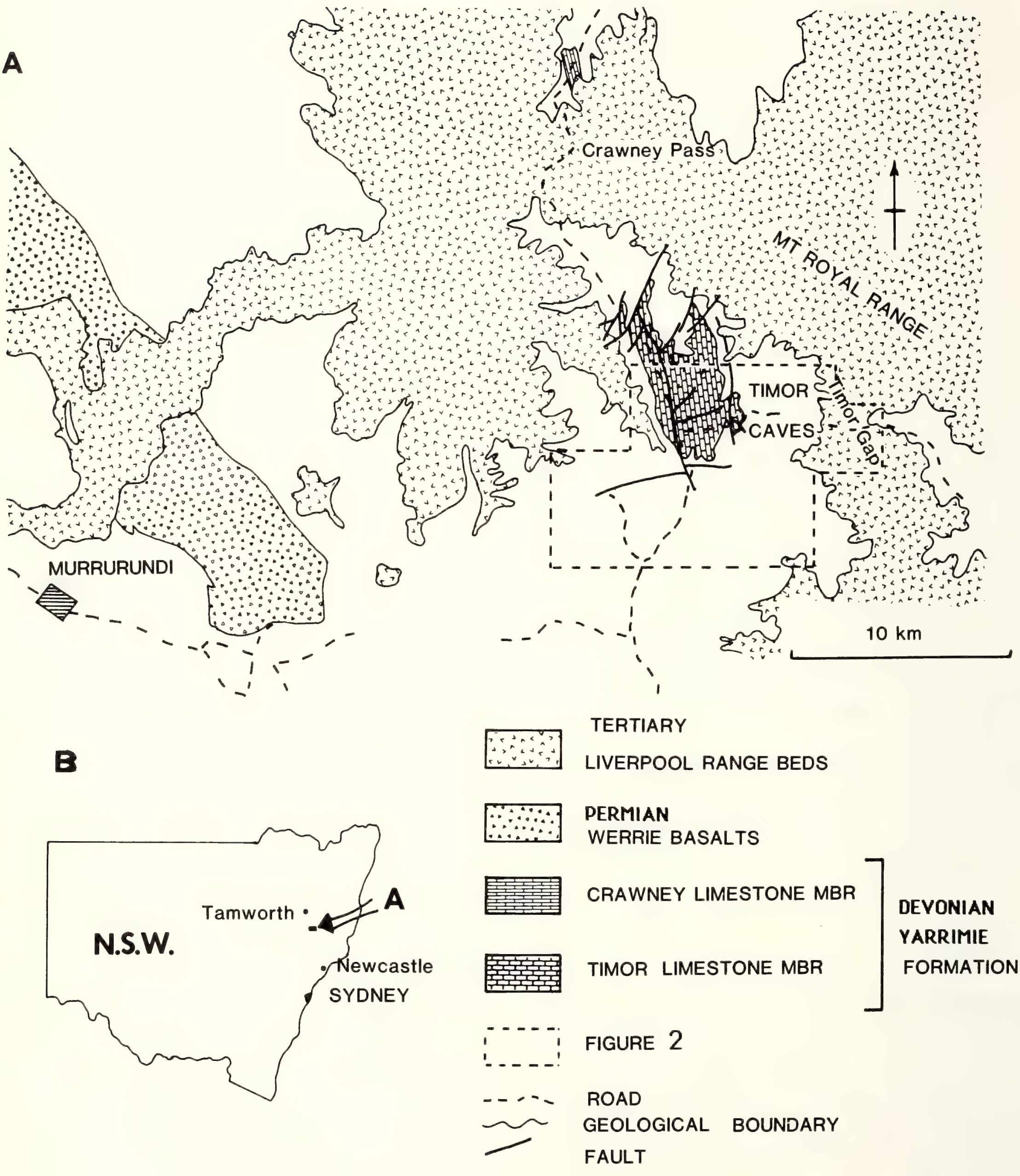


Figure 1. A: Geological setting. Abridged geology after Offenberg (1971).
B: Location

TIMOR CAVES

Forty five caves are recorded at Timor Caves of which thirty are described by James *et. al.* (1976) and a further ten by Warden *et. al.* (1981). Main Cave (TR 1), Belfry Cave (TR 2), and Helictite Cave (TR 4) were described in detail by Connolly and Francis (1979).

Main Cave

Main Cave lies close to the surface high in Caves Ridge (Fig. 3) with an entrance elevation of approximately 640 m A.S.L. It consists (Fig. 4) of a large room, the Ballroom, around which a number of smaller side chambers are developed. Wall and ceiling morphology is indicative of solution under sluggish phreatic (nothephreatic) conditions. The floor is largely composed of grey earth (entrance facies) and guano. Small descending passages at the sides of the Ballroom, developed between the sediment and the walls, suggest that this deposit is quite thick.

Belfry Cave

Belfry Cave is located much lower in Caves Ridge than Main Cave and has its entrance approximately 90 m below that of Main Cave. The cave (Fig. 5) is much more complex than Main Cave and consists of a series of structurally-controlled passages.

The cave can be divided into two quite distinct sections separated by a squeeze over flowstone (A in Fig. 5). The outer section, in which the basalts are exposed, has planed ceilings, channel grooves and asymmetrical wall scallops which Connolly and Francis (1979) recognised as indicating inflow, and at times outflow, through the present entrance, and suggest that this section of the cave was developed (or considerably modified) by shallow phreatic (epiphreatic) and possibly vadose solution. The floor of the outer section is composed of grey earth (entrance facies) similar to that in Main Cave.

The inner section of the cave is significantly structurally controlled and has many features; roof pendants, blades, and spongework which Connolly and Francis (1979) recognised as indicating nothephreatic solution. The atmosphere in the inner section is considerably more humid than that in the outer section and the floor is composed of phosphatic clay.

EVIDENCE FOR MULTIPLE KARSTIFICATION

The term multiple karstification (Osborne, 1984a) is used to describe karst processes affecting the same body of rock on a number of distinct occasions during its history. Multiple karstification results in complex interrelationships between modern karst forms and palaeokarsts of varying ages. Multiple karstification is indicated by the truncation and exposure of palaeokarst sediments in caves.

Connolly and Francis (1979) recognised that Main Cave contains "evidence of substantial former infill" and described how pool crystals and flowstone are exposed in the cave walls. When outlining evidence for multiple karstification in the Lachlan Fold Belt, Osborne (1984a) cited Main Cave at Isaacs Creek as a possible example of multiple karstification in the New England Fold Belt.

Main Cave is an extraordinary example of multiple karstification. Large parts of the cave are developed in flowstone, rather than bedrock. Like the bedrock walls, the walls composed of flowstone (Fig. 6) have been dissolved by nothephreatic solution with wall morphologies being continuous across the unconformity between bedrock and flowstone.

The palaeokarst sediments are composed mainly of flowstone and other speleothem material (both stalactites and stalagmites can be recognised in some exposures, Fig. 7) indicating that the former cave was subjected to vadose conditions for a considerable period after which it re-entered the phreatic zone and the present cave was excavated.

Multiple karstification, although probably on a smaller scale, is indicated by the development of phreatic roof pendants in cemented cave sediments in the inner section of Belfry Cave. These were described and discussed by Connolly and Francis (1979).

Another, apparently older, palaeokarst deposit is exposed in the inner reaches of Belfry Cave (locality "B", Fig. 5) where a body of chert with highly irregular relationships to bedding and the present cave walls is exposed. The chert U.S.G.D. 63251 shows some signs that it may represent a silicified basic igneous rock.

BASALT BODIES IN MAIN AND BELFRY CAVES

Connolly and Francis (1979) reported the presence of amygdaloidal basalt dykes with flow banding in Main and Belfry Caves. They considered that the basalt was too weathered for dating or petrographic study but that, if possible, dating the dykes would provide a maximum age for the caves. Connolly and Francis also proposed that the basalt need not be Tertiary in age but could be Mesozoic since Dulhunty (1976) had established Mesozoic ages for intrusions in the Hunter Valley formerly considered to be of Tertiary age. G. Francis (pers. comm.) suggested that the basalt in the caves might correlate with the Permian Werrie Basalts (see Fig. 2)

In contrast Connolly (1983) concluded that the dykes were the same age as the adjoining basalts of the Mt Royal Ranges thus making the caves less than 50 Ma old.

Francis and Osborne (1983) suggested that although the basalts in the caves are weathered it might still be possible to make petrographic studies of them.

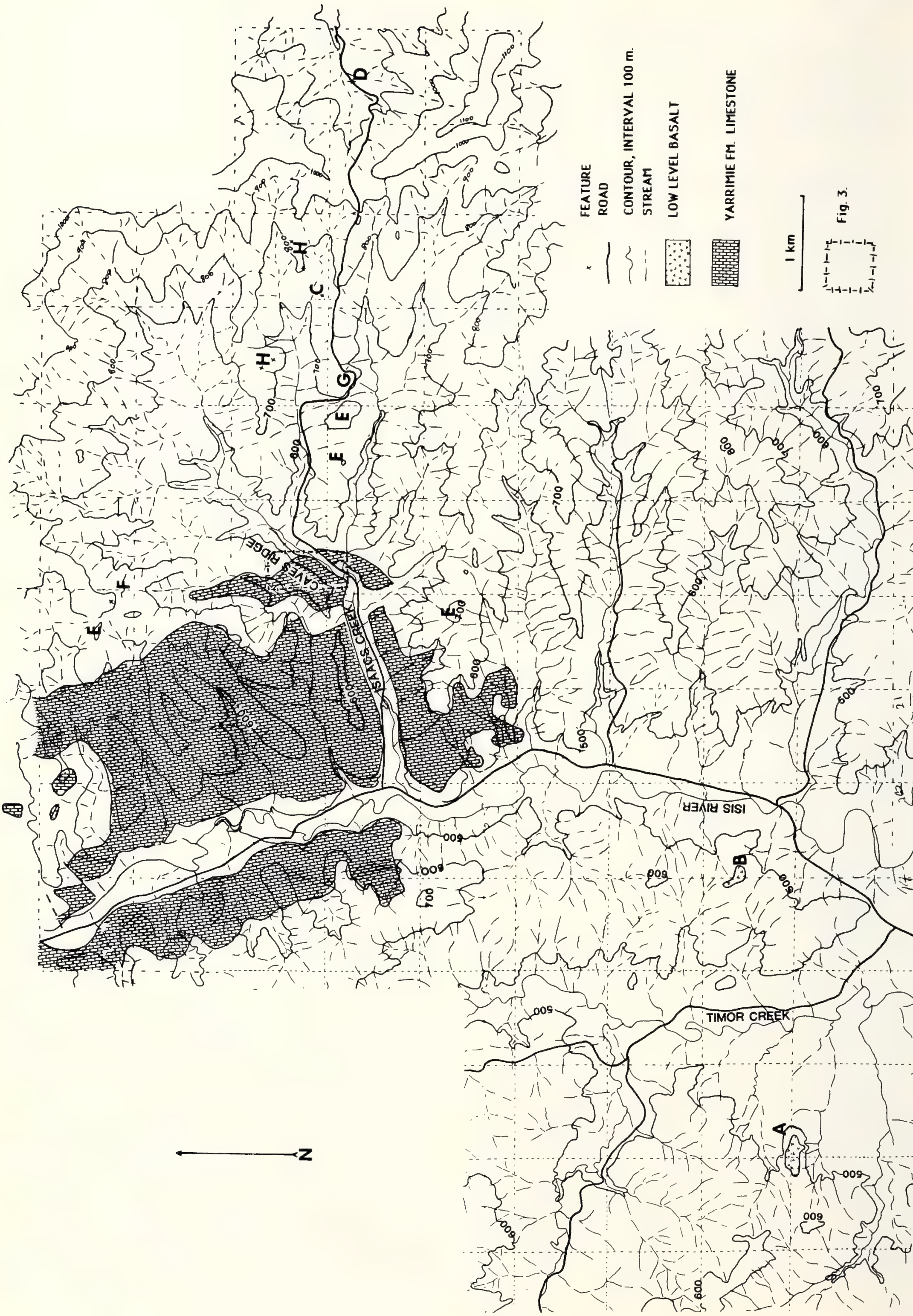

FEATURE
ROAD
CONTOUR, INTERVAL 100 m.
STREAM
LOW LEVEL BASALT
YARRIMIE FM. LIMESTONE
1 km
Fig. 3.
ISIS RIVER
TIMOR CREEK
CAVE RIDGE
N

Figure 2. Topography of the Timor Caves area. Contours after C.M.A. Isis River 9134-IV-N 1:25 000 Sheet.

A: Low level basalt (U.S.G.D. 62361) mapped by Robson (1982).
B: Low level basalt (U.S.G.D. 62360) mapped by Robson (1982).
C: Tuff (U.S.G.D. 62362) illustrated in Fig. 17.
D: Lowest exposure of basalt (830 m) on road east of Timor Gap.
E: Basalt-free hills with elevations over 700 m A.S.L.
F: Lowest unambiguously in-situ outcrop of basalt on Caves Ridge (circa 790 m).
G: Basalt knoll (U.S.G.D. 62357)
H: Basalt knolls in adition to "G" identified by Connolly and Francis (1979) as having associated magnetic anomolies.
I: Basalt knoll (U.S.G.D. 62356)

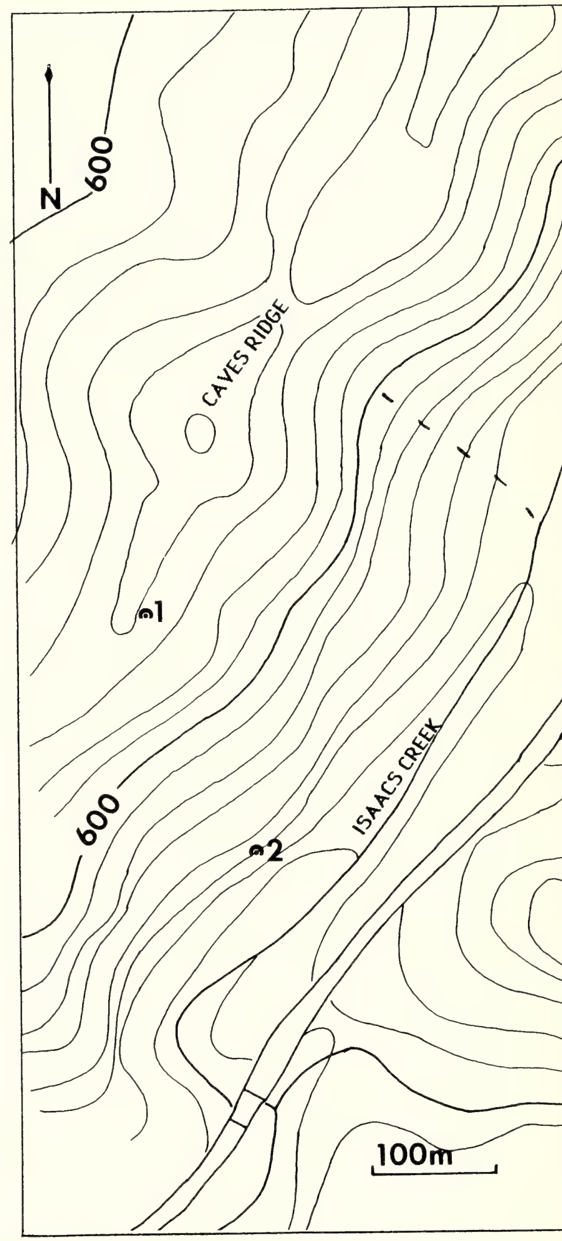


Figure 3. Location of Main and Belfry Caves. Contour base after C.M.A. Isis River 9134-IV-N 1:25 000 Sheet, cave entrance positions after James *et. al.* (1976).

Examination of basalts in Main and Belfry caves has shown that they are not as weathered as previous workers had implied and thin sections have been successfully produced from them. The field relationships are more equivocal than has been previously assumed; the decision as to whether the bodies are dykes or flows filling caves is far from simple. It is now possible to compare petrographically the basalts from the caves with the Tertiary basalts. Connolly (1983) has correlated these and with the low level flows in the Isis River Valley described by Robson (1982).

Field Relationships

Three basalt bodies (one in Main Cave and two in Belfry Cave) are truncated by development of the present caves and thus, as recognised by both Connolly and Francis (1979) and Connolly (1983), set a maximum age for the development of the present caves. This, however, does not rule out the possibility that they may be flows filling former caves rather than dykes.

The basalt in Main Cave is exposed in horizontal cross-section in the cave roof and in vertical cross-section in a niche in the south western corner of The Ballroom.

Where it is exposed in vertical cross-section the basalt does not extend to the cave floor, but rather, overlies a palaeokarst deposit. The basalt (Fig 8) is bounded on two sides by massive limestone walls which at the base of the exposure enclose palaeokarst deposits and some 2 m above the cave floor enclose the basalt. The lateral boundary between the basalt and the limestone is sinuous, as is the trace of the boundary in the cave roof.

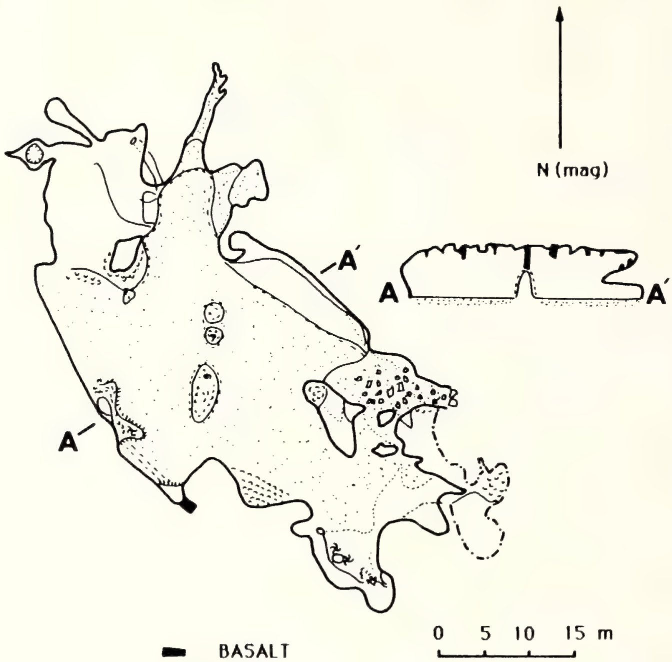


Figure 4. Main Cave, TR 1, map (plan view and section A-A′) after James *et. al.* (1976).

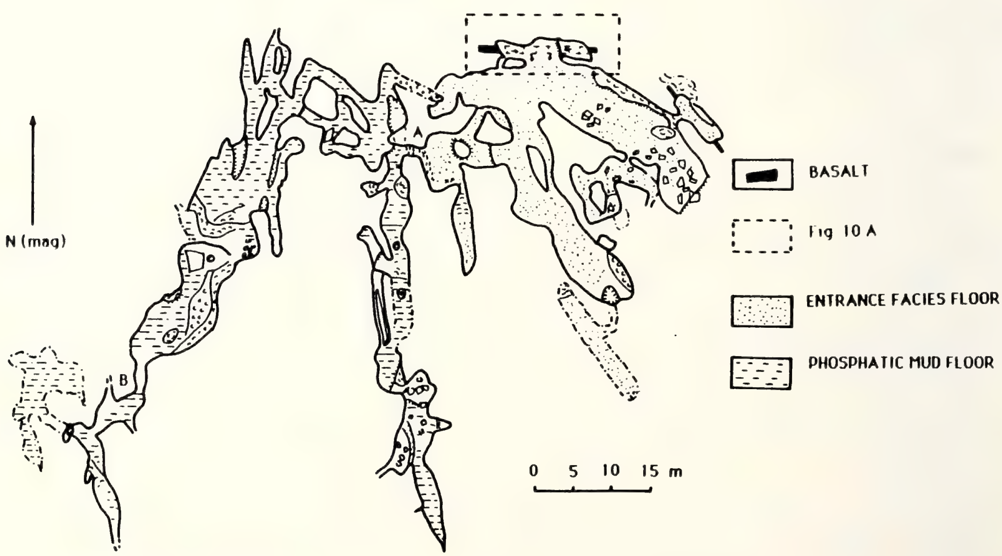


Figure 5. Belfry Cave, TR 2, map (plan view) after Connolly and Francis (1979).
A: Squeeze separating dry outer portion of cave from moist inner portion with phosphatic clay floor.
B: Location of chert body, U.S.G.D.62351.

Figure 6.

Figure 7.

Figure 6. Palaeokarst exposed in Main Cave. Note how phreatic wall features of present cave continue over the exposed boundaries between bedrock and vadose palaeokarst.

Figure 7. Palaeokarst exposed in Main Cave. In this exposure figured by Osborne (1984b) the unconformable boundaries between bedrock and palaeokarst speleothem and between palaeokarst speleothem and recent speleothem (marked by pen) are visible. Note the preservation of small stalactites within enclosing speleothem in centre of frame.

The observed relationships could have been produced in several ways the two most probable of which are illustrated in Fig. 9. The first, and preferred, history (Fig. 9, A) interprets the basalt as a flow, filling a narrow vadose shaft that had already been partially filled with palaeokarst sediment. With this interpretation the palaeokarst predates the basalt and the present cave is younger than both.

In the second history (Fig. 9, B) basalt intrudes the limestone as a dyke and is partially eroded to form a cave which is later filled by the palaeokarst sediment. The present cave then forms, exposing both the basalt and the palaeokarst. With this interpretation the palaeokarst is younger than the basalt.

The nature of the boundary between the basalt and the limestone supports the former, rather than the latter, interpretation. If the basalt were a dyke emplaced along a joint , as proposed by Connolly and Francis (1979) , it would be expected to have more planar lateral boundaries and very little linear sinuosity. The lateral boundaries between the basalt and the limestone, and the palaeokarst and the limestone, indicate that the cavity in which the basalt and the palaeokarst is enclosed has the form of a vadose cave passage with channel incuts (Fig. 8, A & B).

While the bodies in Belfry Cave are more dyke-like, the western body has parallel walls in the upper part of its exposure but bifurcates downwards (Fig. 10) with an inclined branch sloping towards the main passage of Belfry Cave suggesting that the basalt came from above as a flow entering a cavity, rather than as an intrusion from below. If this body is a flow then it entered a cave above the local water-table, as it shows no sign of interaction with standing water.

Petrography

The igneous bodies in both Main and Belfry Caves are composed of weathered vesicular basalt with flow banding.

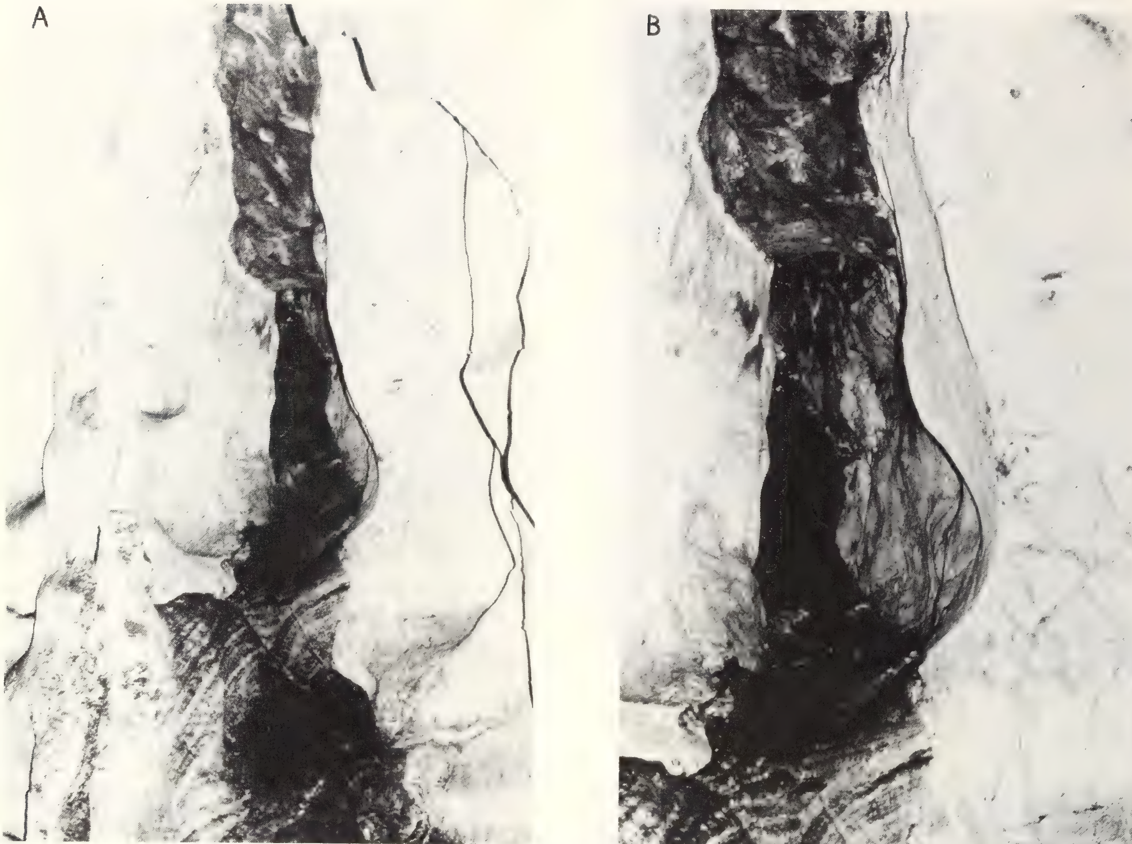


TABLE 1

Potassium-argon data on plagioclase separated from basalt in Timor Caves, 8 km Northeast of Timor, New South Wales.

Sample No.	K wt%	Radiogenic ^{40}Ar (10^{-11} mol/g)	$\frac{100 \text{ Rad. } ^{40}\text{Ar}}{\text{Total } ^{40}\text{Ar}}$	Calculated Age (Ma) ± 1 s.d.
84-451	0.291, 0.292	3.82	66.6	73.9 ± 0.8
U.S.G.D. 62359		3.78	73.8	73.2 ± 0.8

$\lambda_e + \lambda_e' = 0.581 \times 10^{-10}\ yr^{-1}$ $\lambda_\beta = 4.962 \times 10^{-10}\ yr^{-1}$

$^{40}K/K = 1.167 \times 10^{-4}$ mol/mol Locality: 31° 42.2' S, 151° 7.2' E.

Figure 8. Basalt body exposed in Main Cave.

A: View of basalt body showing basalt at top and vadose palaeokarst at base, both enclosed by limestone. Note shape of boundary between basalt and limestone.

B: Closer view of basalt/limestone boundary than in "A". Note how vadose channel incut in base of passage is reflected in basalt/limestone boundary and how meandering morphology, typical of vadose cave passages, is shown by basalt/limestone contact in roof.

C: Close up view of basalt at its boundary with palaeokarst, marked by pick, again note shape of basalt-limestone boundary.

Sample U.S.G.D. 62359 from Main Cave (Fig. 12) contains zoned laths of plagioclase (up to 1 mm long) and vesicles filled with banded chalcedony and secondary carbonate. The plagioclase laths have remained relatively intact despite the considerable alteration of much of the rest of the basalt.

A less weathered sample from Belfry Cave, U.S.G.D. 62354, (Fig. 13) again contains plagioclase laths with olivines weathered to clay and carbonate-filled vesicles. A flow texture and carbonate-filled linear structures parallel with the walls of the body (? cooling joints) are developed.

The basalt in the caves is petrographically dissimilar to samples from the ridges to the east, which are titanaugite-rich microgabbros (Fig. 14), but is similar to the plagioclase-rich low-level basalt (B in Fig. 2) mapped by Robson (1982) (Fig. 15). Robson's other low level basalt (A in Fig 2) is in outcrop and petrography more like those to the east of Timor Caves and contains large pyroxene xenocrysts.

Radiometric Dating

The relatively unalterered nature of the plagioclase laths in the basalt from Main Cave (U.S.G.D. 62359) suggested that it might be possible to obtain a mineral date on an extract of the laths. To this end a concentrate of non-magnetic fines consisting largely of plagioclase from the basalt was submitted to the Research School of Earth Sciences, Australian National University for K-Ar dating. The concentrate was purified and the dating carried out with measurements being made in duplicate (Table 1). A date of 73.5 +- 0.8 Ma (Late Cretaceous) was obtained.

A Late Cretaceous date for the basalt in Main Cave may seem anomalous in view of the proximity of extensive Tertiary basalts. There is, however, some evidence for vulcanism of similar age near by.

McDougall and Wilkinson (1967) obtained a date of 58.6 Ma for the Square Top intrustion near Nundle, 25 km north of Timor Caves. They considered this to be a reliable minimum age.

Hollis and Sutherland (1985) have obtained fission track dates of 60.4 Ma for zircons from Duncans Creek east of Nundle and note that this closely matches a recalculated minimum age of 60.1 Ma for the Square Top intrusion. F.L. Sutherland (pers. comm) has found zircons of similar age (60 Ma) in the Gloucester area, 96

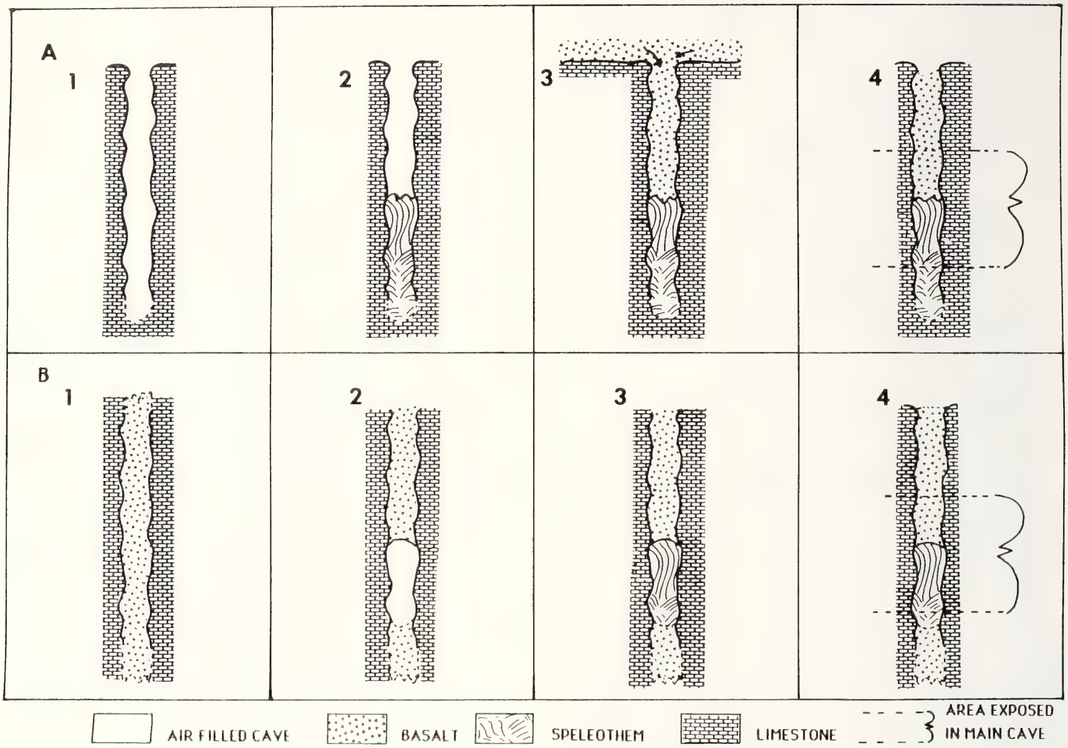


Figure 9. Possible modes of emplacement of basalt body in Main Cave.

A: 1, Formation of narrow cave with connection to surface.
2, Deposition of speleothem in cave.
3, Basalt enters cave from surface flow overlying speleothem.
4, Section between dotted lines is exposed by development of present Main Cave.

B: 1, Basalt dyke intrudes limestone
2, Cave develops by erosion of dyke
3, Cave is filled with speleothem
4, Section between dotted lines is exposed by development of present Main Cave

km east of Timor Caves, suggesting that igneous activity older than the Eocene basalts may have occurred over the same broad area.

Dating of the Liverpool Range Beds as Eocene is based on samples that may represent only the youngest flows in the sequence. Mt Wombramurra, from which the samples dated at 53 Ma by Wellman and Mc Dougall (1974) were obtained, has an elevation of 1417 m, over 600 m above the base of the basalt near Timor Caves. Although, as evidence discussed below suggests, this difference in elevation may not represent the true thickness of the Liverpool Range Beds due to extrusion over an irregular surface (Crook, 1961b considered that their thickness exceeded 300 m). Despite the thickness of the Liverpool Range Beds it seems unlikely that they would range in age over 20 Ma.

The palaeotopography of the Timor Caves area proposed below would require the Late Cretaceous basalts to have flowed from the east or north-east, i.e. from the same direction as the Eocene basalts. It is thus possible that the Late Cretaceous vulcanism represents an earlier phase of activity (evidence for which is mostly now buried) from similar sources to those producing the Liverpool Range Beds.

BASALT AND LANDSCAPE DEVELOPMENT

The Isis River and its upstream tributaries, like Isaacs Creek, rise among the basalt ridges and plateaux of the Mount Royal and Liverpool Ranges.

Crook (1961b) defined the Liverpool Range Beds as a sequence of sub-basaltic sediments and boles together with basalts. These extend from Mt Snowden in the north-west to south and east of Crawney Pass, with representative sections at Snowden Creek and Mt Yellow Rock (19 km north of Isaacs Creek).

Wellman and McDougall (1974) include these basalts as part of their Barrington Volcano and obtained a K/Ar age of 53 Ma for flows at Mt Wombramurra, 7 km north of Isaacs Creek.

Robson (1982) correlated basalt flows in the Yarrabin district with the Liverpool Range Beds.

The Liverpool Range Beds (Crook, 1961b), despite their extension over the Liverpool Range south of Murrurundi Gap by Pogson (1972), are not to be confused with the Liverpool Range Volcano of Wellman and McDougall (1974) or the basalts of the Liverpool Range described by Schön (1982). These form the basalt plateaux and ridges of the Liverpool Range west of Murrurundi and are distinct in age, origin, and composition from the Liverpool Range Beds.

The Liverpool Range Beds unconformably overlie the Devonian Tamworth Group, their basal unconformity being a significant indicator of the early Tertiary landscape. D.F. Branagan (pers. comm.) believes that the sub-basaltic sediments ascribed to the Liverpool Range Beds are significantly older than the basalt.

The Sub-Basaltic Surface

The nature of the sub-basaltic surface is crucial to any discussion of cave and landscape chronology in the Isaacs Creek area. If the sub-basaltic surface is, or was, level and has been disrupted by significant post-basaltic faulting, then caves and landscapes at Isaacs Creek must have originated from erosive events in post-basaltic times. If, however, the sub-basaltic surface had significant relief then the landscapes and older caves could predate the basalt, being covered and later exhumed.

Benson (1913) concluded that the prebasaltic surface was mature in character and trenched by immature valleys. He believed that this supported the idea of Andrews (1911) that peneplanation, uplift, and partial dissection had occurred prior to the eruption of the basalts.

Osborne *et.al.* (1948) recognised a sub-basaltic surface with an average elevation of 2,700-2,800 feet (823-854 m A.S.L.) with pulsatory uplift of the basalt-covered surface resulting in the development of valley-in-valley topography. They also noted "remarkable adjustment" between geological structure and detailed physiographic development of the area.

Crook (1961b) noted the presence of an inlier of Palaeozoic rocks within the Liverpool Range Beds on Ryans Oakey Creek suggesting an uneven prebasaltic surface. He found the Liverpool Range Beds to be tectonically little disturbed and no evidence for post-basaltic faulting.

Galloway (1967) constructed a model of sub-basaltic surfaces in the Hunter Valley in which he assumed that small basalt occurrences are the remnants of a former continuous sheet, that considerable post-basaltic tectonism had produced a sub-basaltic surface which was distinct from the prebasaltic land surface, and that post-basaltic drainage evolved *ab-initio* on a relatively featureless basalt surface.

For the Isaacs Creek area Galloway proposed that the sub-basaltic surface had an elevation of between 2,500 and 2,750 feet (762-838 m A.S.L.).

Connolly and Francis (1979) based their arguments for the antiquity of caves and landscapes on a model of the sub-basaltic surface similar to that of Benson (1913) and Crook (1961b). They proposed a sub-basaltic surface qualitatively like the present surface, with prebasaltic Isaacs Creek flowing in much the same path as the present Isaacs Creek and Main Cave (entrance elevation about 640 m A.S.L.) already well above creek level in prebasaltic Caves Ridge. They further proposed that the basalt filled Isaacs Creek valley and post-basaltic erosion had removed most of the basalt and only slightly deepened the valley into bedrock.

Connolly (1983) rejected this model and returned to the ideas of Galloway (1967) by having Caves Ridge

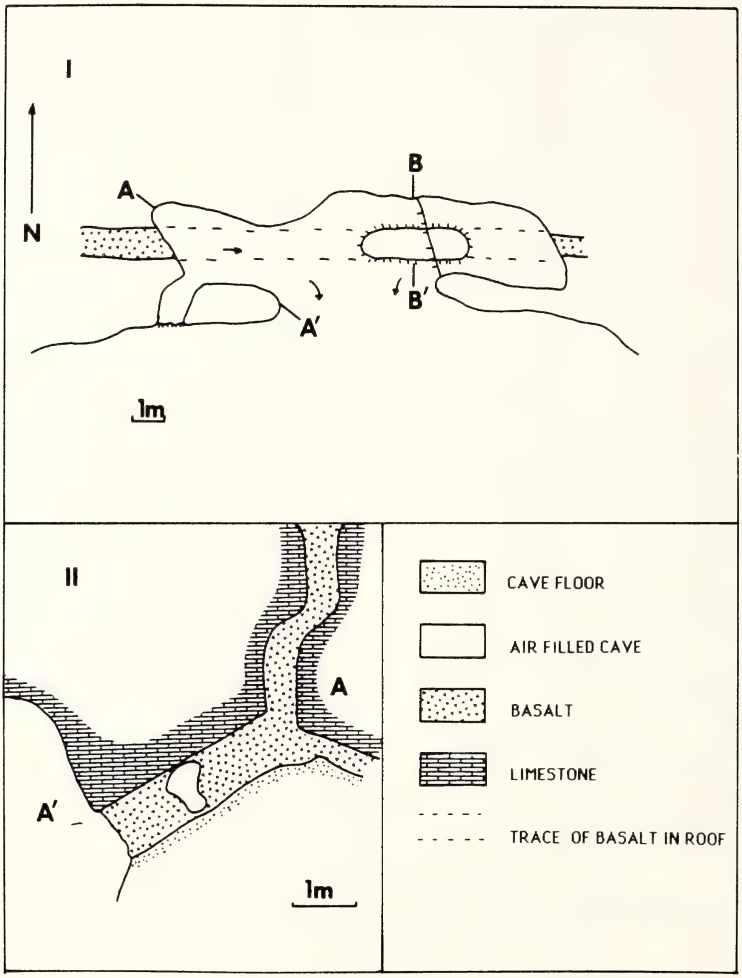


Figure 10. Western basalt body in Belfry Cave
I: Map (plan view) showing basalt body and location of sectional views.
II: Sketch of basalt body viewed in section A-A' (vertical section) showing bifurcation.

Figure 11. Photograph of vertical exposure of western basalt body in Belfry Cave. View corresponds to section line B-B' in Figure. 10, A.

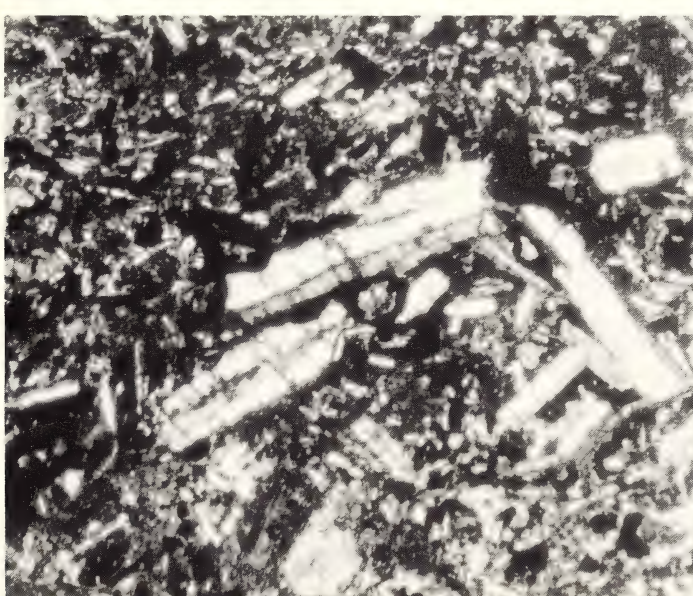


Figure 12. Basalt from Main Cave, U.S.G.D. 62359. crossed nicols, approx. 75 x. Note plagioclase laths and chalcedony-filled vesicle in lower middle of photograph.

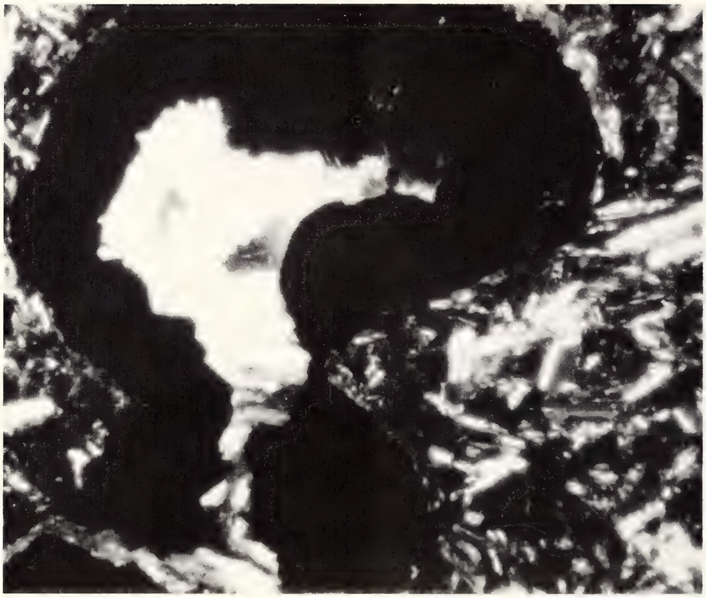


Figure13. Basalt from Belfry Cave, U.S.G.D. 62354. crossed nicols, approx. 75 x showing calcite and clay filling vesicle.

Figure. 14 Basalt from knoll at "G" in Figure. 2, U.S.G.D. 62357. plane polarized light, approx. 75 x. Note presence of titanaugite and fresh nature of rock.

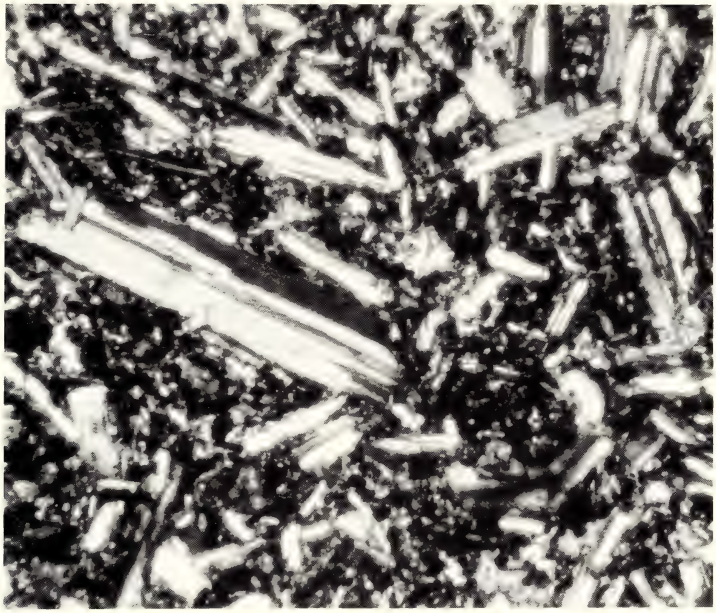


Figure 15. Basalt from low level flow "B" in Figure 2, U.S.G.D. 62360. crossed nicols, approx. 75 x. Note similarity of texture and composition to U.S.G.D. 62359 (Fig. 12) and U.S.G.D. 62354 (Fig. 13).

Figure16. Basalt knoll on south side of outcrop at "A" Fig. 2. Note conical shape and angular nature of basalt blocks at top of cone.

Figure 17. Tuff underlying basalt exposed in gully within the valley of upper Isaacs Creek (C in Fig 2). Further down hill tuff unconformably overlies the Yarrimie Formation.

Figure 18. View of Isaacs Creek valley from Caves Ridge looking east. Cuesta topography on southern valley side is developed on siliceous mudstones of the Yarrimie Formation.

"not a prominent feature in the landscape" in prebasaltic times. Connolly (1983) considered that Connolly and Francis' mapping of basalt at low levels upstream in Isaacs Creek was in error due to mass movement of weathered basalt.

The nature of the sub-basaltic (or prebasaltic) surface, like the age of the caves, is thus controversial. Considerable evidence has been canvassed by Connolly and Francis (1979) indicating that the sub-basaltic surface was irregular.

Connolly and Francis also argued that the basalt flows in the Isaacs Creek area were localised, identifying five basalt knolls ("G" and "H" in Fig. 2) associated with strong magnetic anomolies that they considered to be plugs, sources for localised flows. Connolly (1983) rejected this interpretation concluding instead that the knolls were "accumulations of corestones exposed by the removal of finer material". In addition to the five knolls identified by Connolly and Francis, a knoll is found at "I" in Fig 2 and one of Robson's low level basalts ("A" in Fig. 2) also has a knoll associated with it.

The basalt knolls do not consist of corestones, but rather of angular, relatively unweathered basaltic fragments in a cone-shaped outcrop (Fig. 16). This, along with the relatively coarse grainsize and presence of xenocrysts indicates that the knolls are most likely plugs as originally proposed.

An irregular sub-basaltic surface is supported by more recent observations.

Robson (1982) mapping the geology of the Yarrabin district (an area extending to within 3 km south west of Timor Caves) mapped basalt flows with bases as low as 510 m A.S.L. (Fig. 2). The closest of these flows to the caves ("B", Fig. 2) rests on the interfluve between the Isis River and Timor Creek and has a basal elevation of 560 m A.S.L. , 110 m above the adjoining bed of the Isis River.

To the east of the caves where Connolly and Francis (1979) mapped the base of the basalt down to 660 m A.S.L. field investigation has found unambiguous basalt outcrops down to 730 m while adjoining higher hills are free from basalt. Basalt-free hills in the caves area with elevations over 700 m A.S.L. are shown in Fig. 2.

East of Timor Gap basalt extends down to an elevation of 830 m (D in Fig. 2) where it is well within the valley sides.

In the area mapped by Connolly and Francis the basalt is underlain by tuff forming the base of the Liverpool Range Beds. Slumps are common here with mass movement being due to slippage between the Lower Palaeozoic basement and the basalt when the tuff becomes mobilized. In places well within the valleys, however, it is clear that the tuff and the overlying basalt are in-situ (Fig. 17) and overlie sloping valley walls formed of Lower Palaeozoic, rocks indicating that valley development predated the basalt.

Taken together these observations indicate that the relief on the base of the basalt within 5 km of the caves must be at least 150 m (a similar figure to that found by Robson (1982) in the Yarrabin district) and that significant valley development took place prior to extrusion of the basalt.

Interpretation of the basalt body in Belfry Cave as a flow indicates that significant relief existed at Timor Caves at an even earlier time. The basalt shows no sign of interaction with standing water during its emplacement, thus it must have entered an air-filled cave. For this to occur the karst water table must have had an elevation lower than 550 m , i.e. less than 30 m above the present creek bed, in the Late Cretaceous making the landscape then, and the elevation of Caves Ridge above Isaacs Creek, much the same as we see today .

Origin of the Present Landscape

In addition to the evidence for an irregular prebasaltic surface the character of the present landscape is difficult to reconcile with development *ab initio* on a relatively featureless basaltic surface.

The Isis River, the main drainage feature of the area, follows the strike of Lower Palaeozoic rocks while its tributaries such as Farm Gully, Seckolds Gully, and probably the lower tract of Isaacs Creek are developed along cross faults. If the present topography were largely the result of erosion of a relatively flat basalt-covered landscape it is likely that the present drainage would reflect the palaeogeography of the basalt surface, rather than the structure of the Palaeozoic basement.

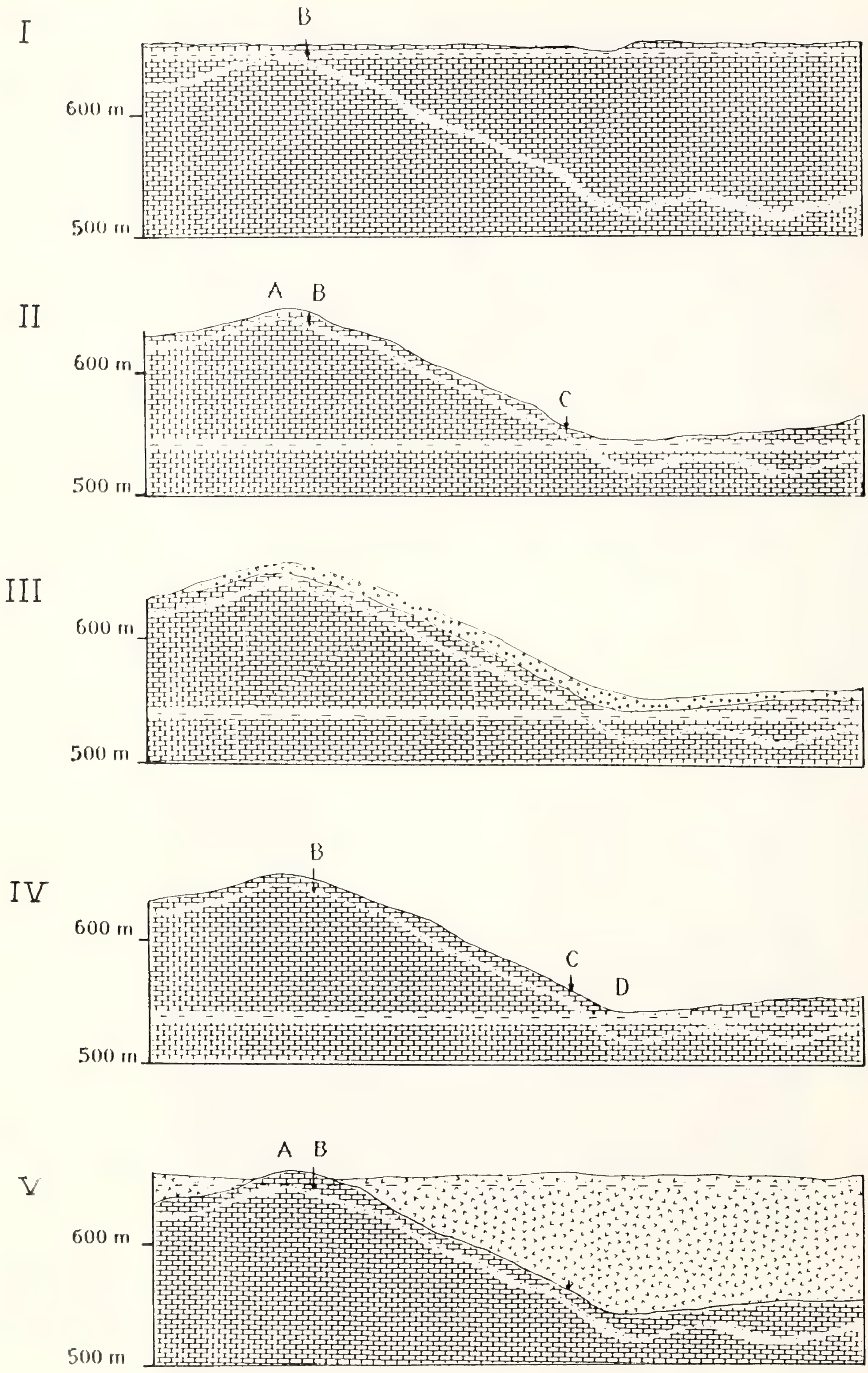
The development of drainage controlled by basalt palaeogeography has been shown to occur in the Barrington Tops area by Pain (1983). This has resulted in a radial drainage pattern, quite unlike the trellis pattern found in the Isaacs Creek area.

Another feature which demonstrates the significance of bedrock rather than basalt control over the landscape is the development of cuestas, dip slope hills, which are a prominent feature of the Isaacs Creek landscape (Fig. 18).

The drainage pattern and the cuestas further support the idea of the Isaacs Creek area having inherited much of its relief from prebasaltic times.

HISTORY OF SPELEOGENESIS

Cave development in the area now occupied by Main Cave preceded extrusion of Late Cretaceous basalt (Fig. 19, I). The development of extensive vadose palaeokarst indicates that prior to latest Cretaceous times the area now occupied by Main Cave was well above the water table (Fig. 19, II).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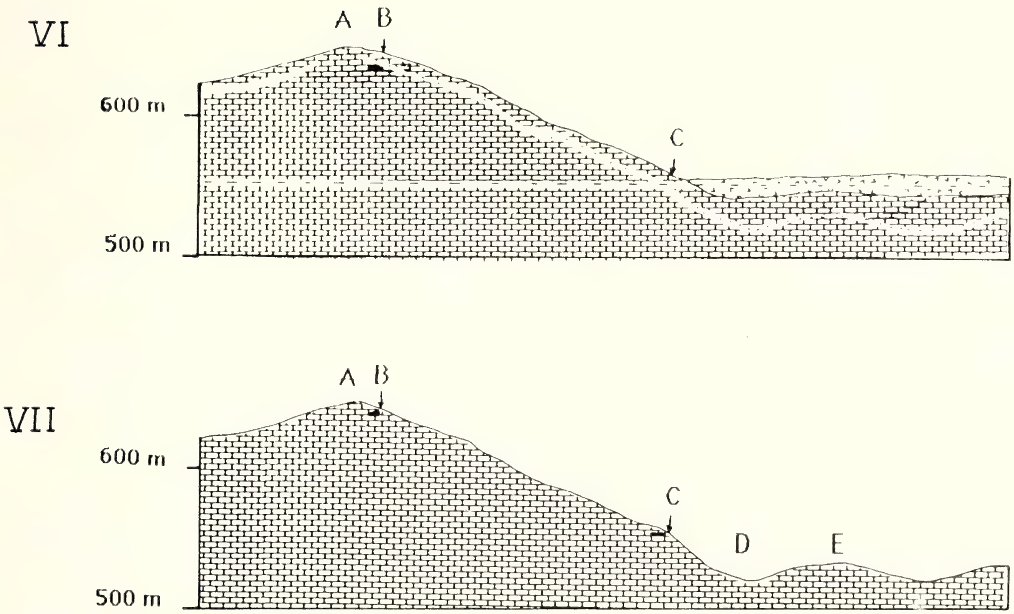

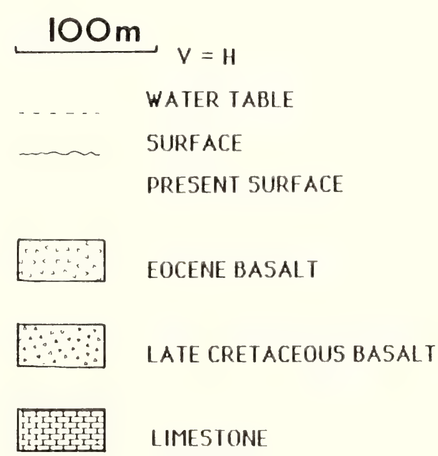

Figure. 19 Cartoon showing cave and landscape development at Timor Caves, based on cross-section through Main and Belfry Caves (see Fig. 3). A, Caves Ridge; B, Main Cave; C, Belfry Cave; D, Isaacs Creek; E, Road.

I Prior to incision of Isaacs Creek. Phreatic cave forms in position of present Main Cave. Water table >640 m A.S.L.

II Pre Late Cretaceous. Water table below level of present Belfry Cave. Vadose speleothem deposited in cave where Main Cave now located. (Corresponds to "A 1" in Fig. 9)

III Late Cretaceous. Basalt flow enters vadose caves forming bodies now exposed in Main and Belfry Caves. (Corresponds to "A 2" in Fig. 9)

IV Late Cretaceous to Eocene. Basalt flow is eroded off surface of limestone. Basalt remains as fill in caves.

V Eocene. Basalt and tuff of the Liverpool Range Beds fills Isaacs Creek Valley. This raises the water table to >640 m A.S.L. Main Cave is formed by phreatic solution.

VI Post Eocene. Basalt is eroded from Isaacs Creek. Belfry Cave becomes a stream sink when approx 30m of basalt still remains in the valley.

VII Present condition with Main and Belfry Caves above the water table.

If the Late Cretaceous water table was below the level of Belfry Cave, as proposed above, then phreatic solution of palaeo-Main Cave occurred much earlier, prior to the excavation of Isaacs Creek which must have been almost complete by Late Cretaceous times.

The small size of the basalt infilling compared to the extensive nature of the palaeokarst suggests that perhaps only a small vadose slot was still open in the Main Cave area, the original larger cave having almost completely been filled with speleothems.

In Eocene times basalt filled the valley of Isaacs Creek. This raised the water table and kept it high and stable long enough to cause nothephreatic solution of bedrock and palaeokarst resulting in the formation of Main Cave (Fig. 19, V). Belfry Cave and other caves at the same level, e.g. Shaft Cave and Helictite Cave, were formed at a later time when headward erosion had removed some of the basalt fill (Fig. 19, VI).

The development of phreatic roof pendants in flowstone in Belfry Cave seems to be related to a local backflooding event rather than to a major change in water table.

The detailed history of Belfry Cave and the later history of Main Cave cannot be elucidated without further study. Both contain thick deposits of entrance facies while Belfry Cave also contains much laminated clay. A study of these, and of the other caves in the area is essential if the problems of of cave and landscape development in the area are to be finally resolved.

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Department of Geology and Geophysics,
University of Sydney,
N.S.W., 2006, Australia.

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The Influence of Sedimentary Environment on the Development of Stratiform Ore Type

R. L. Stanton

ABSTRACT. At the beginning of 1951 when Dr. J.A. Dulhunty assumed responsibility for the teaching of Economic Geology in the University of Sydney, it was believed almost universally that layered sulphide orebodies within sedimentary and meta-sedimentary rocks had been formed by subsurface replacement. It was considered that the relevant hydrothermal solutions were derived from underlying granitic intrusions, and that localization and layering of the orebodies reflected the selectivity of the replacement process.

The year 1951 may almost be seen as a turning point. Within the next two years it began to be suspected that many of these layered sulphide occurrences were not manifestations of selective replacement, but were sediments in their own right.

In the ensuing 35 years not only has this suspicion been abundantly confirmed, but it has also been recognized that such layered ores embrace a wide variety of types, and that such types are in turn largely a reflection of variation in sulphide sedimentology.

It appears that a spectacular example of the influence of sedimentary environment over the development of ore type may be provided by the class of "stratiform skarn" deposits. These stratiform concentrations of calcsilicates and sulphides have been loosely attributed to contact metamorphic recrystallization and replacement of limestone and associated carbonate-rich metasediment. However, it now appears that at least some of these occurrences represent primary seafloor exhalation and alteration, and the direct deposition of calc-silicate and sulphide, in moderately restricted environments of carbonate sedimentation.

This well-known but perhaps frequently misinterpreted ore type may illustrate yet again the influence of sedimentary processes and facies development in the formation of many of our most important ore deposits.

Having worked as assistant to L.L. Waterhouse in 1949 and 1950 I was - for better or for worse - part of Dr. J.A. Dulhunty's inheritance when he assumed responsibility for the teaching of Economic Geology in the Department of Geology and Geophysics in 1951. I had of course been conscious of Dr. Dulhunty and his somewhat mysterious expertise in coal research since my earliest days as an undergraduate in 1944, and I had come to know him as a very pleasant and interesting person. However it was not until I began my close association with him in 1951 that I really came to appreciate his dedication and ability as a scientist and teacher, and his honourableness and humanity as an individual. No young man beginning a scientific career could have had a more kindly, helpful, considerate and encouraging person to work for and I look back with pleasure on the five years of my teaching apprenticeship with him.

As some of you know it was during those five years that I chanced on the then hitherto unsuspected principle that many conformable, or stratiform, orebodies were, as products of volcanic-sedimentary processes, manifestations of volcanic island arc evolution - not, as then currently thought, of much later plutonism.

Such ideas were too unconventional to be entertained by most ore genesis theorists in 1951; in most geological company my thoughts would have received short shrift and I would probably have been discouraged to the point of abandoning them. However, working in the atmosphere of John Dulhunty's tolerance and encouragement I continued with this line of thought in spite of outside criticism, and I think it correct to say that it was ultimately accepted by the scientific world. Whatever credit may be due for this outcome must go largely to John Dulhunty, for his creation of an unoppressive, encouraging scientific environment in which a young man could work freely, unrestrained by the demands of contemporary fashion. Now, 35 years later, I should like to take the present opportunity to acknowledge this, and to thank him.

As 1951 began the hydrothermal replacement theory of formation of concordant sulphide ores in metasedimentary rocks was completely in the ascendant. Adherents to this theory considered that hydrothermal solutions ascending from cooling plutons replaced, here and there and usually with the greatest selectivity, small segments of susceptible sedimentary and metasedimentary materials. Well layered, or bedded ores - "stratiform ores" as they are now familiarly known - were thus seen as pseudomorphs. The possibility that some of them might *be* sediments as well as looking like them was not seriously entertained.

However, although not widely admitted it had by that time been recognized for more than 100 years that sedimentary processes might have played an important part in the formation of many metallic

orebodies. Among the first to be recognized as chemical sedimentary rocks were the banded iron formations, whose volcanic-sedimentary origin was proposed by Whitney in 1854. In 1873 the Norwegian geologist Helland perceived that certain stratiform metallic sulphide ores associated with volcanic rocks and located in volcanic sediments in Scandinavia might have originated as seafloor hot spring deposits. In 1904 Fukuchi and Tsujimoto, and in 1919 Ohashi, proposed a similar mechanism for the formation of the Japanese Kuroko deposits. In Europe Niggli, Schneiderhöhn and many others of the 1920's and 1930's were fully conscious of the importance of sedimentary processes in the deposition of bedded, stratiform orebodies, and in 1948 the German, Hegeman, reenunciated the theory of seafloor exhalative ore formation in quite explicit terms, mentioning Broken Hill specifically in this context.

However these ideas seemed not to penetrate, or to remain in eclipse in, most of the English speaking world. As a young postgraduate student in the early 1950's I was certainly quite unaware of them. Nonetheless 1951 saw the first stirrings of what was to become, by the middle of that very decade, not only a monumental controversy, but a major turning point in the history of ore genesis theory.

Since their discovery in 1926 the great stratiform deposits of the Rhodesian Copperbelt, with their beautifully bedded conformation and close affiliations with sedimentary facies patterns, had been the subject of intense argument between the "hydrothermal replacement" and "sedimentary" schools. Part of the evidence said to favour hydrothermal replacement was the existence of "younger granites" - granite intrusions apparently post-dating the ore-bearing Roan sediments and identified at that time as a likely source of late-stage hydrothermal solutions. However in 1951 W.G. Garlick and J.J. Brummer presented evidence to show that these granites were in fact part of the basement to the Roan Sediments, were thus *older* than these sediments and, therefore, unsustainable as potential sources of solutions that might have induced late-stage replacement of carbonate sediment by hydrothermal sulphide. Garlick and Brummer had thrown down a challenge of the most serious kind and ore genesis theorists of the English-speaking world now had to confront the problem of primary sedimentary ore formation whether they liked it or not.

By 1953 King and his colleagues (King and O'Driscoll, 1953; King and Thompson, 1953) were suggesting that the great Broken Hill orebody, regarded since the work of E.S. Moore in 1916 as an example *par excellence* of highly selective hydrothermal replacement, was sedimentary. In 1954 Ehrenberg and his co-workers proposed that the great bedded sulphide orebodies of Meggen in Germany were seafloor hot-spring deposits and in 1955 Kraume and his colleagues proposed a similar origin for the ores of Rammelsberg. By 1954-55 my own work (Stanton, 1954, 1955a and b) had indicated that some of the small stratiform orebodies south of Bathurst in New South Wales had formed by volcanic: sedimentary/diagenetic processes related to individual volcanic rises of volcanic island festoons such as those of Indonesia and the Solomons - and, as a corollary of this, that many of the world's well-known "metallogenetic provinces" were in fact old volcanic island arc structures.

It now became apparent that sedimentation had to be regarded very seriously as an ore-forming process. For the next decade the world of oregenesis theory - or that part of it concerned with stratiform sulphide ores - divided itself into two groups: the "hydrothermal replacement" school and the "sedimentary" or "syngenetic" school.

Somewhat surprisingly, when one looks back, the sedimentary label was taken to imply a single category: the products of bacterial sulphate reduction accumulated in quiet, poorly oxygenated areas of the seafloor. The naivety of such a view became apparent during the 1960's and it is now recognized that those stratiform deposits that may be regarded as "sedimentary" in the broad sense are derived from a variety of sources, deposited by a variety of mechanisms, and accumulated in a wide variety of sedimentary environments. The latter are now recognised as ranging from saline lakes of arid rift valleys to the deep basaltic ocean floor. Deposits such as the Dugald River Pb-Zn orebody near Cloncurry have in fact been ascribed to sedimentary and early diagenetic sulphide deposition in the sediments of saline lakes. Some copper and Pb-Zn deposits appear to have formed by the subsurface interaction of groundwaters in sabka and shallow back-reef environments prior to the consolidation of the sediments concerned. Deposits such as those of the Kuppferschiefer have been laid down in highly organic nearshore depressions and lagoons. The pyritic gold deposits of the Rand appear to have accumulated, during sedimentation, in near-shore fluviatile and deltaic environments, and in part incorporated in algal mats that grew just below wave base around the fringes of such deltas and associated shorelines. Limestone Pb-Zn deposits are localized in carbonate, usually reef, environments. They have formed in back-reef and fore-reef sediments and in the reef wall itself. Such deposits probably have a long and complex history ranging from reef-associated sulphide sedimentation right through to diagenetic and postdiagenetic sulphide transport and deposition in the reef complex. Conceptually, perhaps the simplest of the sedimentary ore deposits are the Fe-Cu-Zn sulphide lenses, ranging in age from the Archaean to modern, that have formed around hydrothermal plumes such as those recently observed on the modern deep basaltic seafloor. Calc-alkaline volcanism of shallower marine environments - most commonly those of island arcs - also yields sulphide-rich sediments, usually more complex than those of abyssal basalt association. Because of their shallower and hence more varying environments of deposition these accumulations often display zonation of mineralogy reflecting microfacies changes - particularly in oxidation-reduction potential - within the relatively small confines of the ore-forming locale.

Such examples are no more than an indication of the variety of sedimentary and sedimentary/diagenetic ore types that we now recognize. As with other chemical sediments, a wide spectrum of stratiform and related sulphide ore types results from, and reflects, the wide spectrum of sedimentary environments and facies in which sulphides may accumulate.

Perhaps the most widely and commonly occurring of all these sedimentary ores is the pyritic Pb-Zn-Cu type that I first encountered around 1950

at Wiseman's Creek, Peelwood and Captain's Flat, and which are so abundantly developed in the Palaeozoic rocks of Eastern Australia, the Appalachians, Caledonides, the Urals and elsewhere. These are the now familiar stratiform lenses usually dominated by pyrite and/or pyrrhotite, and containing variable quantities of sphalerite, galena, chalcopyrite and minor sulphides, silicates such as the ferrous chlorites, garnets, cordierites, staurolites and so on, depending upon the metamorphic environments, and sometimes minor magnetite. In essence they are small silicate-sulphide facies iron formations, with minor development of oxide facies as indicated by the subordinate incidence of magnetite. The interbedded and enclosing sediments commonly contain carbon, and by far the major part of the iron is in the ferrous form. These ores are products of chemical sedimentation in reduced environments.

A somewhat less common and conspicuous, but nonetheless characteristic, ore type of the same provinces is that of the sulphide-bearing conformable calc-silicate concentrations, or "stratiform skarns". These consist of abundant andraditic garnets and iron-rich epidote, stilpnomelane, carbonate and magnetite, subordinate pyroxene and amphibole, and variable quantities of sulphide, chiefly pyrite, sphalerite and chalcopyrite. The ores are invariably associated with biohermal and related carbonate and carbonate-rich sediments and, with their prominent calc-silicate assemblages, immediately invite identification as contact metamorphic deposits.

However a conspicuous feature of many of these occurrences is that they show no obvious spatial relationship with intrusive margins, on the surface, at least. This is commonly accounted for - in contact metamorphic terms - by postulating a hidden intrusive "at shallow depth". However the fine grainsize and immaturity of grain boundaries in many of the associated carbonates belies such a state of affairs. Other than that their calc-silicate and associated mineral assemblages are very reminiscent of those of true contact metamorphic orebodies, there is little to suggest that these stratiform deposits, often far distant from any visible intrusion, are themselves of contact origin.

It seems likely that, like the associated chloritic, pyritic stratiform Pb-Zn-Cu ores of Captain's Flat-Woodlawn-Wiseman's Creek-Peelwood type, these sulphide-bearing calc-silicate lenses are simply marine exhalative accumulations. Like the Pb-Zn-Cu ores they constitute highly localized iron anomalies - they may be seen as small, base metal sulphide bearing iron formations. However instead of being dominantly *ferrous* iron formations localized in sulphide-rich carbon-bearing sedimentary environments, they are substantially *ferric* iron formations localized in sulphide-poor, carbonate sedimentary environments. With a low sulphide activity in the environment, much iron was precipitated as silicate rather than sulphide. As the locale was one of copious calcium precipitation, the silicates tended to be Fe-*Ca* silicates, and with relatively abundant oxygen, much of the iron was deposited in ferric form. Thus there was precipitated abundant precursor material for the formation of ferric iron-rich andraditic garnet and epidote, together with ferristilpnomelane, chlorites with a ferric component, and magnetite. Formation of abundant, flocculant and gel-like silicate and iron oxide would have protected the lesser base metal sulphides from oxidation, just as the abundant nontronite and goethite of the modern Red Sea-brine deposits protect the associated - and quite abundant - sulphides of those sediments.

Fig. 1 shows what appears to be the nucleation and growth of very high-iron andraditic garnet from a dark, microscopically irresolvable material occurring in drill core from a stratiform, Zn-bearing calc-silicate deposit of the Brindabella Valley, near Canberra. Fig. 2 illustrates the general nature of the relationship between the two ore types now proposed.

On such a basis these two ore types are closely related in their derivation. Their differences stem principally from sedimentary factors - simple differences in the environments, that is the facies, to which the exhalations were contributed and in which the resultant minerals accumulated.

May I suggest that this is just one more illustration of the importance of sedimentology - in its broadest sense John Dulhunty's life-long interest - in understanding, predicting and searching for the world's mineral resources.

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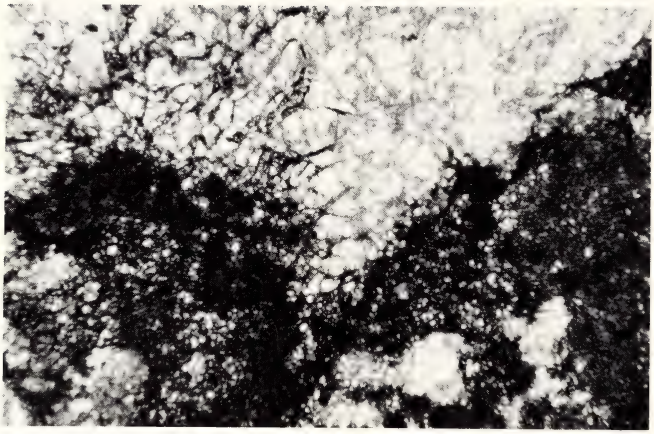

Fig. 1. Apparent nucleation and growth of andraditic garnet (light) in amorphous to microcrystalline precursor material (black). Ordinary transmitted light, x 10 approx.

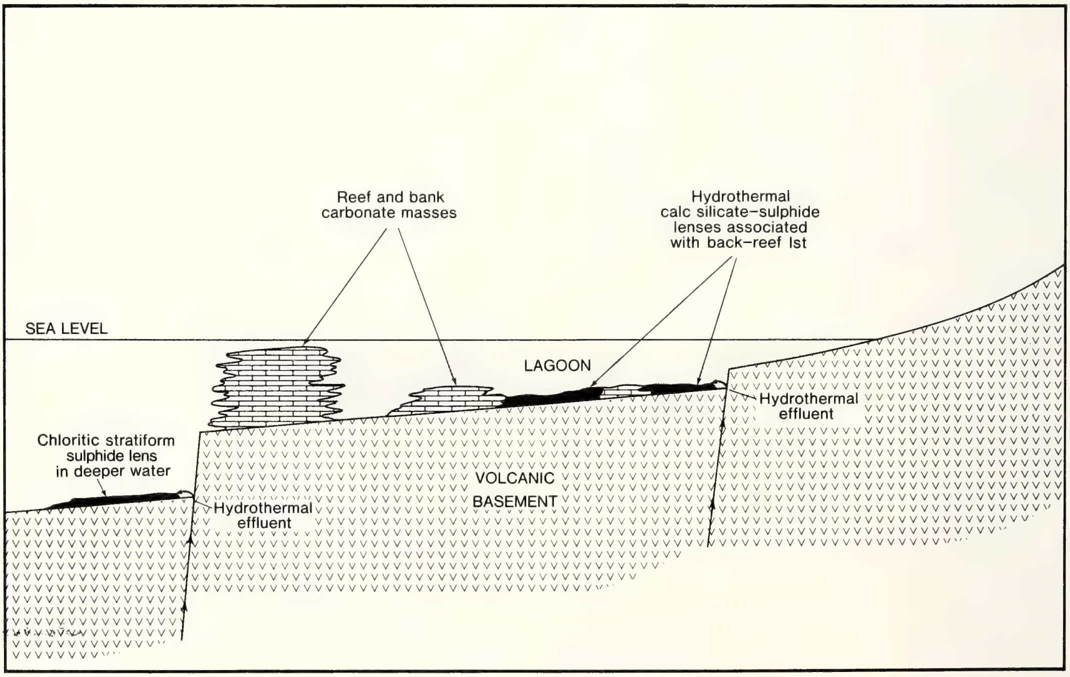

Fig. 2. Proposed relations in principle between stratiform base metal sulphide deposits of "Captain's Flat-Woodlawn" type and those of "stratiform skarn" type. The two have essentially similar derivations but the former is deposited in deeper, quieter, reduced environments whereas the latter is deposited in relatively shallow, carbonate reefal environments.

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R.L. Stanton,
Department of Geology,
University of New England,
ARMIDALE, N.S.W. 2351

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The Rapid Weathering of a Siltstone

D. J. Swaine

ABSTRACT. The rapid weathering of an excavated siltstone at Liddell, New South Wales, was investigated. Framboidal pyrite in the siltstone was intimately associated with carbonate minerals, mainly dolomite. It seems that the pyrite was oxidised by air, water and probably iron-oxidising bacteria to form sulfuric acid which reacted with the carbonate minerals to form gypsum and epsomite. These chemical changes together with some expansion effects caused the breakdown of the siltstone.

INTRODUCTION

The rapid breakdown of some excavated rock at Liddell has been investigated. Liddell is about 90 km northwest from Newcastle, which is about 130 km northeast from Sydney, New South Wales. The Permian sedimentary rocks are in an area near the base of the Mulbring Subgroup and are mainly sandstones overlain by siltstones.

SAMPLING

The samples of siltstone were taken near the site for the Liddell Power Station, prior to its erection. Brief details of the samples include the initial description at the time of sampling and the designation, based on a mineralogical examination, in terms of the classification given by Williams, Turner and Gilbert (1955).

Sample A is siltstone (subfeldspathic lithic wacke) which was freshly removed by jackhammer from a slope.

Sample B is a siltstone (subfeldspathic lithic arenite) which was collected similarly to Sample A, at the same level but 15 m away from it.

Sample C is a siltstone (subfeldspathic lithic wacke) which was sampled 2 m below the weathered material (Sample D).

Sample D is weathered material from a level 12 m above Sample A.

Sample E comprises several pieces of weathered material which were collected from the slope near the area where samples A to D were taken.

Sample F is a piece of core which was regarded as representative of the basement sandstone (lithic arenite).

METHODS OF ANALYSIS

The mineralogical composition was studied by microscopy using polished section and thin section techniques, and by X-ray diffraction. Major elements were determined by X-ray fluorescence spectroscopy, trace elements by atomic emission spectrography and carbon and sulfur by chemical methods. The nature of the organic matter was ascertained by a differential thermal analysis-technique (Swaine, 1969) using samples of powdered rock which had been demineralised by treatment with hydrochloric and hydrofluoric acids. An estimate of pyrite (FeS_2) was obtained by determining pyritic iron.

RESULTS AND DISCUSSION

The results of the chemical analyses are given in Table 1. The compositions of the three siltstone samples (A, B, C) are remarkably similar. The highest silicon and the lowest aluminium and iron values are in the sandstone (Sample F), as expected. There is more organic carbon, total sulfur, sulfate sulfur and pyrite in the siltstone samples than in the sandstone sample. The most important differences are the lower values for calcium, carbonate (given as CO_2), organic carbon, total sulfur, sulfate sulfur and pyrite in the weathered siltstone (Samples D and E), compared with the siltstone.

The trace-element contents (Table 2) are not unusual and are mostly between those for an 'average' sandstone and an 'average' shale. These results would seem to indicate that trace elements are not a relevant factor in the weathering of the siltstone. The values for boron in the siltstone. namely, 70-80 ppmB, are in the range that has been suggested for coals and associated sediments that have been exposed to brackish water, probably during their early history (Swaine, 1962, 1967, 1971).

Microscopical examinations gave detailed information on the mineralogy of the samples.

Sample A is a subfeldspathic lithic wacke. The detritus is composed of quartz (80%) of plutonic and volcanic origin, plus volcanic (acid-intermediate) and metamorphic (metaquartzites, schists, gneisses), rock fragments (15%), with feldspars (mainly plagioclase) and micas forming a minor component (2-3%). Detrital calcitic oolites, minor opaques (ilmenite, pyrite and organic matter) also form a minor component (2-3%). Material of argillaceous composition totals ~10% of the rock, and cement material, chiefly siderite, weathered to limonite, plus a small fraction of framboidal pyrite totals 5-10% of the rock. The average grain

TABLE 1

CHEMICAL COMPOSITION (as per cent)

	A	B	C	D	E	F
Si	32.1	31.9	30.6	30.9	32.3	35.6
Al	6.5	6.4	7.3	7.8	6.9	5.5
Fe*	2.6	2.5	3.0	3.9	3.6	1.7
Mg	0.8	0.7	0.8	0.6	0.5	0.5
Ca	0.8	0.7	0.7	0.06	0.05	0.8
Na	1.0	0.9	1.3	1.0	1.2	1.3
K	2.3	2.2	2.3	2.1	2.0	2.2
H_2O^-	2.0	2.1	2.7	5.4	2.3	0.9
Ti	0.4	0.4	0.4	0.4	0.4	0.2
P	~0.1	~0.1	0.05	~0.1	~0.1	<0.1
CO_2	2.4	1.7	1.4	0.1	0.1	3.0
C (org.)	0.9	1.0	1.7	0.2	0.3	0.06
S^+	1.0	1.1	1.2	0.2	0.2	0.2
SO_4 (as S)	0.2	0.3	0.2	0.06	0.05	0.05
FeS_2	1	1	1.5	0.05	0	0.05
	SILTSTONE				WEATHERED SILTSTONE	SAND-STONE

* total iron
\+ total S

TABLE 2

TRACE-ELEMENT CONTENTS (as parts per million)

	A	B	D	E	F	"Average" sandstone	"Average" shale
B	70	80	80	100	30	35	100
Ba	600	600	600	400	1000	50	580
Co	10	10	<8	<8	<8	0.3	19
Cr	70	90	100	80	30	35	90
Cu	20	30	30	20	15	5	45
Ga	10	10	20	10	15	12	19
La	~80	~80	~80	<80	<80	8	20
Mn	200	200	70	100	200	50	850
Mo	~1	2	3	~1	<1	0.2	2.6
Ni	20	15	10	7	3	2	68
Pb	10?	10?	10?	<10	10?	7	20
Sc	~10	~10	15	~10	~10	1	13
Sn	<10	<10	~10	<10	<10	0.5	6
Sr	300	400	300	300	300	20	300
V	100	80	150	60	40	20	130
Y	15	10	20	15	10	9	18
Zr	250	250	250	200	150	220	160

NOTES A value of <x means that the element was not detected, i.e., if present, it is less than x ppm, where x is the limit of detection.

No lines were seen for Ag (1), Be (6), Ge (6), In (10), Zn (300); the figures in parentheses are limits of detection.

Values for "average" sandstone and shale are from Turekian and Wedepohl (1961), except for lanthanides from Haskin and Gehl (1962).

size of detritus is ~0.1 mm.

Some detrital pyrite occurs, but framboidal pyrite is the dominant habit. The latter is found chiefly in the matrix, but often within volcanic rock fragments and in clusters between detritus. Distribution of framboids is to a large extent controlled by bedding planes, indicating a syngenetic origin. However, this cannot be clearly separated from the origin of pyrite within detritus, which is certainly of later, diagenetic origin. It would seem that a continuous process is in operation. Pyrite and siderite share an important role in diagenetic processes in the rock. They both occur intimately associated with the detritus along fractures and grain boundaries of rock fragments, and in fractures in quartz grains. It is postulated that siderite and some pyrite were originally deposited in the fracture and have since been remobilised by recrystallisation of glassy phases. This means that the siderite-pyrite phase was deposited pre-devitrification in the diagenetic history. The sample chronology is supported by evidence in other samples.

Particle size of pyrite as framboidal aggregates varies between ~15μm and 1 μm, with a mode of 5 μm. Clusters of 5 μm framboids reach 80 μm across. Individual 'crystallites' making up the framboids have a relatively constant size of 1-2 μm. Organic material is relatively common and fragmentary. Pyrite is sometimes associated with organic matter, but no strict association of either with the other can be discerned. The organic material has a size range 30 - 200 μm with a mode around 50 μm.

Sample B is a subfeldspathic lithic arenite. The detritus is composed of quartz (80%) of plutonic and metamorphic origin, volcanic (andesine, anorthoclase) and metamorphic (microcline) feldspars (5%), and volcanic and metamorphic rock fragments (10 - 15%). Micas, opaques, calcitic oolites, and shell fragments together form a minor component. Oolites show a concentric growth structure, nucleated on fine quartz grains. Argillaceous material totals 5 - 10% of the rock and cement material (mainly siderite) ~5% of the rock. The average grain size of the rock is 0.2 - 0.3 mm.

The chief habit of the pyrite - occurring mainly in the matrix - is framboidal. Detrital pyrite is minor. Pyrite is usually intimately associated with the siderite cement. Framboid size varies between 5 and 35 μm. It was noted in this sample that the 'crystallites' making up the framboids had, probably, an octahedral habit and were relatively constant in size (1 - ~3 μm). Clusters of framboids range up to 100 - 150 μm. Siderite occurs ubiquitously, intimately associated with the matrix material and in fractures. Plagioclase grains are frequently replaced by a carbonate, probably calcitic. Organic matter is often associated with pyrite, but the relationship is by no means conclusive as to the formation of individual framboids. Only the smaller framboids are associated with organic material.

Sample C is a fine grained subfeldspathic lithic wacke. The detritus is composed mainly of quartz (80%), volcanic and metamorphic rock fragments (15%), and minor feldspars and micas (5%). Argillaceous matrix material totals 15% of the rock, and cement material, mainly siderite, totals 10%. The average grain size is 0.03 - 0.05 mm.

Framboidal pyrite occurs ubiquitously in the matrix, though its concentration is highly variable. Limited areas show large (0.5 mm) clusters of minute framboids. One such cluster has a semi-ordered array of 2 μm framboids interspersed with octahedral crystals of comparable size. Individual crystals making up the framboids are sub-micron size (~0.03 μm) and difficult to resolve, but the octahedral crystals occurring with them are uniformly of 2 - 3 μm size and comparable to crystallites in the larger framboids in this and other rocks, implying that they have a similar origin. Organic matter is common in this rock and may reach 2 - 3%. Some structure is present, and is not associated to any great degree with pyrite. As in the other rocks, siderite forms the main cementing agent and is intimately associated with all the components of the rock. A carbonate phase has formed by replacement of plagioclase feldspars, probably a calcic carbonate, as no limonite alteration is seen.

Sample D contains fine grained quartz, muscovite and feldspars in a matrix of limonite and clay. Larger quartz grains (up to 0.5 mm) are occasionally present. Limonitic spherules, presumably oxidised framboids, are dispersed in the matrix.

Sample E contains quartz, muscovite, feldspars and rock fragments in a limonitic clay matrix. Translucent brown limonite spherules, presumably oxidised pyrite, about 10 μm in size, are common.

Sample F is compositionally classified as a lithic arenite. A grain size classification would place it in a fine grained conglomerate class, with average grain size 2.5 mm. The detritus is composed of volcanic and metamorphic (gneissic) rock fragments (40-50%), quartz (plutonic and metamorphic 40%) and minor feldspars, micas and opaques (10%). Matrix material of argillaceous composition is minor, and cementation is effected by a siderite-limonite mixture, supplemented by welding of the detritus. Calcite replaces nearly all of the feldspars (including microcline in gneissic rock fragments) to some extent.

Some detrital pyrite occurs, but framboidal pyrite is conspicuous by its absence, especially in the matrix of the rock, where its occurrence is rare. The only significant occurrence of pyrite is in volcanic rock fragments, where it exhibits similar features to that in other rocks. Pyrite, where present varies from 2 - 15 μm (mode 3 μm). Siderite and limonite play an important role in the matrix of the rock, and as cementers, constituting ~15% of the rock. They occur as limonite-stained siderite nodules generally 10 μm in diameter, with limonite as an alteration product of siderite giving it a deep red colour. Their origin would appear to be diagenetic, possibly

replacing pyrite but more likely formed by nucleated growth. Common occurrence is a small volcanic rock fragment between two large quartz grains, cemented by these nodules, and a surrounding siderite mass which is not as limonitised. The replacement is virtually complete, illustrating the high degree with which this rock has been filled with siderite. The critical point in this rock is that virtually no pyrite is present, siderite and limonite being the cement. Organic matter is very low in abundance and rarely has pyrite associated with it.

Mineralogical information obtained by X-ray diffraction is given in Table 3. It should be

TABLE 3

MINERAL COMPOSITION DETERMINED BY X-RAY DIFFRACTION

	A	B	C	D	E	F
quartz	G	G	G	G	G	G
feldspar	G	G	L	G	G	G
muscovite	L	L	L	L	L	L
kaolinite	L	L	L	L		L
montmorillonite			T			
dolomite	T	T	T			L
pyrite	*	*	*	*		
gypsum	T		T?			

NOTES
G means a major constituent of sample
L means a lesser constituent of sample
T means a trace constituent of sample
? means possibly present
* means that a trace of pyrite is possible, but all diagnostic peaks are masked by muscovite or other major constituents.

noted that low concentrations of pyrite, say of the order of 1%, are not easily detected by X-ray diffraction in samples of siltstone and sandstone, whereas pyrite is readily detected by the polished- and thin section-techniques as used in microscopy. It is not always easy to identify particular carbonate minerals by microscopy, but X-ray diffraction is usually specific, provided that the mineral is present at a concentration of at least about 1%. Hence, it is considered that dolomite (Ca, Mg $(CO_3)_2$) is the main carbonate mineral in the siltstone samples (A, B, C), although siderite ($FeCO_3$) was reported using microscopy. There may be traces (less than about 1%) of calcite ($CaCO_3$) and siderite in the siltstone samples. Calcite was detected in two other samples of siltstone not considered above. Pyrite in the siltstone samples is largely framboidal, that is, it is made up of discrete, spheroidal aggregates of microcrystallites of FeS_2. Framboidal pyrite from a Liddell siltstone is shown in the two scanning electron micrographs, (Figs. 1a and 1b). Fig. 1a shows a cluster of framboids, each one being about 15 μm in diameter. The raspberry-like texture is shown well in Fig. 1b of a single framboid, the microcrystallites being about 2 μm in diameter. Some of the microcrystallites are angular, rather than rounded; such variations in shape and in size have been reported for framboids in shales (Love and Amstutz, 1966).

Fig. 1a. Cluster of framboids, each one about 15 μm in diameter.

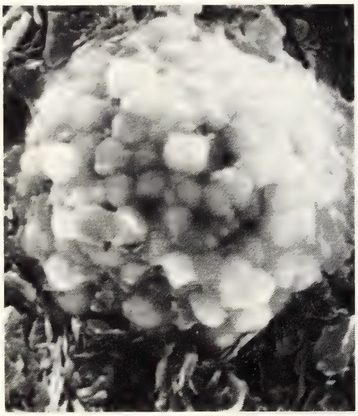

Fig. 1b. Raspberry-like texture of a single framboid, the microcrystallites being about 2 μm in diameter.

At the level of a few per cent of carbon in rocks it is difficult to ascertain the form of carbon by X-ray diffraction. However, this may be done by a differential thermal analysis-technique (Swaine, 1969). The carbon in the siltstone samples was shown to be in the form of bituminous coal. This is not surprising since the sediments are near bituminous coal seams.

X-ray diffraction was carried out on some hand-picked material. A white layer removed from some rock fragments contained dolomite and a trace of pyrite. Vein material and crystals from the weathered zone were mainly gypsum ($CaSO_4.2H_2O$). This explains the low calcium and sulfate in the weathered samples (D, E). Evidently the gypsum was precipitated from solution in discrete areas rather than evenly disseminated. Thin white crust on pieces of siltstone that were weathering contained epsomite ($MgSO_4.7H_2O$).

The results given above may be summarised concisely. The weathered siltstone contains less calcium, carbonate, carbon and sulfur than the siltstone. The pyrite and carbonate minerals (mainly dolomite) in the siltstone are not in the weathered material, which contains gypsum and epsomite. These observations suggest that the pyrite has been oxidised to produce some sulfuric acid which has reacted with the dolomite to form gypsum and epsomite. The oxidation of pyrite is complicated and incompletely understood (Lowson, 1982), but the following reactions, although simplified, cover the main points (Swaine, 1979):

(a) $2FeS_2+7O_2+2H_2O = 2FeSO_4+2H_2SO_4$

(b) $4FeSO_4+O_2+2H_2SO_4 = 2Fe_2(SO_4)_3 + 2H_2O$

(c) $14Fe^{3+} + FeS_2+8H_2O = 15Fe^{2+}+2SO_4^{2-} + 16 H^+$

(d) $Fe_2(SO_4)_3+(3+x)H_2O = Fe_2O_3.x\ H_2O + 3H_2SO_4$

Reaction (b) is slow unless there is a catalyst present. It has been proposed by Singer and Stumm (1970) that iron-oxidising bacteria, for example, *Thiobacillus ferrooxidans*, can catalyse the oxidation of ferrous to ferric iron. Details of this process are given by Lundgren and Dean (1979). The ferric iron oxidises pyrite (reaction c) and is also hydrolysed to give hydrated iron oxide (reaction d). These two reactions yield sulfuric acid which keeps the pH down to below about 3 which favours the optimum growth of the bacteria. The *Thiobacillus ferrooxidans* bacteria seem to be attached selectively to pyrite surfaces (Gormely and Duncan, 1974) thereby being readily available to catalyse the oxidation of ferrous iron, as soon as the pH is lowered by the formation of sulfuric acid (reaction a).

In the presence of oxygenated water, where the redox potential (Eh) is about 0.4v, pyrite is relatively unstable (Hem, 1960). The large surface area of framboidal pyrite (Fig. 1b) allows ready access of water and air (oxygen) which would promote oxidation of the pyrite. The rate of oxidation of pyrite ($dw/_{dt}$) depends on several factors (Swaine, 1980) and can be expressed as

$$\frac{dw}{dt}\ (FeS_2)\alpha[O_2],SS_{H_2O},pH,T,MB,SA,OM$$

where $[O_2]$ = concentration of oxygen, SS_{H_2O} = surface saturation with water, pH = acidity, T = temperature, MB = microbiological factors, SA = surface area and OM = other minerals present. The important factors in the weathering of the siltstone are the availability of air, water and probably iron-oxidising bacteria. The close association of the framboidal pyrite with carbonate minerals, mainly dolomite, has enhanced the neutralisation of sulfuric acid to form gypsum and epsomite. The coaly matter in the siltstone has also been oxidised (Table 1), but it has not been possible to ascertain if this reaction has enhanced or retarded the oxidation of pyrite. The role of water may be that of a reaction medium or its main function may be 'to provide a means by which the oxidation products are desorbed (dissolved) from the pyrite surface' (Morth and Smith, 1966). However, water takes part in reactions (a), (c) and (d), so it should be seen as essential for the complete oxidation to proceed.

CONCLUDING REMARKS

The Liddell siltstone seems to be unusual in having framboidal pyrite intimately associated with carbonate minerals, mainly dolomite. Exposure to air, water and, probably, suitable iron-oxidising bacteria caused rapid deterioration of the siltstone. The oxidation of the pyrite released sulfuric acid which reacted with the dolomite to form gypsum and epsomite. Since the oxidation products occupied a greater volume than the pyrite, some expansion must have taken place. The resultant weathered material should be reasonably stable. The siltstone *in situ*should not oxidise if air and water are excluded and seepage water would be very low in oxygen, because of bacterial breakdown of organic matter at the sediment-water interface. There could also be bacterial reduction of sulfate ions when conditions are anaerobic. It is salutary to recall the statement by Krauskopf (1967) that 'we can decipher what happens chemically in the decay of a rock, but we have no means of predicting accurately what the state of the rock will be at a particular time in the future'. However, at Liddell there is no further evidence of weathering of the siltstone since excavation ceased, which would seem to endorse the above suggestions for keeping oxidation at a minimum.

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Department of Inorganic Chemistry,
The University of Sydney,
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Academic Studies and the Coal Industry
The Sampling of Coal as a Bulk Commodity

D. K. TOMPKINS

ABSTRACT Nearly all of the standards which govern the taking of samples of coal for commercial evaluation may be regarded as "old generation" standards. In general, they have been developed from procedures which were in common use more than 50 years ago when the quantities of coal traded and used were considerably smaller than today, when the needs for accurate evaluation of qualities were much less and when coal handling systems were quite primitive by today's standards. Many of the national and international organisations responsible for the publication of coal sampling standards have been actively pursuing the review of existing documents and procedures with the aim of generating new standards more appropriate to modern practice and conditions. The Standards Association of Australia has taken a leading role in this work and a new Australian Standard in 8 parts was published in 1984/85. This new standard, whilst generally acknowledged as a considerable advance on the previous Australian Standard, published in 1975, must however be regarded as an interim measure and further work is proceeding with a view to incorporating, in the next edition, as many as possible of the advances in sampling theory which have occurred and been refined over the past 5 to 10 years into what must always be an essentially practical document.

This contribution examines some of the aspects of changes in the latest coal sampling standards, with particular reference to the new Australian Standard and attempts to identify some of the further changes that seem likely to be introduced in these same features in the next generation of standards. Future standards will be aimed at further improving the accuracy of sampling coal as a bulk commodity and allowing the development of sampling schemes which are appropriate to the massive infrastructure of the industry.

INTRODUCTION

Academic as well as practical studies of coal are of importance to the industry. They range from those which lead to more efficient extraction and preparation to those which enhance the value of the product in the market place and the acceptance of the product for a broadening range of applications in the face of strong competition.

In the current climate of oversupply buyers can afford to be more selective than they have been in the past. Small quality advantages may mean the difference between winning and losing contracts. Thus accurate sampling and analysis become more important than ever before. Significant advances have been made over the last decade, especially through introduction of instrumental techniques, in characterising both coking and steaming coals in terms of conventional parameters, while new techniques such as coal petrography are steadily gaining wider acceptance as a result of more standardised procedures.

Improvements in the testing arena will however be of limited value unless the procedures for taking accurate and representative samples can be brought to a similar standard. Accurate sampling is essential at all stages of the coal evaluation chain, especially in the ultimate stages of marketing and utilisation.

The large numbers of tests carried out to assess the various qualities of coals during their production, preparation, marketing and utilisation rely on samples taken according to principles laid down in a variety of national and international standards. The most prominent standards in use the international trade are British (B.S.), International (I.S.O.), and American (ASTM). All of the sampling schemes followed in Australian coal exports are designed to comply with all of them. Australian Standards (A.S.), which until recently have been little more than an endorsement of the relevant B.S. document, with generally minor amendment, have tended to be used mainly for internal purposes in the same way that Japanese (JIS), German (DIN) Soviet (GHOST) and others have been used in their respective countries and regions.

All of these standards recognise that coal comprises particles of infinitely varied shapes and sizes which have different physical and chemical properties, so that for a sample to be representative it must be collected by taking a

number of portions or increments distributed throughout the whole of the mass.

All of them emphasise that the whole of the bulk of the coal to be sampled should be exposed so that all parts are equally accessible and have an equal chance of being included in the sample: the most favourable sampling situation is where the coal is being conveyed on a belt or similar device so that it passes the sampling point as a stream.

Precautions for avoiding bias where there is a likelihood of periodic quality variations coinciding with the frequency of taking increments and where size segregation and other factors may result in some particles being excluded, are outlined.

They contain stringent requirements on the minimum masses of increments which should be taken in order to be reasonably representative of the surrounding material and the minimum number of increments to be taken in particular sampling situations to achieve a required standard of precision.

There is generally, inadequate coverage of procedures required in mechanical sampling systems and on-line sample preparation trains which are an essential part of the modern coal-handling plant, and of procedures which are necessary to maintain sample integrity through to the testing laboratory.

In short, most of the national and international standards in use today in the coal industry and in trade worldwide may be considered to be out of date to a greater or lesser extent and in need of review. This is a slow and somewhat difficult process in view of the sectional interests which are at play and the entrenched practices even at the national level. The task of developing a satisfactory standard at the international level to incorporate all the advances in sampling theory and practise of approximately the last 50 years is a daunting one. It is perhaps not surprising that the ISO technical committee which commenced this work in 1979, has progressed through eight drafts of the new standard before releasing a Draft Proposal Standard, which is at present in the course of preparation.

Progress, at the national level, is slightly easier to achieve. In order to most avoid later conflict most of the groups responsible for the development of standards in the main coal producing and consuming countries prefer to defer final decisions on contentious issues until some clear indication of the directions which the new international standard will follow emerge.

In Australia however the situation was perhaps a little more urgent than elsewhere on account of the rapid advances in the coal industry during the 1960s and 1970s to the point where today's production of black coal has reached about 130 million tonnes of raw coal and over 100 million tonnes of saleable product, about three quarters of which is exported to an ever increasing range of markets and one quarter of which is consumed locally. Almost all of this massive tonnage requires sampling, much of it two to three times and to varying levels of accuracy with the highest levels of accuracy being required at the end of the processing and marketing train where it passes from producer to receiver. The fact that such a high proportion of Australia's total output of black coal is exported to the markets of the world has meant that the sampling infrastructure has had to be built up to keep pace with the development of facilities to handle such quantities. At the Kooragang Island Coal Loader at Newcastle for example, which is one of the largest of such installations in the world, the designed maximum loading rate is 10500 tonnes per hour. There are many others in Australia which approach this high level of throughput. These installations require massive and sophisticated sampling plants which apart from the purely mechanical problems which they introduce, create some very special problems in sampling.

It is not surprising that there are considerable difficulties in translating the principles which are enunciated in standards developed over fifty years ago in relation to individual increments of a few kilograms and handling rates of a few hundred tonnes per hour at most to situations where the individual increment will be many hundreds of kilograms and rates many thousands of tonnes per hour.

In these circumstances the Standards Association of Australia's Committee on Coal and Coke, working under the direction of the Mineral Standards Board in 1979 saw the need for an urgent revision of the existing standard which had been published in 1975. The resulting work lead to the publication of a new Australian Standard, covering both coal and coke in several parts during 1984 and 1985:

Part 1 :	Guide to the Use of Parts 2 to 8
Part 2 :	Hard Coal - Sampling from Moving Streams
Part 3 :	Coke - Sampling from Moving Streams
Part 4 :	Hard Coal - Sampling from Stationary Situations
Part 5 :	Coke - Sampling from Stationary Situations
Part 6 :	Hard Coal - Preparation of Samples
Part 7 :	Coke - Preparation of Samples
Part 8 :	Determination of Precision and Bias

INCREMENT MASSES

Most of the existing coal sampling standards specify a minimum increment mass calculated from an empirical equation relating mass to maximum particle size as a linear relationship, e.g.

$P = 0.06D$

where P = minimum mass of the increment in kilograms

D = nominal topsize of the coal in millimetres

This approach results in very variable numbers of particles depending on topsize, ie in most cases a 100 fold increase in particle numbers for the 10-fold reduction in topsize from 50mm to 5mm which is the normal range over which bulk coal

is sampled. The equation is not applicable to material larger than about 150mm where less than one particle would be accommodated in the minimum increment. This means that some modification to the basic formula must be introduced. But even so the number of particles is still in most cases very considerably less than when sampling small coal and would result in much lower precision if the same rules regarding the number of increments taken from a testing sample were followed.

The new Australian Standard - and others under review - use an equation for determining increment mass based on the cube of particle topsize and include a density factor e.g:

$P = 3 \times 10^{-5} \rho D^3$

where P and D are defined as above and

ρ = average density of the coal in kilograms per cubic metre

3×10^{-5} = a factor obtained from the relationship $\frac{10\pi t^2}{6} \times 10^{-3}$ where t = Student's t

This formula, while theoretically more appropriate, still results in major practical problems which in some circumstances, tend to outweigh the advantages. The advantages are that with small particle sizes (say 5 - 10mm) which are sampled at the secondary and tertiary stages of a mechanical sampling plant after primary increments have been crushed, the minimum mass required will be smaller than required by the old standards but the disadvantages are that with large coal (say 150mm or greater) which may require sampling at for example the mine, the minimum mass will be extremely large.

One solution to this problem may be to discard the principle of minimum increment masses altogether and replace it with the concept of minimum sample mass based on the measured co-efficient of variation between individual particles for the quality characteristic under consideration e.g.

(1) using a formula based on the measured coefficient of variation between individual particles of the quality characteristic under consideration

$$m_G = \frac{0.03}{1000}\left[\frac{C_v}{s}\right]^2 \rho D^3$$

where

m_G = minimum gross sample mass in kilograms

0.03=rounded value of the coefficient $\frac{10\pi t^2}{6} \times 10^{-3}$ for t = 2.262

C_v = coefficient of variation between particles of the quality characteristic under investigation

β_S = required relative sampling precision at the 95 percent confidence level expressed in percent

ρ = density of the ore particles (not bulk density) in tonnes per cubic metre

D = nominal top size of the ore in the lot in millimetres, or

(2) considering coal as a binary type ore consisting of mineral and gangue particles only and applying the following formula derived by Gy

$$m_G = \frac{(100 - c)\left[(100 - c)\rho_c + c\rho_g\right] lfgD^3}{250\, c\beta_S 2}$$

where

m_G = minimum gross sample mass in kilograms

c = concentration of the mineral species containing the quality characteristic of interest in percent

ρ_c = density of mineral particles containing the quality characteristic of interest in tonnes/m^3

ρ_g = density of the gangue particles in tonnes/m^3

f = particle shape factor (normally 0.5)

g = size range factor (between 0.25 and 1.00)

D = nominal top size of the ore in millimetres

l = liberation factor

= $\sqrt{D_l/D}$, where D_l is the nominal top size in millimetres at which complete liberation occurs

s = required relative sampling precision at the 95 percent confidence level in percent

This approach would introduce the necessity to know much more about the actual material under consideration, which is also of importance in other factors of the sampling process. It would obviate the need to assume that the material will behave in much the same manner as any other coal or that it can be allocated to one of the limited number of classes all the numbers of which are assumed to have similar sampling characteristics.

Even if it were feasible to carry out the necessary test programs to determine all of the appropriate co-efficients of variation of all of the coals one may wish to sample there may still be difficulties. The sampling devices and systems to perform the tasks on a routine basis are usually not capable of infinite variation and some compromise has to be found.

INCREMENT NUMBERS

The number of increments to be taken from a quantity of coal and the manner in which they are grouped to form samples for analysis is a function of the variability of the lot and the precision required in the final test result. This variability depends on the amount of segregation present in the bulk for which the test result is required, the particle size range of the coal and the size of the parcel. Furthermore it is influenced by a wide range of geological, mining, preparation and handling factors. The desired precision will be determined by commercial factors.

Most sampling standards have been developed around a "reference" standard of precision

generally set at $\pm$ 10% relative precision and the sampling of quantities up to 1000 tonnes with certain variations. The variations are designed to take account of the difficulties of sampling in some situations such as stockpiles or wagons compared with the preferred situation of a falling stream where parts of the mass should have an equal chance of being taken and different "conditions" of coal, e.g. cleaned, uncleaned, sized, graded and blended.

Whilst it would have been difficult to conceive of the very large quantities of coal in individual consignments today, the old standards do provide a mechanism for adjusting the number of increments when it is necessary to sample consignments larger than 1000 tonnes e.g. by multiplying the number of increments specified for a mass of 1000 tonnes by the formula

$$\frac{\text{consignment mass (in tonnes)}}{1000}$$

But generally preference is given to simply divide the total quantity into a series of 1000 tonne portions and treating each of them individually. Standards also provide mechanisms for adjusting the precision to a level different from the $\pm$ 10% "reference" usually through formulae which are of limited value unless the chosen precision is fairly close to the "reference" standard e.g.

$$n_d = n_i \left[\frac{4A_1^{\ 2}}{5A_2^{\ 2} - A_1^{\ 2}} \right]$$

where n_d = number of increments required for the desired precision
n_i = number of increments for 1000 tonnes, specified according to the condition of coal and the sampling situation
A_1 = "reference" standard of precision
A_2 = desired precision

These approaches have been considered unsatisfactory and inappropriate to modern practice and the more stringent uses to which analyses performed on the samples are put, for the following reasons.

1. The "reference" standard at $\pm$ 10% relative precision is too low (numerically high) for most commercial purposes and tends to encourage very imprecise sampling.

2. The "initial" numbers of increments are set in relation to impractically low masses of consignments (1000 tonnes) and the procedures for adjusting increment numbers for more practical masses wil lead to loss of sample integrity, in particular loss of moisture which is in most cases, particularly in a commercial transaction, one of the most important test parameters.

3. They assume that all coals in the same general category or condition will have the same or similar increment variance and therefore the same sampling characteristics, e.g. that all uncleaned coals will be more variable than all cleaned coals which is not necessarily the case. In fact with modern coal cleaning practices it is often the highest grade and most refined products which are the most difficult to sample.

Because of the problems outlined above, many of the newer standards including the new Australian Standard focus greater attention on the variability of the actual coal to be sampled. They set out fairly staightforward procedures for establishing a sampling scheme which will achieve the desired level of precision through determinations of primary increment variance (normally of ash but other critical parameters may be substituted) and the variance of sample preparation and testing. Using this approach the number of increments required to achieve the desired precision for the lot to be sampled may be calculated from the equation

$$n = \frac{V_I}{\left[\frac{\beta_{SPT}}{2}\right]^2 - V_{PT}}$$

where n = number of increments
V_I= increment variance
VPT= variance of preparation and testing
βSPT=overall precision

Despite these changes of approach, which give a theoretically more correct determination of the numbers of increments to be taken, there are still problems in practice. For example if the quantity of coal to be sampled is to be treated as a single entity (one gross sample for testing) the number of primary increments to be taken may be quite large, even for relatively low levels of overall precision and it may not be possible to take increments at the required frequency. In addition, if the selected or desired precision is high (numerically low) in relation to variance of preparation and testing then the number of increments cannot be calculated.

In these circumstances it becomes necessary either to select a new overall precision (worse than that originally selected) or to divide the consignment into a number of sampling units and to perform preparation and testing on each of them individually and to average the results. In this case the number of increments required from each of the sampling units may be calculated from the equation

$$n = \frac{V_I}{n_s \left[\frac{\beta_{SPT}}{2}\right]^2 - V_{PT}}$$

where n, V_I, V_{PT} and β_{SPT} are defined as above and n_s = number of sampling units.

It is usually possible, within the practical limitations of any particular sampling system to achieve a desired or commercially acceptable precision using different combinations of increment numbers and sampling units.

The method for determining quality variation based on measuring the variance V_I of a particular quality characteristic for all increments from the lot assumes that the quality of the coal varies in a random manner throughout the mass and that the obervations will follow a normal distribution. This is not strictly correct as coal flowing in a stream will frequently display serial correlation i.e. long-term trends superimposed on the random short-term variations. The value of V_I and thus the number of increments required for the desired precision may therefore be overestimated.

New approaches, including the use of variograms and the calculation of fundamental sampling error as described by Gy (1982), Royle (1983), Holmes (1985), Lyman (1986) and others have been proposed to overcome these difficulties and it is expected that they will be incorporated in future editions of coal sampling standards.

DIVISION OF PRIMARY INCREMENTS

Earlier standards did not permit division of a primary increment prior to crushing but with the much larger masses handled in modern systems this would be impractical. Therefore new systems have been introduced for dividing these quantities into a mass suitable for crushing. This is usually done as an online function.

MECHANICAL ASPECTS

The older standards contained insufficient information on critical design factors for components of a mechanical sampling system, in particular cutter velocities and cutter apertures and the angles at which cutters intersect the stream of coal. The factors have a significant bearing on the bias which may be introduced. Limitations are now being put on these factors but to date have been determined on a mainly theoretical basis.

MASS BASIS/TIME BASIS

Most of the standards have shown procedures for taking increments on a random basis or on a systematic basis. The random basis, provided it is done within fixed strata, has some advantage in countering systematic errors due to periodic quality variation. It is usually not applicable in mechanical sampling situations because of the additional capacity required to handle the "bunching" of increments. Therefore most sampling involves the taking of increments at regular intervals, usually on a time basis. However this is not applicable in systems which are subject to tonnage surges or show a wide variation in feed rate. In these cases the increments should be taken at uniform mass intervals.

CO-ORDINATION OF STANDARDS

Standards for the sampling of mineral commodities other than coal have likewise developed in a variety of directions. Although there may be some special requirements for each commodity there is no fundamental reason why the basic principles and equipment should be different. The need to "standardise" standards has been clearly recognised by the groups responsible for the development of sampling standards for coal and coke, iron ores, aluminium ores, heavy mineral sands, copper, lead and zinc ores and concentrates, ferroalloys, oil shales and alumina in Australia.

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3 Cawarra Place
GORDON, N.S.W. 2072. AUSTRALIA

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The Love of Numbers*

JOHN H. LOXTON

It is now well known that the answer to the ultimate question of life, the universe and everything is 42. (See note 1.) So we see that numbers are the fundamental elements of civilisation as we know it. Numbers such as telephone numbers and car licenses serve to whip our activities into some sort of order. Numbers are turned to good account by the Gas Board and the Taxation Office. Numbers, especially big round ones, fuel the arguments of economists and politicians. Numbers have mystical properties: 7 is a nice friendly number, while 13 is an unlucky one, particularly on Fridays. Although we would not rationally except to get anything significant by adding Margaret Thatcher's telephone number to Bob Hawke's this is still a very popular method of prophecy. For example, in the prophecy of Isaiah, the lion proclaims the fall of Babylon because the numerical equivalents of the Hebrew words for "lion" and "Babylon" have the same sum. (See note 2.) Numbers are ubiquitous. All this was much more pithily expressed by Leopold Kronecker in 1880: "God created the integers - all else is the work of man".

Mathematics is the numbers game par excellence. This is not to say that mathematicians are better than anybody else at reconciling their bank statements. In fact, Isaac Newton, who was Master of the Mint, employed a book-keeper to do his sums. Rather, mathematicians are the keepers of the odd numbers and the even numbers (see note 3), the square numbers and perfect numbers, the complex numbers and the irrational numbers. So there has grown up the study of the theory of numbers. Ever since Pythagoras, mathematicians have been intrigued, delighted and frustrated by the wonderful world of numbers. To Pythagoras, numbers were the absolute and ultimate foundation of nature, the source of music and, through the music of the spheres, the explanation for the motion of the planets. (See note 4.) Some of the most inspired work of such great mathematicians as Euler, Lagrange, Gauss, Dirichlet and their successors has dealt with the theory of numbers. However, in these more rational times, such elegant pursuits have seemed to some to be merely dilletantism. Fourier was one such critic. Jacobi, in 1840, complained that "Fourier had the opinion that the principal object of mathematics was public use and the explanation of natural phenomena; but a philosopher like him ought to know that the sole object of the science is the honour of the human spirit and that under this view a problem of the theory of numbers is worth as much as a problem on the system of the world".

The most intriguing, delightful and frustrating of all numbers are the prime numbers. Prime numbers are numbers which cannot be factorised as a product of smaller numbers. (See note 5.) Thus the first few prime numbers are

2, 3, 5, 7, 11, 13, 17, 19, 23, 29, 31, 37, 41, 43, 47, 53, 59, 61, ...

The prime numbers are the atoms of arithmetic because, as the ancient Greeks knew, every number can be factorised uniquely as a product of prime numbers. For example

$$666 = (2 \times 333 = 2 \times 3 \times 111 =)\ 2 \times 3 \times 3 \times 37.$$

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* Presidential Address delivered before the Royal Society of New South Wales on the 2nd of April, 1986.

There are infinitely many primes. This also was discovered by the Greeks and the argument is simple, elegant and compelling. Let 2, 3, 5, ..., P be a list of all the primes up to some particular prime P. Consider the number

$$N = 2 \times 3 \times 5 \times \ldots \times P + 1.$$

This number is not divisible by 2; in fact, we are left with a remainder of 1 after dividing by 2. In the same way, N is not divisible by 3, or by 5, or by any of the primes up to P. However, N is divisible by some prime which might be N itself if N is prime. This prime is different from any of the primes 2, 3, 5, ..., P, and so is greater than P. Consequently, the series of primes never comes to an end. Of course, only a finite number of primes have ever been seen, but some of them are pretty big. These big primes are all Mersenne primes, that is primes which are 1 less than a power of 2. (See note 6.) It so happens that there is a very efficient method of testing whether $2^n - 1$ is prime, the amount of calculation being proportional to n^3. The known Mersenne primes are listed in Table 1 with their discoverers and computation times. They serve to underline the spectacular growth in computing power in recent years. Lehmer's primes were found on the first generation of electronic computers, while Slowinski's monsters have been found on the Cray supercomputer. Since n has increased by a factor of about 400 in this time, the amount of calculation has increased by a factor of about $400^3 = 64 \times 10^6$, so we might conclude that today's computers are about a million times faster than the early machines.

To our present knowledge, the detailed behaviour of the prime numbers is unpredictable. There is no useful formula for calculating the n-th prime. This is part of the fascination of the search for bigger and bigger primes. In fact, we have no certain knowledge about where the next Mersenne prime will appear or even if there are any more of them at all. There is more suspense in the prime numbers than in the whole of the "Hitch-Hikers Guide to the Galaxy". (Note 1.)

On the other hand, the distribution of the primes exhibits stunning regularity. This can be seen in the behaviour of the function $\pi(x)$ which counts the number of primes up to x. As Table 2 reveals, $\pi(x)$ is approximately $x/\log x$. To put it another way, the probability that a randomly chosen number n is prime is about $1/\log n$. This is the famous prime number theorem. It was discovered experimentally around 1800 by Gauss and Legendre and eventually proved in 1896 by Hadamard

Table 1. Mersenne primes

Value of n for which $2^n - 1$ is prime	When proved to be prime	Discoverer	Computation time
2, 3, 5, 7	antiquity	mentioned by Euclid	
13	1461	in codex Lat. Monac. 14908	
17, 19	1588	Pietro Antonio Cataldi	
31	1772	Euler	
61, 89, 107, 127	1876	Lucas	
521, 607, 1279, 2203, 2281	1952	Lehmer, Robinson	1 min - 1 hr
3217	1957	Riesel	$5\frac{1}{2}$ hrs
4253, 4423	1961	Hurwitz	50 min
9689, 9941, 11213	1963	Gillies	2 hrs
19937	1971	Tuckerman	35 min
21701, 23209	1978	Noll, Nickel	8 hrs
44497	1979	Slowinski, Nelson	8 min
86243	1983	Slowinski	
132049	1984	Slowinski	
216091	1985	Slowinski	

Table 2. Values of $\pi(x)$

x	$\pi(x)$	$\pi(x)/\frac{x}{\log x}$
10	4	0.92
100	25	1.15
1 000	168	1.15
10 000	1 229	1.14
100 000	9 592	1.11
1 000 000	78 498	1.09
10 000 000	664 579	1.08
100 000 000	5 761 455	1.06
1 000 000 000	50 847 534	1.05
10 000 000 000	455 052 512	1.04

and de la Vallée Poussin with the aid of new and powerful analytic methods. However, $x/\log x$ is only a reasonably good approximation to $\pi(x)$ and it is natural to ask for a better one. This question was explored by Riemann in 1860 and he saw that the prime numbers are intimately connected with a function now called the Riemann zeta function. Riemann obtained what is essentially an exact formula for $\pi(x)$ in terms of the zeros of his zeta function. It is therefore crucial to know where the zeros of the zeta function are. This is what the infamous Riemann Hypothesis does. (See note 7.) The Riemann Hypothesis is supported by an impressive amount of experimental evidence. For example, if we make a list of the zeros of the zeta function, we find that the first 200 million zeros or so are exactly where Riemann predicted However, there are infinitely many zeros and every one of them is important, so the ultimate answer to the question of the distribution of the prime numbers is still a long way off. The story continues : the latest attempts to settle the Riemann Hypothersis uncovered connections between prime numbers and quantum electrodynamics.

What then are we to make of this theory of numbers? It seems to be a very private science and there are those who will ask why the serious study of these matters is really worth-while. I have tried to illustrate how the theory of numbers captures the essence of mathematics : beauty, inevitability, unexpectedness and depth. Here the interplay of ideas is at its most dazzling. For this reason, the work of the great men of this subject is permanent. For example, Riemann wrote only one paper on the theory of numbers and the Riemann Hypothesis is not much more than a throw-away line, but it will always be the Riemann Hypothesis even after it is settled. There is a further twist to this argument. The theory of numbers is harmless. The point is beautifully made in "A mathematician's apology" by G.H. Hardy. (See note 8.) "A good deal of elementary mathematics, which includes, for example, a fair working knowledge of the differential and integral calculus, has considerable practical utility. These parts of mathematics are, on the whole, rather dull; they are just the parts which have the least aesthetic value. The real mathematics of the real mathematicians, the mathematics of Fermat and Euler and Gauss and Abel and Riemann, is almost wholly useless. Mathematicians may be justified in rejoicing that there is one science at any rate, and that their own, whose very remoteness from ordinary human activities should keep it gentle and clean. It is the dull and elementary parts of mathematics that work for good or ill. Real mathematics has no effects on war. No one has yet discovered any warlike purpose to be served by the theory of numbers or relativity and it seems very unlikely that anyone will do so for many years". Despite its contradictions, it seems to me that this sums up the motives of real mathematicians. But the world is a funny place and, sooner or later, much real mathematics finds a public use.

In recent times, the hazards of space travel and electronic banking have spawned exciting developments in secret codes. Much of this involves the theory of numbers. The work is so important that the Defence Establishment in the United States made strenuous efforts to have the study of prime numbers classified. One species of code is the error-correcting code. Signals from the depths of space, say, are transmitted as strings of zeros and ones. When a 0 is sent, usually a 0 is received, but occasionally, because of noise, a 1 is received instead. A 1 is usually received as a 1, but occasionally as a 0. We could guard against these occasional errors by sending each symbol several times, but this is very inefficient. Space satellites and compact disc players use subtle error-correcting codes which rely on ideas from algebraic number theory

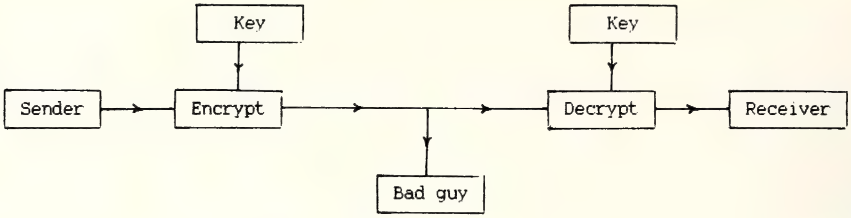

Figure 1. Conventional cryptography.

and geometry. Another species of code is the public-key code which makes it possible to send signed electronic mail. This is what I will try to describe next. (See note 9.)

Conventional cryptography makes use of a secret key known to the sender and the receiver, but not to the "bad guy". (See Figure 1.) This is the stuff of trench warfare and spy stories. The main problem is to exchange a sufficient amount of key between sender and receiver and to keep it secret, because once the key is compromised at either end the "bad guy" can read the message. Public-key cryptography, by contrast, looks like this. (See Figure 2.) The code requires two different keys, one for encrypting and one for decrypting and depends on the fact that knowledge of the encrypting key is no help in decrypting. The mechanism is a trap-door function. This is a function E for encryption

$$\text{message } X \xrightarrow{E} \text{cipher } E(X)$$

which has an inverse D for decryption

$$\text{cipher } E(X) \xrightarrow{D} \text{message } D(E(X)) = X,$$

with the essential property that the inverse D cannot be discovered by studying E. So E is a trapdoor through which messages vanish and they can only be recovered by a different route with the aid of the special key D. If we have a supply of one-way functions, we can set up a cryto-net. Suppose a group of people wish to talk to each other privately. Each person i chooses a trapdoor function E_i with an inverse D_i. The functions E_1, E_2, ... are listed in a public directory, while each person keeps his inverse function D_i secret. When i wants to send a message X to j, he encrypts the message X as

$$Y = E_j(D_i(X))$$

and sends it to j. Now j can recover the message by calculating

$$E_i(D_j(Y)) = E_i(D_j(E_j(D_i(X)))) = E_i(D_i(X)) = X.$$

Only j can read the message because only j knows D_j. This ensures privacy. Moreover, only i could have sent the message because only i knows D_i, so the scheme provides authentication as well. (See note 10.)

Do these marvellous trap-door functions exist? They are, of course, logically impossible and it will therefore take a little time to construct one. The first ingredient is the modular arithmetic

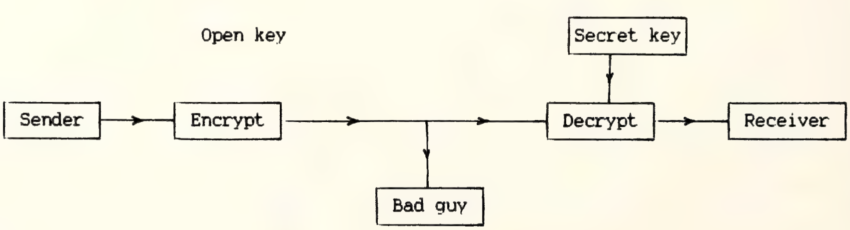

Figure 2. Public-key cryptography.

invented by Gauss around 1800. If a number n when divided by the modulus m leaves the remainder r, we say n is congruent to r modulo m and write $n \equiv r (\mathrm{mod}\ m)$. This means that m divides $n - r$ exactly with no remainder. For example, $37 \equiv 1 \pmod 9$ because $37 = 4 \times 9 + 1$; $37 \equiv 4 \pmod{11}$ because $37 = 3 \times 11 + 4$. The notation is meant to suggest that arithmetic works with these remainders according to the usual rules. For example,

$$37 \times 41 = 1517$$

can be written as $(3 \times 11 + 4) \times (3 \times 11 + 8) = (137 \times 11 + 10)$ and on taking remainders modulo 11 this yields the correct equation

$$4 \times 8 \equiv 10 \pmod{11}.$$

Arithmetic modulo 9 makes a useful check on computation. For example,

$$123456789 \times 987654321 \not\equiv 121932631212635269$$

because the left side is $0 \times 0 \pmod 9$ and the right side is $1 \pmod 9$ as you may easily check. (See note 11.)

The second ingredient is Fermat's little theorem. (See note 12.) Fermat discovered in 1640 that if p is a prime and b is any integer not divisible by p, then

$$b^{p-1} \equiv 1 \pmod p,$$

that is $b^{p-1} - 1$ is exactly divisible by p. For example, $2^6 - 1 = 63$ is indeed divisible by 7, that is $2^6 \equiv 1 \pmod 7$. On the other hand, $2^{322} \equiv 123 \pmod{323}$ and we see that 323 cannot be a prime. This is the prototype for the fast methods of testing for primality. The proof of Fermat's little theorem is another gem of the theory of numbers. Observe that the possible remainders modulo p are $0,1,2, \ldots, p - 1$. The numbers $b, 2b, 3b, \ldots, (p - 1)b$ have distinct non-zero remainders modulo p, so these remainders must be $1,2, \ldots, p - 1$ in some mixed up order. If we multiply these two sets of numbers together, we must get the same result modulo p, that is

$$b.2b.3b.\ldots.(p - 1)b \equiv 1.2.3.\ldots.(p - 1) \pmod p.$$

Cancelling $1.2.3. \ldots (p - 1)$ on both sides gives $b^{p-1} \equiv 1 \pmod p$. Actually, we need a little more. If p and q are distinct primes and b is any integer not divisible by either prime, then

$$b^{(p-1)(q-1)} \equiv 1 \pmod{pq}.$$

For example, $323 = 17 \times 19$, $288 = 16 \times 18$ and $2^{288} \equiv 1 \pmod{323}$. This extension follows from Fermat's little theorem because

$$b^{(p-1)(q-1)} \equiv 1^{(q-1)} = 1 \pmod p,$$

i.e. p divides $b^{(p-1)(q-1)} - 1$, and

$$b^{(p-1)(q-1)} \equiv 1^{(p-1)} = 1 \pmod q,$$

i.e. q divides $b^{(p-1)(q-1)} - 1$,

whence pq divides $b^{(p-1)(q-1)} - 1$ as required.

Here is the trapdoor function devised by Rivest, Shamir and Adleman in 1978. Choose two large secret prime numbers p and q about 50 digits long. Their product $r = pq$ is the encypting modulus. Also choose the encrypting exponent s, making sure that s has no common factor with either $p - 1$ or $q - 1$. The numbers r and s define the public key and go in the telephone book of encrypting functions. Before encryption, a written message is converted into a string of digits, say by A = 01, B = 02, C = 03, ... We break the resulting string into blocks of 100 digits. Each block X is then encrypted by the function

$$E(X) \equiv X^s \pmod r.$$

To decrypt, find the decrypting exponent t by solving the congruence $st \equiv 1 (\mathrm{mod}(p - 1)(q - 1))$ and then use the decrypting function D given by

$$D(Y) \equiv y^t \pmod r.$$

This works because $st = 1 + k(p - 1)(q - 1)$ and so

$$D(E(X)) \equiv E(X)^t \equiv X^{st} = X^{1+k(p-1)(q-1)}$$
$$= X.(X^{(p-1)(q-1)k} \equiv X (\mathrm{mod}\ r).$$

(The last step depends on $X^{(p-1)(q-1)} \equiv 1$ (mod

pq).) For a simple and therefore utterly useless example, take $r = 187 = 11 \times 17$ and $s = 7$. The message $X = 003$ is encrypted as

$$Y \equiv X^s = 3^7 = 2187 \equiv 130 \pmod{187}.$$

The decrypting exponent comes from solving $7t \equiv 1 \pmod{160}$ which gives $t = 23$. So we can decrypt $Y = 130$ by calculating

$$Y^t = 130^{23} \equiv 3 \pmod{187}.$$

Despite initial appearances, all these calculations are very easy on a computer. The point to notice is that this really is a trapdoor because the decrypting exponent cannot be calculated from the public information r and s. We must factorise $r = pq$ first before we can calculate t. (See note 13.) But r is a 100 digit number and it appears to be essentially impossible to factorise such large numbers. The strength of the scheme depends on the fact that it is easy to find large primes but it is very difficult at the moment to factorise large numbers. However, there is no guarantee that some one will not invent a revolutionary method of factorising tomorrow and unhinge this trapdoor in the process. There is now more incentive for trying to understand the mysteries of the primes than ever.

I have tried to defend my science by appealing to public use and base motives. I have also tried to illustrate the private face, the incorruptible beauty and fascination of the subject. I have neither the time nor the knowledge to explain how the ideas of the theory of numbers permeate the whole of science. Consider this Platonic dialogue (note 14):

Corbeiller: "In the last 60 years, however, a new revolution has taken place, and everywhere we look we find that what seems to be continuous is really composed of atoms."

Empeiros: "But are not modern mathematicians interested in such things?"

Corbeiller: "They are, but they give them other names. They call them Number Theory and the Theory of Discontinuous Groups. Actually, they have found much more than we can use yet in physics, but we have in crystals illustrations of some of their simpler theorems".

Again, here is the great Russian mathematician Yu. I. Manin (see note 15):

"It is remarkable that the deepest ideas of number theory reveal a far-reaching resemblance to the ideas of modern theoretical physics. Like quantum mechanics, the theory of numbers furnishes completely non-obvious patterns of relationship between the continuous and the discrete (the technique of Dirichlet series and trigonometric sums, p-adic numbers, non-archimedean analysis) and emphasises the role of hidden symmetries (class-field theory, which describes the relationship between prime numbers and the Galois groups of algebraic number fields). One would like to hope that this resemblance is no accident, and that we are already learning new words about the World in which we live, but we do not yet understand their meaning".

Numbers, then, are the key to knowledge. The last word, as is only proper, belongs to Winston Churchill, who saw the great importance of this subject and how he might have been a really great man but for one small thing:

"I had a feeling once about Mathematics - that I saw it all. Depth beyond Depth was revealed to me - the Byss and the Abyss. I saw - as one might see the transit of Venus or even the Lord Mayor's Show - a quantity passing infinity and changing its sign from plus to minus. I saw exactly why it happened and why the tergiversation was inevitable - but it was after dinner and I let it go". (See note 16.)

Notes

1. D. Adams, "The Hitch-Hikers Guide to the Galaxy". (Pan, 1979). The apotheosis of Deep Thought is revealed in chapter 27.
2. Isaiah 21 : 8, 9.
3. No mathematician could have been so crass as the Minister in the New South Wales Government who declared during a petrol strike that, for the purposes of petrol rationing, car number plates ending in 0 were to be considered even.
4. For a modern opinion, see D. Adams, "Life, the universe and everything" (Pan, 1982, chapter 7). "It is now realised that numbers are not

absolute, but depend on the observer's movement in restaurants".

5. By convention and convenience, 1 is not a prime. So the Prime Minister, despite his ego, is not prime. Neither, in most cases, are prime ribs of beef.

6. They are named after Marin Mersenne who corresponded with mathematicians of the day, provoking them with his execrable hand-writing and conjectures in number theory. In 1644, he gave a list of the Mersenne primes up to 10^{79} which was only correct up to 10^{18}, so he is perhaps fortunate to be commemorated in this way.

7. The Riemann zeta function is defined by the formula

$$\zeta(s) = 1^{-s} + 2^{-s} + 3^{-s} + \dots$$

Here $s = \sigma + it$ is a complex variable. The Riemann Hypothesis asserts that the solutions of the equation $\zeta(\sigma + it) = 0$ all have $\sigma = \frac{1}{2}$ (except for "trivial" zeros of ζ at $-2, -4, -6, \dots$).

8. It is worth noting that this delightful little book was written in 1940 and that its rhetoric is the distillation of years of high table conversation in Oxbridge Colleges.

9. The prehistory of cryptography up to 1967 is described breathlessly in "The Codebreakers" by David Kahn (Macmillan, 1967). For later developments, see the article by N.J.A. Sloane on "Error-correcting codes and cryptography" in "The mathematical Gardner", edited by D.A. Klarner (Wadsworth, 1981).

10. Devise a protocol by which two potentially dishonest players can play a fair game of poker without using any cards, for example, over the telephone. See "Mental Poker" by Shamir, Rivest and Adleman in "The Mathematical Gardner" (ibid).

11. This trick called "casting out nines" is almost obsolete now. Should you have forgotten your 987654321 times tables, all you need recall is that $987654321 \times 81 = 80000000001$. So multiply 123456789 by 80000000001 and divide the result by 81 to get 121932631112635269.

12. Not to be confused with the infamous Fermat's last theorem which is neither a theorem nor the last thing he did and which there is no space to explain here.

13. At least, this is the current position. If s is chosen carefully, the only way to crack the code is to solve the congruence for the decrypting exponent. It is perfectly possible that someone will discover a better method. This is exactly what happened recently to another "trapdoor" based on the knapsack problem. None of the modern schemes for cryptography have been proved to be secure.

14. Philippe Le Corbeiller, "Crystals and the future of physics", Scientific American, January 1953, 50-56.

15. Yu. I. Manin, "Mathematics and Physics", translated by Ann and Neal Koblitz (Birkhauser, 1981).

16. Quoted in "The Mathematical Magpie" by C. Fadiman (Simon and Schuster, 1962, page 255).

School of Mathematics,
University of New South Wales,
KENSINGTON, NSW, 2033,
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The Volatile Leaf Oils of Some Central Australian Species of *Eucalyptus*

JOSEPH J. BROPHY AND ERICH V. LASSAK

ABSTRACT. The volatile leaf oils of *Eucalyptus intertexta* var. *fruticosa*, *E. lucens*, *E. mannensis*, *E. normantonensis*, *E. ochrophloia*, *E. orbifolia*, *E. pachyphylla* and *E. sparsa* have been examined by means of a combination of capillary gas liquid chromatography and mass spectrometry. All oils, with the exception of *E. ochrophloia* which contains significant amounts of sesquiterpenoids, are monoterpenoid in nature and contain 1,8-cineole as their main component. The flavonoid glycoside rutin has been isolated from *E. pachyphylla* foliage.

INTRODUCTION

In continuation of our work on the volatile oils of the genus *Eucalyptus* (family Myrtaceae) we have now examined the leaf oils of the following central Australian species : *E. intertexta* R.T. Baker var. *fruticosa* (a minor variant of *E. intertexta*), *E. lucens* Brooker and Dunlop, *E. mannensis* Boomsma, *E. normantonensis* Maiden et Cambage (which also extends into the Gulf country of northern Queensland), *E. ochrophloia* F. Muell.,*E. orbifolia* F. Muell., *E. pachyphylla* F. Muell. and the recently described and rare *E. sparsa* Boomsma.

With the exception of *E. intertexta*, the leaf oil of which has been reported to contain cineole (35%), α-pinene and unidentified sesquiterpenes (Baker and Smith, 1920), none of the other seven species has been previously chemically examined.

E. sparsa appears to be a problem species. Whilst Boomsma (1979) referred it to the Pryor and Johnson series *Largiflorentes* (section *Adnataria*, subgenus *Symphyomyrtus*) there are reasons for placing it in a separate series created for it alone within the *Adnataria* (L.A.S. Johnson, personal communication.)

RESULTS AND DISCUSSION

The steam distilled leaf oils of all species, except *E. ochrophloia*, were almost entirely monoterpenoid and characterized by high to very high 1,8-cineole contents. The oil of *E. ochrophloia* contained, besides some 40% of 1,8-cineole, a significant proportion of the sesquiterpenoid alcohols α-, β- and γ-eudesmols. Several trace components of some of these oils, such as *cis*-3-pinen-2-ol (I), an isomer of rose oxide (II), α-campholenic aldehyde (III) and ar-curcumene (IV) have not, to our knowledge, been previously reported from the genus *Eucalyptus*.

The occurrence of α-campholenic aldehyde (III) in the oils of *E. sparsa* and *E. lucens* may not be surprising in view of the presence of small amounts of camphor (V) in both oils. Ciamician and Silber (1910) have shown that exposure of an aqueous/alcoholic solution of camphor to sunlight results in the formation of some α-campholenic aldehyde. Similarly, the presence of trace amounts of *cis*-3-pinen-2-ol, pinocarvone, pinocamphone and verbenone may be caused by sunlight initiated free radical oxidation of α- and β-pinene.

Insufficient chemical data are available on the essential oils of the eucalypts of the section *Adnataria* to allow any really meaningful suggestions about the placing of *E. sparsa* based on chemical grounds. However, what little information is available supports Johnson's suggestion that it does not belong to the *Largiflorentes*. The 1,8-cineole content of the leaf oils of the four species investigated in this group is only moderately high: 57-62% in *E. populnea*, 48% in *E. largiflorens*, 45% in *E. behricha* (Baker and Smith, 1920) and 75% in *E. normantonensis*, reported here for the first time, and is much less than the very high level of 90% found in *E. sparsa*. The highest levels of 1,8-cineole in this section are encountered in the series *Odoratae*, subseries *Odoratinae*: 63% in *E. odorata*, 80% in *E. viridis* and 77-84% in *E. polybractea* (Baker and Smith, 1920). More recent work on *E. polybractea* has shown that individual trees may yield oils with 1,8-cineole contents of the order of 90% (E.V. Lassak, unpublished results).

Brooker and Dunlop (1978) placed *E. lucens* in the series *Polyanthemae* (section *Adnataria*) whilst noting that it did not, at least superficially, resemble any other species in this series. Our results appear to support their observation. The low to medium high 1,8-cineole contents of the four species belonging to this series investigated by Baker and Smith (1920), *E. conica* (35%), *E. bauerana* (5-10%), *E. polyanthemos* (45-54%) and *E. fasciculosa* (less than 15%) as well as the presence of substantial amounts of sesquiterpenes in all four of them are significantly different from the cineole rich (74.5%) and sesquiterpenoid poor oil of *E. lucens*.

E. mannensis, considered by Boomsma (1964) to be close to *E. oleosa*, was placed by Pryor and Johnson (1971) closer to *E. jutsonii* and *E. bakeri* in their series *Bakeranae* (rather than in the series *Oleosae* next to *E. oleosa*). Chemical considerations are of little help since 1,8-cineole concentrations in the leaf oils of both series are all relatively high and of comparable magnitude. The absence of *p*-isopropylphenol and 2,4-dimethoxy-6-hydroxyacetophenone in *E. mannensis* leaf oil, compounds identified by Penfold (1927) in *E. bakeri* leaf oil, does not necessarily imply closeness to *E. oleosa* since Marshall and Watson (1936/7) reported the presence of unidentified phenolic compounds in the leaf oil of the latter.

The percentage leaf oil compositions of all oils investigated are presented in Table 1.

The flavonoid glycoside rutin has been isolated in small yield from the still waters of *E. pachyphylla* distillation. Most previous occurrences of rutin have been recorded from species belonging to the subgenus *Monocalyptus*, e.g. *E. macrorhyncha* ssp. *macrorhyncha* and *E. youmanii* (Rodwell, 1950), *E. alpina*, *E. blaxlandii*, *E. baxteri*, *E. caliginosa*, *E. delegatensis* and *E. macrorhyncha* ssp. *cannonii* (Humphreys, 1964), *E. deuaensis* (Boland et al., 1986) and *E. dives* (E.V. Lassak, unpublished results). Hillis and Isoi (1966) have reported rutin from the foliage of *E. sideroxylon* and *E. pachyphylla* represents, therefore, only the second instance of a species belonging to the Pryor and Johnson subgenus *Symphyomyrtus* where rutin has been positively identified.

EXPERIMENTAL

Collection of Plant Material and *Isolation of Volatile Oils.*

The foliage, packed in loosely woven canvas bags and already partly dry when received, was spread out on sheets of newspaper and allowed to fully air-dry protected from direct sunlight. After removal of all twigs the dry leaves (400g in each case) were cohobatively steam distilled in an all-

Table 1. % Composition of *Eucalyptus* leaf oils

Peak no.	Compound	*E. intertexta* var. *fruticosa*	*E. lucens*	*E. mannensis*	*E. normantonensis*	*E. ochrophloia*	*E. orbifolia*	*E. pachyphylla*	*E. sparsa*	Method of identification
1	3-methylbutanal	-	0.4	tr	0.5	tr	4.2	0.5	0.1	MS, co-GLC
2	α-pinene	9.5	5.4	9.7	6.9	6.8	-	8.2	1.2	MS, co-GLC
3	camphene	-	0.1	tr	0.1	0.1	-	tr	-	MS, co-GLC
4	β-pinene	0.1	tr	2.4	tr	3.8	tr	tr	0.9	MS, co-GLC
5	sabinene	-	-	0.1	-	0.1	-	-	0.2	MS
6	myrcene	-	-	-	tr	-	-	-	0.1	MS, co-GLC
7	α-terpinene	tr	tr	0.5	0.1	0.1	-	0.1	tr	MS, co-GLC
8	α-phellandrene	tr	tr	tr	tr	0.1	-	0.1	-	MS, co-GLC
9	limonene	0.4	0.8	2.8	2.0	2.0	1.6	2.1	1.0	MS, co-GLC
10	1,8-cineole	75.1	74.5	80.0	75.4	40.7	74.3	68.1	90.0	MS, co-GLC IR
11	γ-terpinene	-	-	-	2.0	0.2	-	1.6	0.1	MS, co-GLC
12	β-*trans*-ocimene	-	0.2	0.6	1.2	0.2	-	3.0	-	co-GLC
13	*p*-cymene	2.3	0.6	1.5	1.3	2.0	12.7	5.2	1.9	MS, co-GLC
14	terpinolene	-	tr	tr	0.5	0.3	-	0.2	-	co-GLC
15	un	-	0.3	-	-	-	-	-	-	
16	rose oxide (?)	-	-	-	-	-	-	-	tr	MS
17	α, *p*-dimethylstyrene	-	tr	-	0.1	0.1	-	0.3	tr	MS
18	α-campholenic aldehyde	-	tr	-	-	-	-	-	tr	MS
19	pinocarvone + camphor	0.8	3.5	0.9	1.0	0.2	-	0.4	0.2	MS, co-GLC
20	pinocamphone	-	0.2	tr	0.1	0.1	-	tr	0.2	MS
21	terpinen-4-ol	0.3	tr	0.7	0.3	0.5	-	0.8	0.4	MS, co-GLC
22	*allo*aromadendrene (?)	0.4	-	tr	tr	0.7	0.7	0.7	0.1	MS
23	myrtenal	-	-	tr	tr	-	-	-	0.2	MS
24	*trans*-pinocarveol	6.5	10.7	tr	2.1	0.6	-	1.9	0.3	MS, co-GLC
25	δ-terpineol	-	tr	tr	-	tr	0.1	-	0.1	MS
26	*cis*-3-pinen-2-ol	-	-	-	-	-	-	-	0.1	MS
27	$C_{10}H_{16}O$	-	tr	tr	tr	7.4	-	1.0	tr	MS
28	α-terpineol	0.6	0.8	0.3	0.7	2.1	0.8	1.3	0.3	MS, co-GLC
29	borneol	-	tr	-	0.2	0.5	-	-	tr	co-GLC
30	verbenone	-	tr	-	0.1	0.2	-	-	0.3	MS, co-GLC
31	piperitone	-	tr	-	-	1.2	tr	-	tr	MS, co-GLC
32	carvone (?)	-	-	-	-	-	-	-	0.3	MS
33	ar-curcumene	-	-	-	tr	0.2	tr	-	0.3	MS
34	citronellol	-	0.8	tr	0.1	1.4	tr	-	0.1	MS
35	mentha-1(7),8-dien-2-ol	-	-	-	-	tr	-	-	0.1	MS
36	myrtenol	-	tr	tr	tr	0.2	-	-	0.2	MS
37	*p*-cymen-8-ol	-	-	tr	tr	0.2	0.1	0.4	0.5	MS
38	un	-	0.4	-	-	-	-	tr	-	
39	globulol	2.0	0.6	-	0.1	1.2	0.1	2.5	0.2	MS, co-GLC
40	$C_{15}H_{26}O$ (alcohol)	-	-	-	-	tr	-	-	tr	MS
41	un	0.7	-	-	0.1	tr	-	-	-	
42	γ-eudesmol	-	-	-	0.1	6.5	-	-	-	MS, co-GLC
43	un	0.3	-	-	1.8	0.5	-	-	-	
44	un	-	-	-	0.2	0.5	3.4	-	-	
45	α-eudesmol	-	-	-	0.1	4.2	-	-	-	co-GLC
46	β eudesmol	-	-	-	0.1	13.2	-	-	-	co-GLC
47	un	-	-	-	0.9	0.3	-	-	-	

tr: less than 0.1%

un: unknown

glass apparatus as described previously (Lassak, 1979) to yield pale yellow oils (Table 2.) Botanical voucher specimens are lodged at the Northern Territory Herbarium, Arid Zone Research Institute, Alice Springs.

Species	Locality	% Oil yield V/W (dry wt.)	n_D^{20}	α_D	$d_{20^o/4^o}$
E. intertexta var. *fruticosa*	White Range near Arltunga, N.T.	1.54	1.4702	-2.8^o	0.9374
E. lucens	Areyongo, N.T.	0.50	1.4644	n.d.	n.d.
E. mannensis	42 km south of Alice Springs, N.T.	1.48	1.4605	$+0.8^o$	0.9208
E. normantonensis	Elkedra Station, Davenport Ranges, N.T.	1.23	1.4702	$+1.6^o$	0.9278
E. ochrophloia	The Gardens Homestead, N.T.	4.27	1.4788	$+12.0^o$	0.9410
E. orbifolia	Chewing's Range, near Serpentine Gorge, west of Alice Springs, N.T.	1.73	1.4689	$+1.0^o$	0.9258
E. pachyphylla	Soakage Bore, Utopia, N.T.	0.57	1.4704	n.d.	n.d.
E. sparsa	45 km south of Pipalyatjara, N.T.	2.54	1.4606	$+0.7^o$	0.9253

N.D. : not determined owing to insufficient oil sample

Identification of Essential Oil Constituents

Analytical GLC was conducted on a Perkin Elmer 900 gas chromatograph using 15m by 0.5mm i.d. stainless steel FFAP coated SCOT columns with He as carrier gas. Individual runs were programmed from 80^oC to 170^oC at 6^oC/min following an initial holding period of 9 min at 80^oC. A Hewlett Packard 3370A electronic integrator was used to determine percentage compositions. Individual components were identified by their retention times and, where possible, by co-injection with authentic compounds.

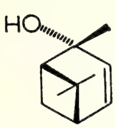
GLC/MS was conducted as described previously (Brophy *et al.*, 1985).

The identity of 1,8-cineole was also confirmed by its infrared spectrum.

Isolation of Rutin

The still water from *E. pachyphylla* distillation deposited on cooling overnight pale yellow crystalline aggregates of crude rutin. After filtration and two recrystallizations from boiling water pure rutin was obtained as minute pale yellow needles (1.7g; 0.43% on air dried foliage). Its infrared

(nujol mull) and ultraviolet spectra were superimposable upon those of authentic rutin.



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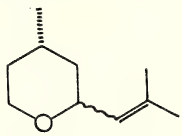

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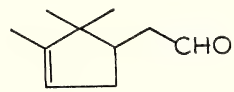

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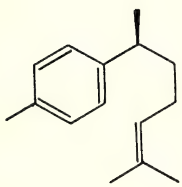

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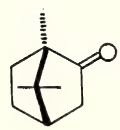

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Department of Organic Chemistry,
University of New South Wales,
Kensington, N.S.W., 2033 Australia.

Joseph J. Brophy,

School of Technology,
Kalgoorlie College,
PMB 22,
Kalgoorlie, WA, 6430, Australia.

Erich V. Lassak

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Meson Source Densities For Excited States of the Nucleon

J. L. Cook and E. K. Rose

ABSTRACT. A relationship is established between effective range theory and the reaction matrix. Pion-nucleon scattering, excited state single level meson source densities and pion wave functions are then given for two reference sets of low energy phase shift data, fitted to the multilevel reaction matrix. The resultant P11 nucleon ground state wave function reproduces the nucleon form factor and gives the correct position and residue for the nucleon pole.

INTRODUCTION

A recent paper by Hartt (1980) deals with the success of inverse scattering theory, using the approach that the effective range function k cotα, where k is the momentum in the centre of mass (CM) system and α the scattering phase shift, is a Padé approximant. Known analytic properties of such rational functions permit the evaluation of local Bargmann (1949) potentials. Hartt comprehensively reviews the literature on this problem for both the forward and inverse situation and examines the application of his method to N-N scattering.

An inverse calculational scheme (Cook, 1972) was based on an entirely different approach. It was shown that the historical concept of the reaction matrix led to the direct construction of a non-local potential, which had a variety of possible forms, thus leading to the verification of the existence of phase equivalent solutions. However, one particular representation for the quantity known as the overlap matrix gave especially simple solutions. The advantage of this method is that it can be used readily for all partial waves and not just s-waves, as in the Hartt technique, where the higher partial waves are difficult to treat. Furthermore, one deals with Padé coefficients that are exactly related to resonance parameters, whereas Hartt's coefficients have no such simple interpretation. Clayton, Cook and Rose (1977) later used the phase shifts of Roper, Wright and Feld (1965) to solve the inverse problem for the low energy pion-nucleon scattering data and gave phase equivalent local and non-local potentials. These data were employed by Rose, Cook and Clayton (1979) to extrapolate back to the nucleon ground state configuration where the potential fields and matter probability densities for the nucleon interior region were evaluated.

In this paper we begin by defining the effective range function for finite range forces as an exact relationship. Thus all such Padé approximants, which in current theories are only defined close to the reaction threshold, must necessarily sum to give this exact solution. This demonstrates the relationship between the Hartt and our theories. It also defines very simple connections between the scattering lengths in each partial wave and the reaction matrix evaluated at zero momentum. We give the Padé approximant to second order and show the key role that the reaction matrix plays in such expansions.

The next section begins by giving the three possible representations for an interaction, namely the source technique, the energy dependent local potential (which we refer to as the multi-level potential since it takes account of resonance overlap), and the single level local potential, which always reproduces the single level Breit-Wigner shape. Since Clayton et al. (1977) have exhaustively examined the source as a function of energy, we give the source terms corresponding to the single level potential.

The third section deals with the results of obtaining consistent data sets to fit reaction matrices by the method of Padé approximants. Originally, we chose the Roper et al. (1965) set mainly because they used statistically smoothed fits to the data. Our method of fitting the reaction matrix by Padé approximants is sensitive to statistical fluctuations and the fits often reproduce spurious poles to fit points showing a large statistical deviation. More recently, we have fitted data from Almehed and Lovelace (1972), Carter, Bugg and Carter (1973), Nielsen and Oades (1974) and Rowe, Salomon and Landau (1978). Because our interest lies just within the low energy scattering region, we selected the last two of these sets; however, we excluded the Rowe et al. (1978) P11 data for which we could find absolutely no evidence for the nucleon pole. Rowe et al. (1978) used an empirical approach in which the nucleon pole should have been included in the formula but was left out.

Next we deal with the results of our fits to the phase shifts and resonance parameters. It must be recalled that these resonance parameters are multi-level constants and not the single level values usually found in particle data compilations.

We hesitated for some time to speculate that these regions may be quark orbital clouds, since the linear forces between each quark makes it extremely difficult to see if such a structure can be dynamically stable.

EFFECTIVE RANGE THEORY

The connection between the effective range function and the reaction matrix is quite simple. We give the derived result for the tangent of the phase shift in all partial waves as

$$\tan \alpha_\ell = \frac{j_\ell(ka) - a\,\mathcal{R}_\ell(k) j'_\ell(ka)}{n_\ell(ka) - a\,\mathcal{R}_\ell(k) n'_\ell(ka)} \tag{1}$$

where

$$\mathcal{R}_\ell = \frac{\psi_\ell(a)}{a\psi'(a)}$$

is a reaction matrix, a = finite force range, $\psi_\ell(r)$ is the radial wave function of the interaction, the derivatives are with respect to radius, $j_\ell(ka)$ and $n_\ell(ka)$ are the spherical Bessel functions used by Cook (1972) and α_ℓ is the scattering phase shift. Now the reaction matrix used normally, which we call $R_\ell(k)$, where k is the CM momentum of the two-body system, is connected to $\mathcal{R}_\ell(k)$ by the relationship

$$\mathcal{R}_\ell = \frac{R_\ell}{1-\ell R_\ell} \quad . \tag{2}$$

Using this in equation (1) to find the phase shift, and employing the relationship between j_ℓ and n_ℓ, together with the recurrence relation for the spherical Bessel functions, we obtain the final result:

$$\tan \alpha_\ell = \frac{j_\ell(ka) - (ka) R_\ell(k) j_{\ell-1}(ka)}{n_\ell(ka) - (ka) R_\ell(k) n_{\ell-1}(ka)} \quad . \tag{3}$$

Note that if R_ℓ vanishes, only the hard core form for the phase shift remains. The function

$$f(k) = \frac{\tan \alpha_\ell(k)}{k^{2\ell+1}} \tag{4}$$

is defined as the effective range function; it can be seen immediately that equation (3) does not define a rational function or Padé approximant, but is an entire function of k. It only becomes a rational function when a Taylor series for the Bessel functions, applicable near threshold, is usable. The reaction matrix has the usual Wigner Eisenbud form:

$$R(k) = \sum_\lambda^N \frac{\gamma_\lambda^2}{k_\lambda^2 - k^2} + R_o \quad , \tag{5}$$

which is also a rational but very accurate approximation in normal circumstances. γ_λ^2 is the reduced width of the resonance, k_λ^2 is its position, and R_o is the contribution from distant resonances, taken as constant.

Using the Taylor expansions for the Bessel functions (Abramowitz and Stegun 1965; Preston 1965) we obtain the second order results as follows:

(a) s-Waves

$$\tan \alpha_o = \frac{kaR(k) j_{\ell-1}(ka) - j_\ell(ka)}{kaR(k) n_{\ell-1}(ka) - n_\ell(ka)} \tag{6}$$

leading to the result for small (ka) of

$$\frac{\tan \alpha_o}{k} \approx -\,a \left[\frac{(1-R) + (ka)^2\{-\frac{1}{6} + \frac{R}{2}\}}{1 - (ka)^2\{\frac{1}{2} - R\}} \right] \tag{7}$$

and a scattering length for s-wave denoted by

$$\beta_o = a \left(R_o(o)-1\right) \quad . \tag{8}$$

This equation relates scattering lengths to the tails of all resonances of the system. Note that equation (7) is a Padé approximant.

(b) p-Waves

The same theory applied to p-waves leads to the low energy result

$$\frac{\tan \alpha_1}{k^3} = a^3 \left[\frac{\frac{1}{3} - R - \frac{1}{30}(ka)^2 + \frac{1}{6}(ka)^2 R}{-1 - \frac{1}{2}(ka)^2 + (ka)^2 R - \frac{1}{2}(ka)^4 R} \right] \quad . \tag{9}$$

The p-wave scattering length is then

$$\beta_1 = a^3 \left(R(o) - \frac{1}{3}\right) \quad . \tag{10}$$

The general result for all partial waves is

$$\frac{\tan \alpha_\ell}{k^{2\ell+1}} = a^{2\ell+1} f_\ell(k) \tag{11}$$

where

$$f_\ell(k) = \frac{\frac{1}{(2\ell+1)!!}\left[1 - \frac{(ka)^2}{2(2\ell+3)}\right] - R_\ell(k) \frac{1}{(2\ell-1)!!}\left[1 - \frac{(ka)^2}{2(2\ell-1)}\right]}{-(2\ell-1)!!\left[1 - \frac{(ka)^2}{2(2\ell-1)}\right] - R_\ell(k)(ka)^2(2\ell-3)!!\left[1 - \frac{(ka)^2}{2(2\ell-3)}\right]} \tag{12}$$

$$\beta_\ell = \frac{\frac{1}{(2\ell+1)!!} - \frac{R_\ell(o)}{(2\ell-1)!!}}{-(2\ell-1)!!} \quad .$$

By including higher terms, it is easy to become lost in the complexity of the relationship between resonance parameters and the Padé approximant, so we take the view that it is better to work from the exact relationship (3) to derive $R_\ell(k)$ and then find the interior potentials directly from Padé approximant fits to R_ℓ using equation (5).

REPRESENTATIONS OF THE SOURCE DENSITY

During development of the inverse method, ways were found to describe the source term in the Schrödinger equation. In passing, it is noted that we now favour a momentum representation form because it remains valid for the relativistic regions encountered.

The first description is by means of a source term in the Schrödinger equation,

$$\rho(kr) = \sum_{\lambda\mu} A_\lambda(k) V_{\lambda\mu} W_\mu(r) \tag{13}$$

where

$$A_\lambda(k) = \frac{1}{aR} \frac{U_\lambda(a)}{k_\lambda^2 - k^2} \psi(k,a) \quad . \tag{14}$$

$U_\lambda(a)$ is the interacting reaction matrix eigenfunction $U_\lambda(r)$ evaluated at the force range which arises from the boundary condition

$$\left(\frac{dU_\lambda}{dr}\right)_{r=a} = - \frac{\ell}{a} U_\lambda(a) \tag{15}$$

and gives an orthonormal set of interior eigenfunctions when applied to the wave equation (16).

The residue of the poles in equation (5) are related to $U_\lambda(a)$ by

$$\gamma_\lambda^2 = \frac{U_\lambda^2}{a}$$

$$\frac{d^2\psi}{dr^2} + \left(k^2 - \frac{\ell(\ell+1)}{r^2}\right)\psi = \rho$$

for which $$U_\lambda(k_\lambda,r) = \psi(k_\lambda,r) \tag{17}$$

and $$\int_0^a dr\, U_\lambda(r)U_\mu(r) = \delta_{\lambda\mu} \tag{18}$$

$V_{\lambda\mu}$ is the interaction matrix defined for local potentials as

$$V_{\lambda\mu} = \int_0^a dr\, V(r)U_\lambda(r)W_\mu(r) \tag{19}$$

and for non-local potentials as

$$V_{\lambda\mu} = \int_0^a dr \int_0^a dr' V(r,r')U_\lambda(r)W_\mu(r') \tag{20}$$

$W_\mu(r)$ are the orthonormal set of free particle interior eigenfunctions obtained by setting the source to zero in equation (16). For non-local potentials

$$\rho(k,r) = \int_0^a dr' V(r,r')\psi(k,r') \tag{21}$$

Let the overlap matrix of Cook (1972) be defined as

$$B_{\lambda\mu} = \int_0^a U_\lambda(r)W_\mu(r)\, dr \tag{22}$$

in which case

$$\begin{aligned} &\text{(i)} \quad U_\lambda(r) = \sum_\mu B_{\lambda\mu}W_\mu(r) \\ &\text{(ii)} \quad W_\mu(r) = \sum_\lambda B_{\lambda\mu}U_\lambda(r) \end{aligned} \tag{23}$$

It can be shown that the interaction matrix is related to the overlap matrix by the simple equation

$$V_{\lambda\mu} = (k_\lambda^2 - \kappa_\mu^2)B_{\lambda\mu} \tag{24}$$

where κ_μ^2 are the eigenvalues of the zero source solutions to equation (16).

The multi-level potential representation then becomes

$$V(k,r) = \frac{\sum_{\lambda\mu} A_\lambda(k)V_{\lambda\mu}W_\mu(r)}{\sum_{\lambda\mu} A_\lambda(k)B_{\lambda\mu}W_\mu(r)} \tag{25}$$

If we now make the single level approximation to equation (25), noting that A_λ becomes enhanced in the vicinity of such a resonance, we obtain the single level source as

$$\rho_\lambda(r) = \rho(k_\lambda, r) \tag{26}$$

$$= \sum V_{\lambda\mu} W_\mu(r) \tag{27}$$

Such a contribution to the interior interaction appears exactly in the superposition of single level potentials as

$$\rho(r) = V(k,r)\cdot\psi(k,r) = \sum_\lambda A_\lambda(k) V_\lambda(r) U_\lambda(r) \tag{28}$$

and for this reason we choose to regard ρ_λ as the 'pure' resonance source which applies when the Breit-Wigner formula is valid. Equation (28) takes account of resonance overlapping.

REACTION MATRIX FITS TO DATA

The presentation of Nielsen and Oades (1974) data and the Rowe et al. (1978) data as poles and residues is shown in Table 1(a) and 1(b). The Rowe et al (1978) data extended up to about 400 MeV, whereas the Nielsen and Oades (1974) data only went to 270 MeV. The points to notice in these tables are that fewer poles were needed to explain the Nielsen and Oades (1974) data and, since we are primarily interested in this region to extrapolate to the nucleon pole, we prefer these data. The second point is that the Nielsen and Oades parameters gave the value of the nucleon pole almost precisely, whereas the Rowe et al (1978) phase shifts produces a very poor fit to P11 with no bound state at all.

TABLE 1(a)
REACTION MATRIX PARAMETERS FOR NIELSEN-OADES DATA

	k_λ^2	γ_λ^2	R_o	V_o	a
S11	0.567 0.555	0.515 0.590	0.1293	0.1148	1.902
S31	0.528 9.185	0.0021 7.109	0.3332	3.859	0.750
P11	-1.067 7.067 54.29	0.218 1.164 7.786	0.2092	16.147	0.809
P13	2.391 5.410 43.873	0.0003 1.196 1.835	0.1028	2.595	1.407
P31	0.403 4.838 31.308	7.5×10^{-7} 1.015 1.616	0.1155	3.925	1.510
P33	2.318 12.431	0.402 2.399	0.2704	4.593	1.195

TABLE 1(b)
REACTION MATRIX PARAMETERS FOR ROWE ET AL. DATA

	k_λ^2	γ_λ^2	R_o	V_o	a
S11	2.587	2.636	0.0744	0.7490	0.86
	15.719	1.913			
	466.0	3.564			
S31	0.855	0.663	0.0506	0.0	1.82
	8.187	0.617			
	19.047	0.531			
P13	5.160	1.040	0.0575	0.0	1.42
	19.943	0.944			
P31	4.711	0.943	0.0479	0.0	1.52
	17.907	0.788			
P33	2.152	0.340	0.1178	3.031	1.38
	7.009	0.886			
	28.754	1.925			

RELATIONSHIP TO NUCLEON STRUCTURE

The state of the art with respect to nucleon structure has recently been summarised by Brown and Rho (1983). The picture which emerges is quite a complex one, but can be viewed as follows. From about 1.5 Compton wavelengths to about 0.35 Compton wavelengths of the pion, there exists a cloud of virtual pions and vector mesons which dominate the behaviour of the low energy nucleon form factor and pion-nucleon scattering. This is known as the "Chiral bag" whose properties in relation to the magnetic moment are explained by the vector-dominance model (Gell-Mann and Zachariasen, 1961). Otherwise, the behaviour of the Chiral bag is not well understood. At the centre of the nucleon, within a radius of 0.35 Compton wavelengths exists the quark bag in which a red, green and blue quark, thought of as point-like particles, exist in a mutual harmonic oscillator potential. The degrees of freedom of the odd parity excitations evident in pion-nucleon scattering, and manifested in this paper as states of the reaction matrix, are excited states of this three-quark core, and the five odd-parity excited states are well fitted in energy by assuming that the spins of the three quarks can be coupled to give a total spin of $S = \frac{1}{2}$ or $\frac{3}{2}$, and these can have an orbital angular momentum of $\ell = 1$ to yield total angular momenta of

$$J = \frac{1}{2},\frac{3}{2},\left(S = \frac{1}{2}\right); \quad J = \frac{1}{2},\frac{3}{2},\frac{5}{2}\left(S = \frac{3}{2}\right) \quad .$$

The actual energies are fitted by taking into account gluon exchange effects which produce a spin-spin interaction Hamiltonian perturbation

$$\delta H = \sum_{i \neq j} f(r_{ij}) \underset{\sim}{\sigma}_i \cdot \underset{\sim}{\sigma}_j \quad .$$

The P11 wave function plotted in Figure 13 shows the structure mainly of the Chiral bag, but note the zero at 0.35 Compton wavelengths.

The single level sources for the s- and p-wave states are shown in Figures 1 to 6. Figure 1 shows an extremely simple structure for S11. The lowest pole produces a repulsive impulse with a flat back-ground well, whereas the $\lambda = 1$ state is just a flat, repulsive barrier.

Figures 2 to 6 show particle exchange collisions; except for the P11 $\lambda = 0$ state, which is the nucleon ground state, we have no model to explain their distribution. Here we see two exchange regions, both attractive and almost equally spaced.

Fitted phase shifts are summarised in Tables 2(a) and 2(b). All fits were satisfactory, except for the Nielsen and Oades (1974) S11 and the Rowe et al (1978) P11 results. The scattering length contributions from low energy resonances and bound states is given in Table 3 for the best fits.

Wave functions for each state are given in Figures 7 to 12. The P11 probability distribution is displayed in Figure 13.

TABLE 2(a) ROWE ET AL. DATA

E_{lab} (MeV)	S11		S31		P13		P31		P33	
	α_{exp}	α_{calc}	α_{exp}	α_{calc}	α_{exp}	α_{calc}	α_{exp}	α_{calc}	α_{exp}	α_{calc}
0.47	0.758	0.759	-0.402	-0.402	-0.0003	-0.0003	-0.0006	-0.0007	0.0043	0.0042
1.88	1.512	1.514	-0.809	-0.809	-0.0021	-0.0021	-0.0048	-0.0053	0.035	0.034
4.22	2.257	2.260	-1.222	-1.222	-0.0071	-0.0072	-0.016	-0.018	0.117	0.115
7.45	2.988	2.993	-1.646	-1.646	-0.017	-0.017	-0.038	-0.042	0.280	0.274
11.55	3.703	3.708	-2.084	-2.084	-0.033	-0.033	-0.074	-0.082	0.551	0.540
16.49	4.396	4.404	-2.540	-2.541	-0.056	-0.057	-0.127	-0.140	0.961	0.945
22.21	5.065	5.073	-3.017	-3.017	-0.089	-0.090	-0.200	-0.219	1.543	1.521
28.69	5.705	5.715	-3.518	-3.518	-0.132	-0.133	-0.296	-0.323	2.335	2.308
35.87	6.314	6.324	-4.045	-4.045	-0.186	-0.188	-0.417	-0.452	3.380	3.349
43.73	6.889	6.899	-4.601	-4.601	-0.252	-0.254	-0.565	-0.610	4.728	4.698
52.22	7.427	7.437	-5.188	-5.188	-0.332	-0.335	-0.742	-0.796	6.443	6.417
61.30	7.926	7.936	-5.808	-5.808	-0.426	-0.429	-0.949	-1.013	8.599	8.580
70.95	8.385	8.395	-6.463	-6.463	-0.534	-0.538	-1.188	-1.259	11.290	11.284
81.14	8.803	8.812	-7.153	-7.153	-0.658	-0.662	-1.458	-1.537	14.640	14.651
91.84	9.179	9.187	-7.880	-7.879	-0.797	-0.801	-1.759	-1.843	18.810	18.829
103.00	9.513	9.519	-8.643	-8.640	-0.951	-0.956	-2.093	-2.179	23.980	23.990
114.70	9.806	9.811	-9.442	-9.440	-1.121	-1.126	-2.456	-2.544	30.370	30.400
126.80	10.060	10.062	-10.280	-10.272	-1.306	-1.310	-2.849	-2.934	38.190	38.207
139.40	10.270	10.275	-11.140	-11.141	-1.505	-1.510	-3.269	-3.352	47.580	47.617
152.30	10.450	10.450	-12.040	-12.032	-1.718	-1.722	-3.715	-3.788	58.410	58.385
165.70	10.590	10.594	-12.970	-12.958	-1.945	-1.948	-4.183	-4.248	70.190	70.218
179.50	10.710	10.709	-13.930	-13.909	-2.183	-2.186	-4.671	-4.726	82.100	82.232
193.70	10.800	10.800	-14.900	-14.880	-2.432	-2.434	-5.176	-5.220	93.290	93.560
208.30	10.870	10.873	-15.890	-15.874	-2.691	-2.693	-5.694	-5.727	103.200	103.610
223.20	10.930	10.934	-16.900	-16.872	-2.957	-2.958	-6.222	-6.242	111.700	112.120
238.50	10.990	10.993	-17.910	-17.880	-3.230	-3.229	-6.756	-6.764	118.700	119.240
254.20	11.050	11.056	-18.920	-18.890	-3.507	-3.506	-7.292	-7.291	124.600	125.150
270.30	11.120	11.134	-19.920	-19.898	-3.787	-3.785	-7.827	-7.821	129.600	130.060
286.70	11.220	11.240	-20.910	-20.889	-4.067	-4.065	-8.356	-8.346	133.700	134.130
303.50	11.360	11.386	-21.880	-21.862	-4.346	-4.344	-8.878	-8.867	137.200	137.550
320.70	11.560	11.590	-22.820	-22.809	-4.621	-4.621	-9.389	-9.380	140.200	140.440
338.20	11.820	11.868	-23.720	-23.716	-4.891	-4.891	-9.887	-9.878	142.700	142.880
356.10	12.180	12.242	-24.580	-24.578	-5.153	-5.154	-10.370	-10.360	144.800	144.960
374.30	12.660	12.734	-25.370	-25.381	-5.405	-5.405	-10.840	-10.822	146.700	146.740
392.90	13.270	13.375	-26.110	-26.118	-5.645	-5.645	-11.290	-11.262	148.200	148.260
411.80	14.060	14.194	-26.760	-26.774	-5.872	-5.870	-11.730	-11.673	149.500	149.570
431.10	15.060	15.229	-27.330	-27.340	-6.083	-6.079	-12.169	-12.056	150.700	150.700
450.80	16.310	16.525	-27.800	-27.805	-6.278	-6.269	-12.580	-12.405	151.700	151.670
470.80	17.880	18.120	-28.150	-28.151	-6.455	-6.437	-13.000	-12.716	152.600	152.510
491.10	19.830	20.059	-28.370	-28.370	-6.613	-6.583	-13.430	-12.986	153.500	153.230
$\frac{\chi^2}{N}$	0.01		1.5×10^{-4}		5×10^{-5}		0.01		0.04	

TABLE 2(b) NIELSEN-OADES DATA

E_{lab} (MeV)	S11		S31		P11		P13		P31		P33	
	α_{exp}	α_{calc}	α_{exp}	α_{calc}	α_{exp}	α_{calc}	α_{exp}	α_{calc}	α_{exp}	α_{calc}	α_{exp}	α_{calc}
21.50	6.144	5.643	-2.306	-2.708	-0.406	-0.437	-0.172	-0.202	-0.257	-0.278	1.502	1.431
24.79	6.138	6.006	-3.002	-2.929	-0.485	-0.518	-0.209	-0.247	-0.316	-0.342	1.866	1.802
31.03	6.752	6.613	-3.663	-3.290	-0.639	-0.668	-0.285	-0.338	-0.438	-0.474	2.627	2.609
37.03	6.941	7.122	-3.945	-3.482	-0.741	-0.804	-0.355	-0.431	-0.554	-0.613	3.726	3.513
41.53	7.463	7.468	-4.252	-3.085	-0.826	-0.899	-0.412	-0.504	-0.652	-0.722	4.497	4.276
59.46	8.844	8.636	-5.483	-5.818	-1.135	-1.196	-0.648	-0.803	-1.080	-1.194	8.352	8.107
77.99	9.693	9.633	-6.982	-6.886	-1.301	-1.342	-0.935	-1.114	-1.577	-1.727	13.780	13.620
97.96	10.520	10.576	-8.483	-8.171	-1.275	-1.287	-1.296	-1.435	-2.165	-2.330	21.800	21.836
113.00	11.110	11.227	-9.405	-9.170	-1.105	-1.085	-1.582	-1.661	-2.636	-2.796	29.800	30.001
120.00	11.210	11.517	-9.968	-9.643	-0.955	-0.939	-1.742	-1.762	-2.857	-3.016	34.210	34.490
135.00	11.870	12.105	-11.010	-10.665	-0.577	-0.515	-2.121	-1.966	-3.359	-3.489	45.230	45.566
142.00	13.100	12.363	-11.440	-11.147	-0.664	-0.261	-1.781	-2.055	-3.697	-3.712	51.080	51.367
151.00	11.880	12.678	-12.070	-11.765	-0.137	0.117	-2.231	-2.159	-3.963	-3.996	58.940	59.185
165.00	12.010	13.134	-13.000	-12.748	0.705	0.843	-2.856	-2.236	-4.408	-4.449	71.850	71.931
170.10	12.070	13.288	-13.390	-13.108	0.990	1.148	-3.045	-2.041	-4.595	-4.615	76.490	76.493
176.10	11.910	13.459	-13.710	-13.533	1.409	1.537	-3.453	-3.326	-4.808	-4.812	82.030	81.707
189.00	12.110	13.790	-14.610	-14.448	2.317	2.483	-2.800	-2.890	-5.268	-5.238	91.360	92.027
193.90	12.250	13.904	-14.820	-14.805	2.752	2.893	-2.407	-2.940	-5.444	-5.405	94.520	95.649
200.00	12.220	14.034	-15.240	-15.243	3.233	3.429	-2.945	-3.018	-5.679	-5.613	99.170	99.775
210.00	12.370	14.218	-15.650	-15.962	4.462	4.392	-3.159	-3.169	-6.044	-5.957	105.300	105.790
217.10	12.150	14.329	-16.230	-16.477	5.221	5.145	-3.476	-3.289	-6.301	-6.206	109.200	109.540
220.00	12.130	14.369	-16.490	-16.687	5.576	5.469	-3.503	-3.340	-6.407	-6.309	110.700	110.960
224.90	12.160	14.430	-16.840	-17.044	6.407	6.040	-3.632	-3.431	-6.597	-6.485	113.100	113.210
246.90	12.350	14.597	-18.090	-18.653	7.972	8.974	-4.124	-3.897	-7.420	-7.305	121.500	121.350
270.00	12.230	14.588	-19.400	-20.356	11.020	12.768	-4.527	-4.499	-8.271	-8.230	127.900	127.240
χ^2/N	1.78		0.17		0.18		0.08		0.01		0.15	

TABLE 3 RESONANCE CONTRIBUTIONS TO SCATTERING LENGTHS

	Nielsen-Oades	Rowe et al.	Fit to Rowe	Hamilton	Lovelace
S11	0.2375	0.185	0.1915		0.196 ±0.011
S31	0.0834	-0.098	0.192		-0.0700±0.0054
P11	-0.0108	-0.047	-0.129	-0.101±0.007	
P13	0.0905	-0.013	-0.077	-0.029±0.005	
P31	0.1501	-0.029	-0.015	-0.038±0.005	
P33	0.5179	0.205	0.357	0.215±0.005	

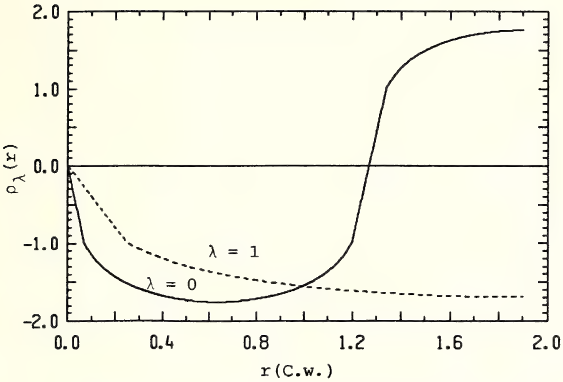

Fig. 1. $\rho_\lambda(r)$ vs r for S11 Nielsen-Oades data

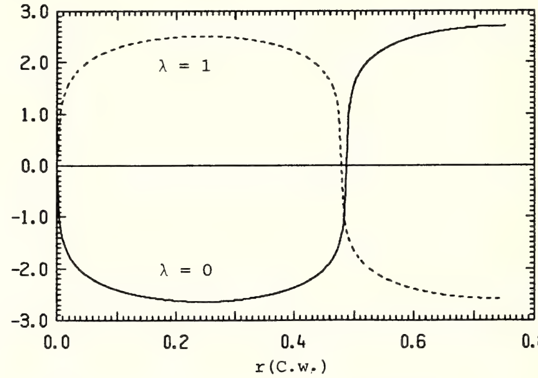

Fig. 2. $\rho_\lambda(r)$ vs r for S31 Nielsen-Oades data

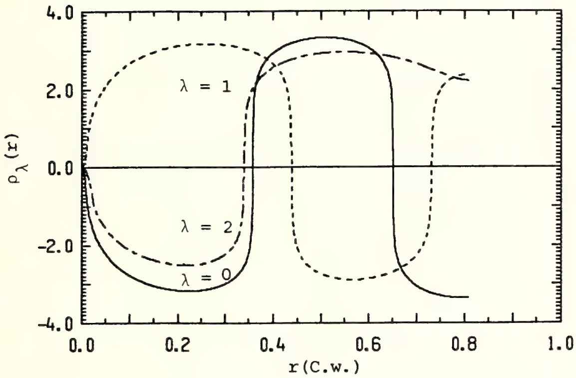

Fig. 3. $\rho_\lambda(r)$ vs r for P11 Nielsen-Oades data

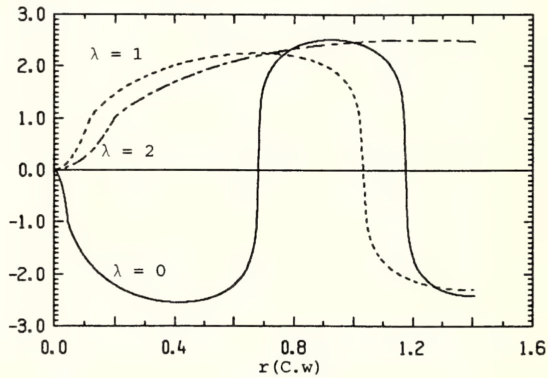

Fig. 4. $\rho_\lambda(r)$ vs r for P13 Nielsen-Oades data

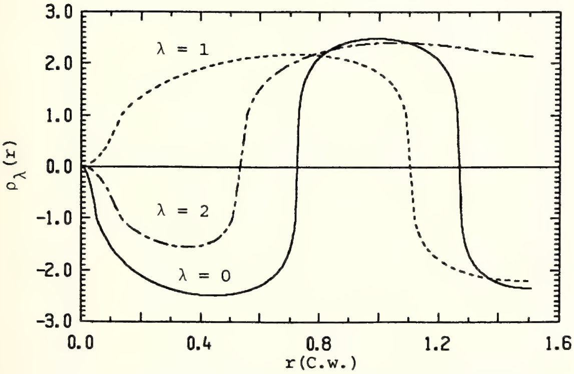

Fig. 5. $\rho_\lambda(r)$ vs r for P31 Nielsen-Oades data

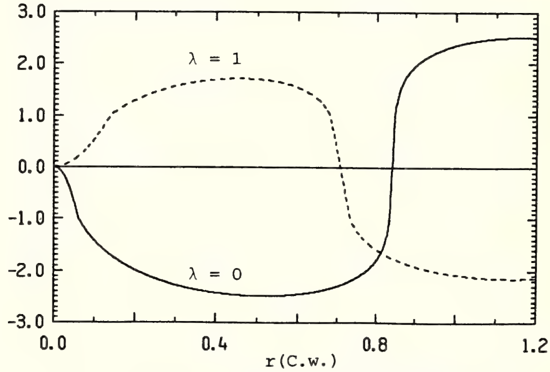

Fig. 6. $\rho_\lambda(r)$ vs r for P33 Nielsen-Oades data

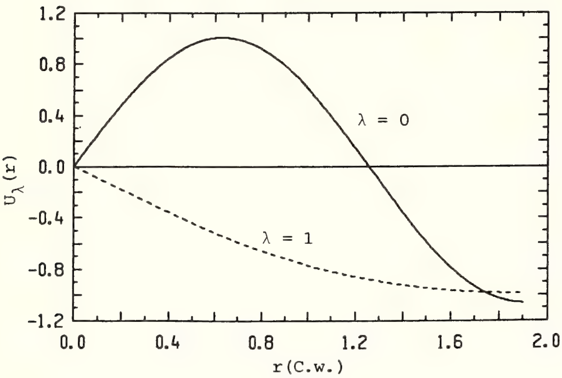

Fig. 7. $U_\lambda(r)$ vs r for S11 Nielsen-Oades data

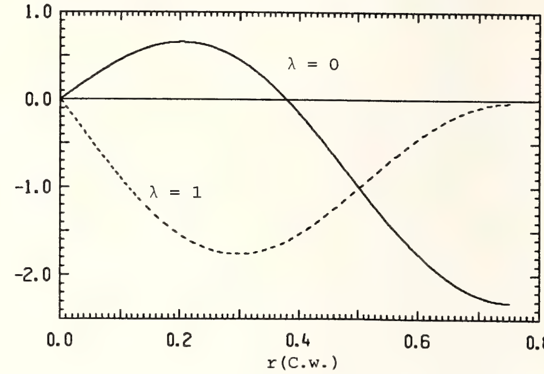

Fig. 8. $U_\lambda(r)$ vs r for S31 Nielsen-Oades data

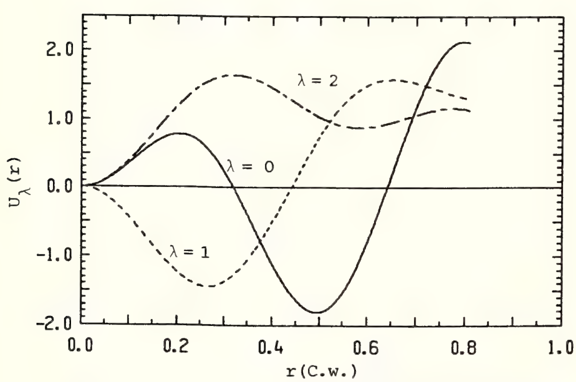

Fig. 9. $U_\lambda(r)$ vs r for P11 Nielsen-Oades data

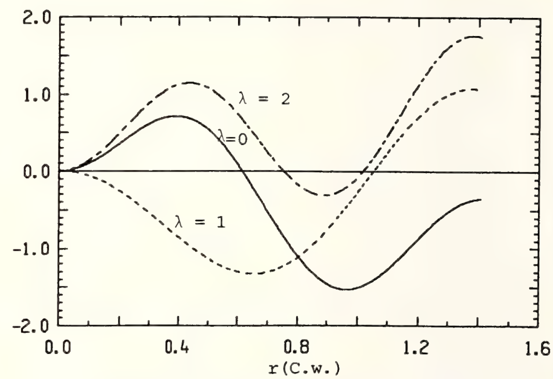

Fig. 10. $U_\lambda(r)$ vs r for P13 Nielsen-Oades data

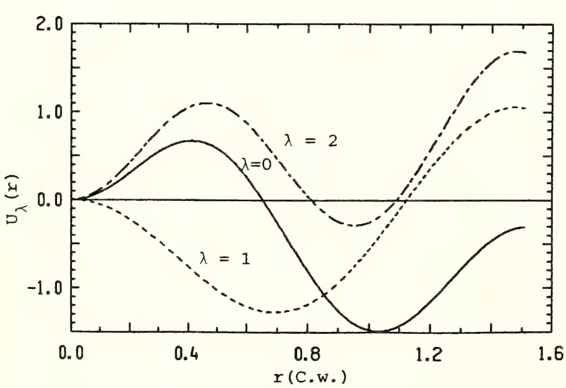

Fig. 11. $U_\lambda(r)$ vs r for P31 Nielsen-Oades data

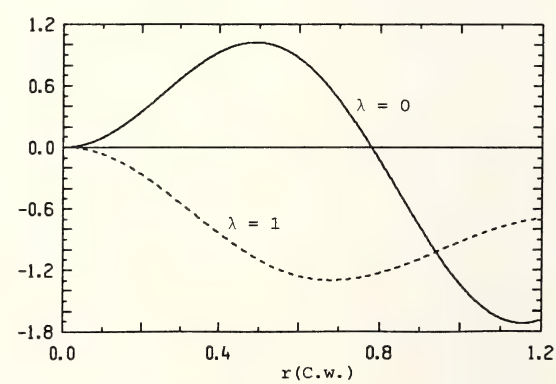

Fig. 12. $U_\lambda(r)$ vs r for P33 Nielsen-Oades data

THE NUCLEON FORM FACTOR

The differential cross section for the elastic scattering of electrons from nucleons is determined by the Rosenbluth (1950) formula

$$\frac{d\sigma}{d\Omega} = \left(\frac{d\sigma}{d\Omega}\right)_{ns} \left\{ \frac{G_E^2 - \left(\frac{Q^2}{4M^2}\right) G_M^2}{1 - \left(\frac{Q^2}{4M^2}\right)} - \frac{Q^2}{2M} G_M^2 \tan^2 \frac{\theta}{2} \right\}$$

where $\left(\frac{d\sigma}{d\Omega}\right)_{ns}$ is the no-spin cross section given by

$$\left(\frac{d\sigma}{d\Omega}\right)_{ns} = \frac{\alpha^2}{4E^2 \sin^2 \frac{\theta}{2}} \cdot \frac{\cos^2 \frac{\theta}{2}}{1 + \frac{2E}{M} \sin^2 \frac{\theta}{2}}$$

in which $Q^2 = -4E^2 \sin^2 \frac{\theta}{2} \Big/ \left(1 + \frac{2E}{M} \sin^2 \frac{\theta}{2}\right)$

$=$ (momentum transfer)^2,

E = the energy of the electron,

θ = the scattering angle of the electron in the centre-of-mass system,

G_M^2 = the electric form factor, and

G_M^2 = the magnetic form factor.

If we refer back to the old atomic theory by Born (1955), we see that the momentum transfer in the form factor is resolving the structure of the nucleon, so standard Fourier transform methods lead to the result

$$G(Q) = 4\pi N\mu \int_0^\infty |\psi(r)|^2 \frac{\sin Qr}{Qr} \, dr \tag{29}$$

where $G(Q)$ is normalised so that $4\pi N\mu = 1$ and $\mu = \mu_p$ or μ_N as is applicable in the proton or neutron case. Using the wave function for the P11 ground state, as shown in Figure 13, the integral (29) is calculated and compared with experiment in Figure 14. The agreement is seen to be quite good.

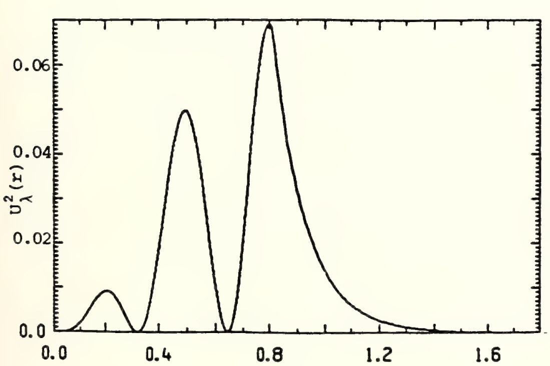

Fig. 13. $U_\lambda^2(r)$ vs r for P11 Nielsen-Oades data for $\lambda = 0$

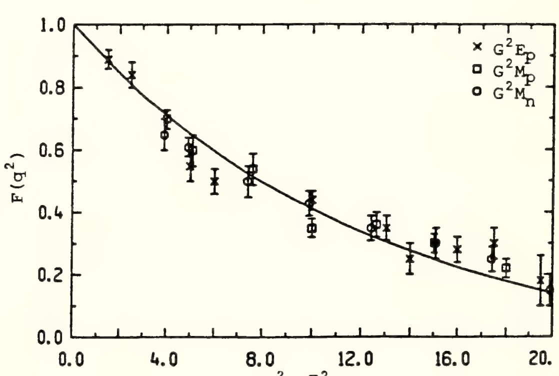

Fig. 14. Predicted Form Factor vs Experimental data.

THE NUCLEON POLE

In Hamilton's (1967) review article, he discussed the contribution to the scattering matrix from the single nucleon intermediate state, which is

$$T_{BORN} = \frac{G^2}{M}\gamma^5 \cdot \frac{i\underset{\sim}{\gamma} \cdot \underset{\sim}{P}-M}{P+M} \tag{30}$$

in which G^2 = the pseudoscalar pion-nucleon coupling constant,

$\underset{\sim}{\gamma}$ = the Dirac spinor matrix,

γ^5 = the pseudoscalar Dirac matrix,

$P = p_1+q_1$,

p_1 = the incident nucleon 4-momentum, and

q_1 = the incident pion 4-momentum.

Working through the algebra from equation (30), the value of this contribution to the P11 amplitude is

$$T_{BORN} = \frac{6Mf^2}{S-M^2} \tag{31}$$

in which $f^2 = \frac{G^2}{16\pi M^2}$ = the pseudoscalar coupling constant,

$$S = (p_1+q_1)^2 = \left\{\sqrt{k^2+1} + \sqrt{k^2+M^2}\right\} .$$

The theoretical location of the nucleon pole from (31) is

$$k_o^2 = -1 + \frac{1}{4M^2} = -0.99466 . \tag{32}$$

If we relate this to equation (1), we find that near the nucleon pole

$$e^{2i\alpha} = e^{2ka}\left\{\frac{1-ka+(ka)^2(R(-k^2))}{1+ka+(ka)^2(R(-k^2))}\right\} \tag{33}$$

$$\rightarrow \frac{H^2}{S-M^2} , \quad H^2 = h^2C$$

and, by taking limits on the residues,

$$C = -2\left[1 + \frac{1}{2}\sqrt{1-k_o^2}\,\sqrt{M^2-k_o^2}\left\{\frac{1}{1-k_o} + \frac{1}{M^2-k_o^2}\right\}\right] .$$

Using the data of Table 1(a) we get

$$k_o^2 = -0.999889 \pm 10\% ,$$

$$h^2 = -0.08417, \quad C = -91.309$$

which yields

$$H^2 = 3.85 \pm 30\% .$$

The best value of H^2 to date is that of Lovelace (1969), who, for the P33 resonance, obrains $H^2 = 3.314 \pm 0.073$.

CONCLUSION

We have used more recent data to fit the pion-nucleon scattering phase shifts more accurately at low energies, and obtained excited state sources applicable to the meson field distribution in the meson bag. This was done in the belief that such potential energies have a physical meaning for the very small lengths involved. The desire to determine off-shell properties from theory can only be overcome with a greater understanding of multiparticle angular momentum eigenfunctions in the relativistic region, and their associated partial wave amplitudes for multiparticle production. This complication makes phase shift analysis very difficult at high energies, as the multiparticle production channels are many in number.

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J.L. Cook, Lock Mail Bag No. 1, Menai, NSW 2234 Australia

E.K. Rose, Lock Mail Bag No. 1, Menai, NSW 2234 Australia

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The Comboyne and Bulga Plateaus and the Evolution of the Great Escarpment in New South Wales

C. F. PAIN AND C. D. OLLIER*

ABSTRACT

The Comboyne and Bulga Plateaus are scarp-bounded outliers of a palaeoplain located east of the Great Escarpment in northern N.S.W. The Comboyne Basalts are related volcanic plugs enable a chronology to be derived, and in the immediate area of the plateaus scarp retreat has brought the escarpments to their present position within the past 16 m.y. The plateaus are preserved not because of basalt or any other caprock, or because of any special structural resistance, but because their position on an ancient watershed was a location most protected from erosion. To some extent the plateaus may have been protected by the resistant Triassic conglomerate between the plateaus and the coast. The plateaus are "plateaus of circumdenudation" and can be seen to be a result of locally slower rates of knickpoint retreat than were generally operative in the area. Scarp retreat is clearly the dominant mechanism of erosion so long as plateau remnants are preserved, and the escarpment has even eroded back into trachyte plugs to form half domes. The relatiionship between the plateaus and other landforms, depicted on a terrain classification map, suggests that once plateaus are removed further landscape evolution is by stream incision and eventually slope decline.

Key words: Comboyne Plateau, Bulga Plateau, Great Escarpment, palaeoplain, scarp retreat

INTRODUCTION

The recognition of the Great Escarp-ment of eastern Australia (Ollier, 1982a) provides a unifying theme in the geomorphology of the eastern uplands. It stresses the considerable antiquity of upland landforms in eastern Australia (Ollier, 1979, Young 1983) and emphasises the major distinction between the older landforms of the inland palaeoplain, with low relief and slow rates of change, and the younger landforms east of the Great Escarpment with their higher relief and faster rates of change.

There is general agreement that the inland palaeoplain was uplifted to form the eastern uplands, and that subsequent erosion has modified their form. There is, however, considerable debate over the timing and nature of the uplift, with conflicting evidence coming from different areas in eastern Australia and from different lines of enquiry (for example see Wellman 1979, Jones and Veevers 1982, Stephenson and Lambeck in press).

Since the broad outlines of the Great Escarpment were published (Ollier 1982a), we have undertaken a number of more detailed studies in smaller areas in an attempt to throw light on the details of the geomorphic development of the Eastern Highlands (Ollier 1982b , 1984; Pain 1983). The present study is another in this sequence.

The Bulga and Comboyne Plateaus are inland of Taree and Port Macquarie, on the central north coast of New South Wales (Fig. 1). They lie east of the main Great Escarpment, which in this area is irregular, with peninsula-like spurs separated by deep embayments. The plateaus are remnants of a former palaeoplain, now separated from the main palaeoplain west of the Great Escarpment. In this they are similar to the Barrington Tops plateau (Pain, 1983). Study of the landforms in this area should provide details of the nature of scarp retreat and geomorphic evolution in the area. In addition, basalts and intrusive igneous rocks provide some age control. Here we describe the Comboyne and Bulga Plateaus and consider them in the wider context of the Eastern Highlands of Australia.

* (COMMUNICATED BY G. S. GIBBONS)

TERRAIN CLASSIFICATION

A study of maps, aerial photographs and Landsat imagery of the area suggests six major landform types (Fig. 2) which are briefly described below::

1. Ridge and valley landscape. Most of the area consists of a landscape of high relief, with angular ridges and V-shaped valleys. Slopes are straight, ridges are sharp, and valleys narrow. These landforms are indicative of active incision and valley slope adjustment, and are typical of humid landforms in many parts of the world.
2. Rounded Hills. East of the area of ridge and valley landforms is an area of hill country where relief is low to moderate and the slopes, hillcrests and valley floors are rounded rather than angular. This is a simple fluvially-eroded landscape, but with lower relative relief and more subdued topography than that of the ridge and valley area.
3. Coastal depositional plain. The coastal plain consists of alluvial floodplains and low terraces, and various coastal landforms including estuary fills, dunes and beach ridges. Occasional outliers of low hill country emerge above the coastal plain.

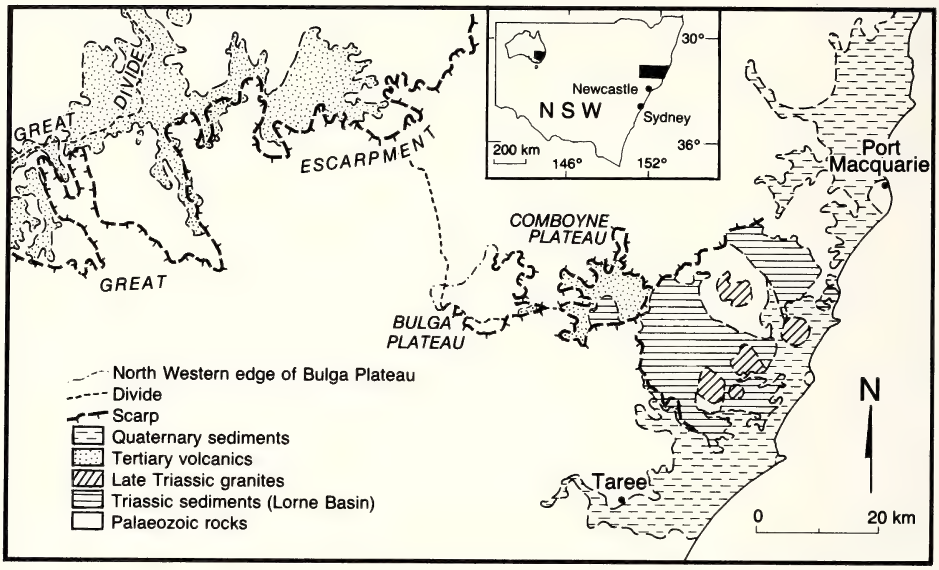

Figure 1. Location and general geology of the Comboyne and Bulga Plateaus and the surrounding area to the Great Divide. Tertiary trachyte plugs are not shown on this figure, but are shown on figure 3

4. Tablelands surface. West of the Great Escarpment, on the Bulga and Comboyne Plateaus and on several minor plateaus, there are landforms of low relief and generally gentle slopes. These are a part of the "tableland surface" of Ollier (1982a), and are described in more detail later.
5. Structural surfaces. In some places the Triassic sediments of the Lorne Basin give rise to low relief and gently sloping landforms that are controlled to a large extent by the dip of the underlying strata. The Triassic sediments give rise to prominent structural escarpments in some outcrops.
6. Granite domes. A number of microgranite intrusions lie within the general area of the Lorne Basin (Figs. 1 & 3). Some of these have been eroded so that they appear very similar to surrounding areas, but others (the Three Brothers for example) stand above the surrounding area as dome-shaped mountains (Fig. 2).

THE COMBOYNE PLATEAU

The Comboyne Plateau is surrounded by a well-defined erosional scarp (Fig. 3). On the southern side there is a fall of 550 m from the plateau edge to the Lansdowne River, while on the western side the plateau stands about 400 m above the Ellenborough River. To the east the difference in elevation between the Comboyne Plateau and the upper surfaces on the Triassic sediments is much less, but there is still a distinct break, in the form of a narrow area of ridge and valley landforms.

The highest points on the Comboyne Plateau are trachyte plugs which stand well above the general plateau level, e.g. Mounts Gibraltar (851 m) and Kophi (797 m). The general level of the plateau falls from about 700 m in the area around Mount Kophi to less than 600 m around the other edges. In the west the edges are at about 600 m and in the east they are about 500 m. To the north the lowest elevations on the plateau are farthest from Mount Kophi. The northernmost part of the long spur extending north from the main part of the Comboyne Plateau is only about 400 m. The surface of the Comboyne Plateau thus has the shape of a part of a low angle cane or dome, centred on a point on the edge of the plateau south of Mouth Kophi. Relative relief on the plateau is between 40 m and 80 m.

As might be expected from the general shape of the Comboyne Plateau, the drainage pattern is radial from the highest point on the southern edge. The only place where this pattern is disrupted is in the headwaters of the Upsalls Creek catchment, where the northernmost tributary of Upsalls Creek appears to have captured the headwaters of originally radial drainage lines (Fig. 3). Drainage to the south and east of the Comboyne Plateau is also radial from the southern edge of the plateau (Fig. 3), suggesting that it may have originated on the same gently sloping cone-shaped surface. This implies that the Comboyne Basalts may have a single central source (Ollier, in press).

Three rivers leave the Comboyne Plateau over waterfalls, while the others have steepened reaches. There is no evidence of aggradation on the Comboyne Plateau: all rivers appear to be cutting down on bedrock.

The geology of the Comboyne Plateau is not as simple as shown on the Hastings 1:250,000

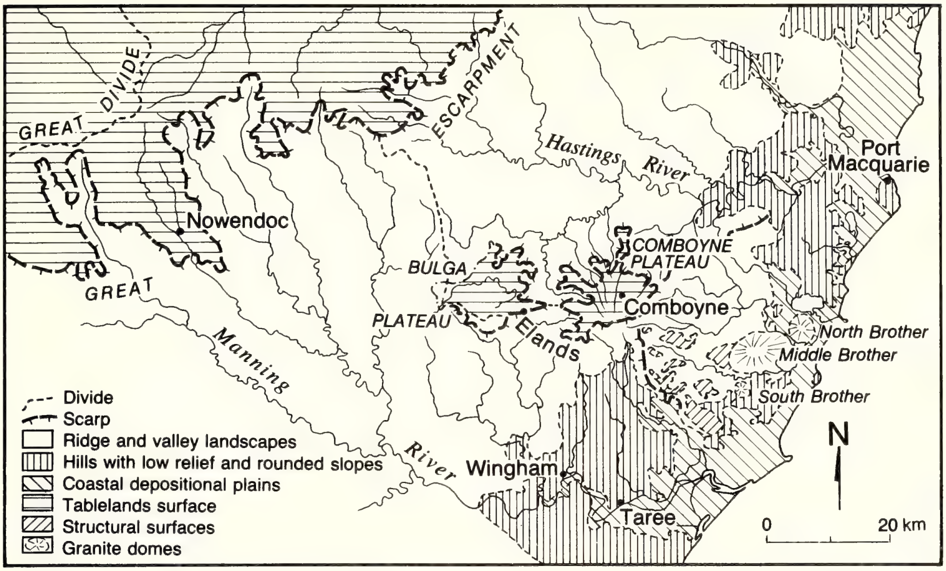

Figure 2. Landforms of the Comboyne and Bulga Plateaus and the surrounding area

geological map (Brunker et al. 1968). Part of the plateau is covered with Comboyne Basalts, which, together with the associated intrusives, constitute a group of pyroxene-bearing trachytes (Stewart, 1969). However, there are significant areas of other rock types (Fig. 3). Two inliers of Palaeozoic rocks occur to the west of Comboyne, and the northernmost extension of the plateau is also on Palaeozoic rocks.

The south-central part of the plateau is formed on Triassic rocks (Figs. 1 & 3). Here there is an extensive outcrop of a claystone rock unit which lies at the top of the Triassic sequence of the Lorne Basin. The claystone weathers to give dark red soils almost identical in appearance to those found on the Comboyne Basalt, and it is only inspection of stream beds and deeper road cuttings that allows the claystone to be identified and mapped.

Branagan (1969) noted the presence of Cainozoic mudstone, tuff and laminated limonite beds up to 30 m thick on the Comboyne Plateau. Trachyte plugs occur on both on the plateau and to the south (Fig. 3). Just north of the plateau, near Bridal Veil Falls, a road cutting exposes 15 m of agglomerate, covered by 4 m of volcanic ash, underlying basalt. The agglomerate overlies 1 - 2 m of quartz rich Tertiary fluvatile gravels, at an altitude of 410 m. The gravels rest directly on Palaeozoic rocks. Tertiary gravels also occur in the headwaters of Toms Creek, on the western side of the plateau (Fig. 3). Here they rest on basement rocks at 465 m. In this area there is also a deposit of diatomite between two basalt flows. The sub-basalt surface thus has a similar relief to that of the plateau in general.

The partial removal of basalt cover on the Comboyne Plateau has led to the development of benches similar to those found around the Barrington Tops Plateau (Pain 1983). In many places, but particularly along the western side of the plateau, the basalt along the edge undergoes mass movement by slumping in such a way that the basalt edge retreats faster than the underlying Palaeozoic rocks. This leads to the formation of a distinct bench below the plateau edge (Figs. 3, 4). The level of the bench is approximately the level of the sub-basaltic surface (Worrell 1984). It is important to stress that the basalt cap is not responsible for protecting the underlying rocks: it is the other way around. Thus the preservation of the Comboyne Plateau cannot be attributed to protection by a cover of basalt.

THE BULGA PLATEAU

The Bulga Plateau is clearly defined by an erosional scarp on three sides. On the northwest side, however, the edge of the plateau is less distinct, grading into ridge and valley landforms (Fig. 3). At Elands there is a fall of 560 m to Dingo Creek, while on the eastern side there is a fall of 400 m to the Ellenborough River. The plateau thus stands well above the surrounding country. The plateau itself rises from a low point at the top of the Ellenborough Falls at 460 m to about 800 m on the western side. The relative relief of the plateau is about 50 to 100 m.

The plateau has the form of a shallow saucer, and drainage is confined largely to the catchment of the Ellenborough River. Bulga Creek and the Ellenborough River both leave the plateau over falls (Fig. 3), while other streams flow over steepened sections. Both falls lie on a fault (Brunker et al., 1968). Currently all streams on the Bulga Plateau appear to be actively incising their beds, and they all flow on bedrock.

The bedrock of the Bulga Plateau is folded Palaeozoic sedimentary rocks. A small cap of

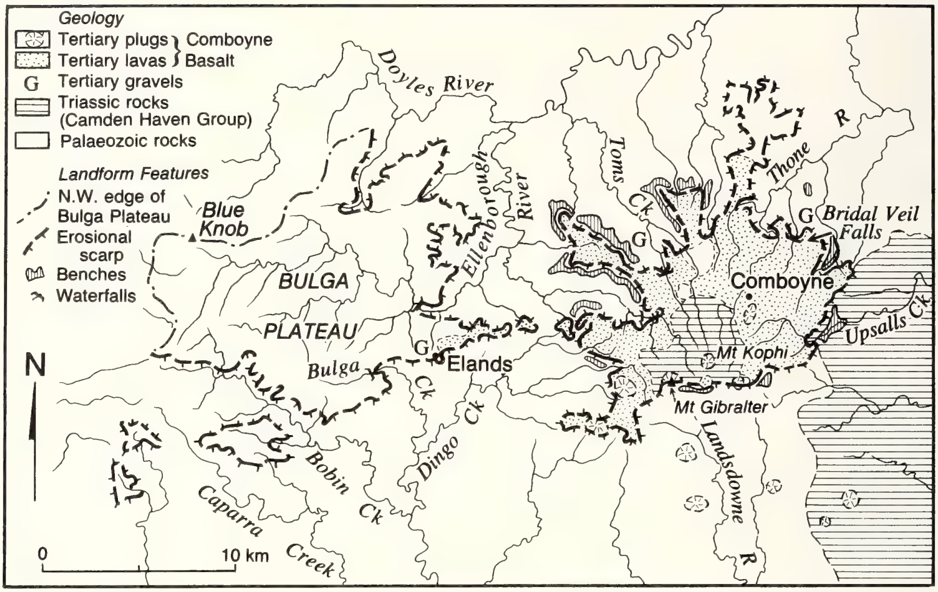

Figure 3. Landform features and geology of the Comboyne and Bulga Plateaus

basalt lies on the easternmost extension of the Bulga Plateau, and on the ridge that joins it to the Comboyne Plateau (Fig. 3). There is no evidence that basalt was ever any more extensive over the Bulga Plateau. A small capping of Tertiary alluvial gravel occurs just west of Elands (Fig. 3) attesting to former high level stream activity in the area, perhaps when it had even lower relief than at present.

Three small plateau remnants lie to the south of the Bulga Plateau (Fig. 3). These remnants have about the same degree of relief as the main Bulga Plateau, and lie at about the same elevation. It seems likely that they were once part of the plateau, and have been separated from it by incision along Bobin and Caparra Creeks.

The Comboyne and Bulga Plateaus are thus similar in general form, but rather different in their details of landform and geology.

THE ORIGIN OF THE PLATEAUS

It seems clear that the tablelands to the west of the Great Escarpment and the Bulga and Comboyne Plateaus were once part of the same surface, and that retreat of the Great Escarpment has isolated the plateaus from the main tablelands surface. This former more extensive surface, or palaeoplain, had a generally low relief (up to 100 m), with perhaps some upstanding mountains. In other words, apart from retreat of the Great Escarpment, and the incision that accompanied this process, the tablelands surface has undergone little change since before the initiation of the Great Escarpment. One of these changes is the eruption of a basalt cover in some areas. However, it is important to note that the eruption of the basalt, in itself, is not a particularly important part of the geomorphic evolution of the area. The results of this evolution would be very little different if the basalt had not been erupted. Nevertheless the basalt gives a useful date, and also provides some protection for pre-basalt sediments and surfaces. Indeed, it is these pre-basalt sediments and surfaces that allow us to be quite certain that the form of the tablelands surface has changed very little in its broad outlines since before the eruption of the basalt.

There is no doubt that the scarps around the plateaus are erosional in origin. They do not coincide with any Palaeozoic rock boundaries so differential erosion can be discounted. The location of two of the Tertiary trachyte plugs underline this point: Mount Gibraltar and a smaller plug to the east are both cut in half by the scarp, leaving them as spectacular half domes (Fig. 5). We have not carried out a detailed study of the nature of scarp retreat (such as that by Moon and Selby, 1983), but scarp retreat could seldom be more obvious than here where it cuts back into a dome-shaped trachyte plug, leaving the domal surface on the plateau side apparently unaffected.

If we accept a hypothesis that the Great Escarpment formed at a location much further east than it is now, then the escarpment has retreated to its present position. This implies that the erosional landforms to the east of the escarpment are diachronous: those close to the present coast are older than those close to the present location of the Great Escarpment. We can visualise a sequence of erosional development from retreat of knickpoints up the major rivers through valley deepening to valley widening and decline of slope angles. In this sequence the Tableland surfaces are very old, and the Great Escarpment, ridge and valley landforms and rounded hills are all sequentially related in an order of increasing age.

Figure 4. The embayment cut by Toms Creek into the northwestern side of the Comboyne Plateau. The plateau edge on basalt is in the foreground, and the buildings in the centre left are on a bench formed approximately at the contact between basalt and the underlying rocks. Other benches are viable on the far side of the valley

These considerations do not answer the question of the preservation of tableland on the Bulga and Comboyne Plateaus. If the Great Escarpment retreated past the location of the two plateaus, how did they survive?

It has already been noted that the cover of basalt on the Comboyne Plateau does not provide an answer, and in any case it appears there was an insignificant amount of basalt on the Bulga Plateau. The only other possible caprock present is the Triassic conglomerate which has a similarly insignificant distribution.

The location of the Comboyne Plateau immediately to the west of the Triassic Lorne Basic sediments may be more than coincidence. The Triassic sediments, particularly the conglomerate facies, are highly resistant to erosion. The conglomerate resembles silcrete, having a silica cement and conchoidal fracture. The highest Triassic sediments commonly survive behind well-defined scarps (Fig. 4). To some extent the Comboyne and Bulga Plateaus may have been protected from the main activity of scarp retreat by the resistant Triassic conglomerate to the east.

The Bulga and Comboyne Plateaus lie on the watershed between the Hastings and the Manning Rivers, with most of their drainage leading into the former (Fig. 2). Such a location, on a major divide, would favour the survival of remnants of the tableland surfaces (Fig. 6). In the broad zone between the Great Escarpment and the two plateaus the northern tributaries of the Manning and Upper Hastings Rivers have a distinct preferred orientation south-southeast (Fig. 2). This suggests either control by underlying rock "grain", for which there is some evidence (Brunker et al. 1968), or an original surface sloping in this direction on which the drainage pattern was developed.

Another factor is the relative size of the catchments of rivers flowing from the tablelands. As Twidale (1978) has noted for more arid areas, there is a decline in discharge over knickpoints as they retreat upstream. The mplication of this is that the rate of knickpoint retreat will also decline over time, and be slower for rivers with small catchments than those with larger catchments. The Ellenborough River now rises much closer to the coast than the other rivers in the area (Fig. 3), and there is no reason to suppose that this was not the case when all the rivers flowed on the previously much larger area of tablelands. Knickpoint retreat up the Ellenborough River would thus have slowed down earlier than on rivers that rise farther west, closer to the Great Divide. The same applies to rivers draining the Comboyne Plateau, although the basalt complicates the situation there. In view of these considerations the plateaus could be seen as a result of locally slower rates of knickpoint retreat than were generally operative in the area.

The pre-Basalt drainage pattern is not clear, but it is probable that the Ellenborough River flowed further east before turning to join the Hastings River, and that it was blocked by volcanism in the Camboyne Plateau area. These events may all have combined to lead to the survival of the two plateaus at some distance from the main tablelands surface to the west.

There are three published K/Ar ages for Tertiary volcanic rocks in the general area of the Comboyne Plateau. McDougall and Wilkinson (1967) report an age of 15.7 + 0.7 Ma from Mount

Figure 5. Stereo vertical air photographs of Mount Gibralter and the surrounding area. Note the half dome of Mount Gibralter, the abrupt erosion scarp to the south, and the much lower relief on the Comboyne Plateau to the north. Reproduced by permission of the Department of Lands, New South Wales

Oliver. Wellman and McDougall (1974) dated two trachyte flows from near Comboyne. One, at Comboyne, gave an age of 16.0 + 0.6 Ma, while the other, 4 km southeast of Comboyne, gave an age of 15.9 + 0.6 Ma. These ages all indicate that volcanism in the area occurred about 16 million years ago.

The nature of the scarp surrounding the Comboyne Plateau, and especially the half dome of Mount Gibraltar, shows that the scarp, if it had already been initiated, must have been some distance to the southeast, and presumably also the northeast, at the time of volcanic activity. The plateau surface was thus in existence 16 million years ago, and although it has suffered some erosion since then the amount is insignificant compared with erosion in the area surrounding the plateau.

Figure 6 summarises our thoughts on the evolution of the area. Warping occurred in the Early Tertiary, probably as a result of the formation of the current eastern edge of the Australian continent. By the time of the volcanic activity 16 million years ago in the Comboyne area, the Hastings and Manning Rivers, and some of their tributaries, had carved large amphitheatre-shaped valleys, but a broad interfluve area of tableland surface still remained. It was onto this divide area that the lavas of the Comboyne Basalt were erupted. Further erosion joined the heads of the Hastings and Manning River valleys to form the Great Escarpment, and the large area of ridge and valley landscape, but the Comboyne and Bulga Plateaus were preserved because of the factors discussed above.

CONCLUSIONS

The Comboyne and Bulga Plateaus are clearly remnants of a formerly much more extensive tablelands surface. Structurally controlled surfaces east of the Comboyne Plateau may also be remnants of the same surface. Scarps surrounding the plateaus are erosional in origin, and this is best shown on the southern edge of the Comboyne Plateau, where scarp retreat has created two half domes from trachyte plugs. The plateaus are thus tableland remnants that survived a period of marked erosion in the general area. This erosion was associated with the retreat of the Great Escarpment in this part of eastern Australia.

Although volcanism about 16 million years ago gave a cover of lavas to the Comboyne Plateau, it does not explain the presence of the plateau remnants. Their survival is rather a result of their location on the major watershed between the

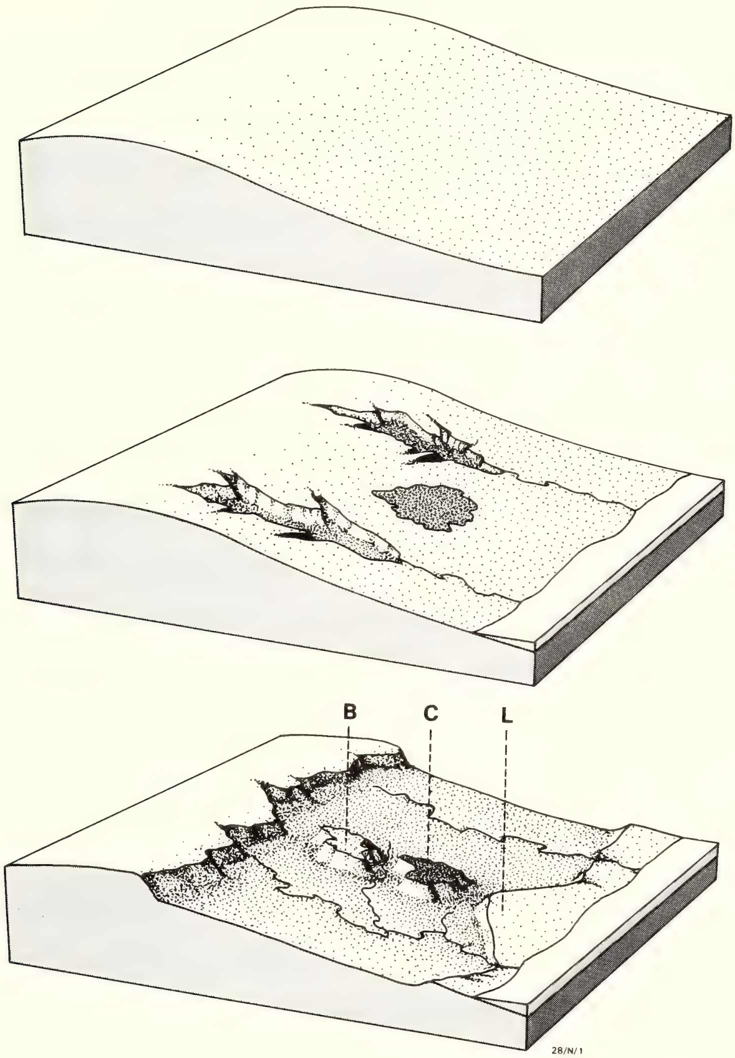

Figure 6. Suggested origin of the landforms around the Comboyne and Bulga Plateaus. This diagram is an adaptation from Ollier 1982a, Fig. 7, modified to fit the study area. B = Bulga Plateau; C = Comboyne Plateau; L = Lorne Basin

Hastings and Manning Rivers, combined with locally slower rates of knickpoint retreat up the Ellenborough River which drains the Bulga Plateau. The presence of the resistant sediments of the Lorne Basin to the east may also have been a factor. These remnants are thus "plateaus of circumdenudation" left behind while major erosion took place all around them.

These conclusions lend support to Ollier's (1982a) hypothesis that the Great Escarpment began with the formation of steep and separate valleys on the steeper, seaward margin of the upwarped eastern edge of the Australian continent. These valleys, retreating headwards, later joined to form a single main escarpment, with some peninsula-like spurs and a few isolated plateaus, of which the Comboyne and Bulga Plateaus are fine examples.

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1. C.F. PAIN
School of Geography, University of N.S.W., P.O. Box 1, Kensington, N.S.W. 2033.

2. C.D. OLLIER
Department of Geography and Planning, University of New England, Armidale, N.S.W. 2351.

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Broadcasting to the Nation*

Leonie Kramer

Asa Briggs, describing the early life of the BBC, wrote:

> It was no longer engaged in an ordeal for survival; it was gradually acquiring a more mature and confident personality of its own.

These words have a familiar ring, for both the sentiment and the metaphor have been applied many times (and at different stages) to artistic development in Australia. They could have been written by any one of a number of critics of Australian literature in the nineteenth or twentieth centuries. They remind us that there are institutional, as well as cultural examples of the links between the old world and the new. There is nothing novel about the general proposition that a newly settled country will begin by importing what it needs until it learns to make things for itself. As in literature, so in broadcasting, Australia was fortunate in its inheritance, and in particular in the example provided by the BBC.

The ABC was born into a decade which had not begun promisingly for the arts. There was none of the literary excitement that had been generated in the 1890s round *The Bulletin*, and none of the major journals which have since played so important a role in the dissemination of ideas about literature and the arts had been established. There were attempts to lay down the principles of a national literature and theatre, notably by the Jindyworobak movement and its founder Rex Ingamells, and by the writer Vance Palmer, but these efforts did not stir the imagination of the public. To this Australian community, sceptical of its own cultural capacities, much inclined to regard imported literature, theatre and music as necessarily better than anything that could be produced locally, and uncertain of standards of accomplishment, the ABC began to broadcast. It will perhaps never be possible to make a definitive assessment of its contribution to Australian cultural development. But over the next fifty years it effectively established and supported the musical life of the country through its broadcasts and orchestral concerts; through its school broadcasts and general talks it helped to educate countless numbers of people; it provided services to isolated communities, so far as its technical facilities permitted; and it provided a new medium for writers, (especially in radio and drama and features), musicians and actors; above all, it was relied upon by the whole community for accurate and objective news and information services. Towards the end of the 1930s Australian literature showed the first signs of flowering into the modern world, and the ABC must take some credit for having prepared audiences for new artistic energies. It is quite unlikely it could have done any of these things without the Reith model of broadcasting to guide it.

Australia has not been good at creating general ideas, and is still not (though it has been conspicuously inventive in science and technology). But once the fundamental principles were understood, their implementation was marked by originality and by sensitivity to the needs and interests of the audience. The comparison with literary growth again suggests itself.

The ABC was established only five years after the BBC became a corporation whose charter guaranteed its independence of both government and business. Even before this, however, Reith had set the stamp of his own high ideals and vision upon the operation of the British Broadcasting Company. At the valedictory dinner which marked the end of 'the old BBC', Reith restated his philosophy of broadcasting and said

> We have tried to found a tradition of public service and to dedicate the service of broadcasting to the service of humanity in its fullest sense.

Three particular aspects of Reith's philosophy were translated directly into the ABC - the absence of the profit motive, the idea of national coverage, and the establishment and maintenance of the highest standards. Both those last two principles demand a particular concept of the audience. Reith took the view that 'it is better to over-estimate the mentality of the public than to under-estimate it'. He also referred, not to a mass audience, but a number of different publics who together make up the audience. As Briggs comments:

> The 'publics' are treated with respect not as nameless aggregates with statistically measurable preferences, 'targets' for the programme sponsor, but as living audiences capable of growth and development. In other words, Reith's theory of public service began with a conception of the public.

Australia was the fortunate inheritor of these ideas, for like many other countries throughout the world, it looked in the late 20s to the BBC as a model. So the ABC began with clear objectives - to serve people throughout Australia; to offer a range of programmes which in content and presentation would

* Address by Professor Dame Leonie Kramer DBE to The Royal Society of New South Wales at the Annual Dinner held at the Sydney Hilton on Tuesday, 18th March, 1986.

provide, so far as is possible, something for everyone at some time; and to maintain high standards. From the beginning, however, the ABC suffered by comparison with the BBC one singular disadvantage. It had commercial competitors - the B-class stations. Reith warned Menzies in 1935 that unless they were checked they 'would become so powerful that no one would touch them.' In the following year Menzies acknowledged that Reith had been right, and when Reith asked if they were going to 'put things right' Menzies replied 'No, we haven't the guts'.

Meanwhile the ABC was following closely the structural arrangements and programming policy of the BBC. Departments of Music, Talks, Education and so-on mirrored their model, much as Charles Harpur a century earlier had taken over poetic forms from his British predecessors. And as Harpur's circumstances and environment gave those forms new substance, so the character of Australia placed different obligations upon the ABC. Country areas in particular relied on it, and rural broadcasts assumed great importance. News and information were eagerly awaited by scattered populations with inadequate roads where great distances delayed mail services. Had Reith visited Australia in the early 1930s (he did not until 1945) he would have had ample confirmation of his faith in the value of broadcasting, and of the soundness of his humanitarian vision. Perhaps he might even have been surprised to see how firmly the ABC had taken root in the still uncertain cultural climate of a new civilization. But at that time the ties of knowledge and feeling with Britain were still strong. These ties, seen as a weakness by fervent nationalists then and now, were a strength for many Australian institutions, not only for the ABC.

It is a measure of the difference between Britain and Australia that the importance of the independence of the ABC has not been as well understood as has the BBC's. Public service broadcasting came to mean over the years broadcasting conducted under the rules of bureaucratic operations. As time went on the ABC was gradually strangled by the finicky attention of government departments, and intermittently threatened by politicians made uneasy by its independent voice. John Curtin's fine speech to Parliament in 1945 defining the difference between the national broadcaster and government agencies was seemingly forgotten:

> The Government recognises that the intent of the Australian Broadcasting Act is to create a position of special independence of judgement and action for the national broadcasting instrumentality. This is inevitably the case because of its highly delicate function in broadcasting at public expense news statements and discussions which are potent influences on public opinion and attitudes. As the legislation provides, this peculiar function calls for an undoubted measure of independence for the controlling body of the national broadcasting instrumentality which cannot be measured by the constitution of other semi-governmental boards or agencies which do not impinge on the tender and dangerous realms of moral, religious, aesthetic and political values.
>
> In the last resort, the healthy and beneficent function of national broadcasting and the maintenance of public confidence in the system must rest, in all matters touching these values, solely on the integrity and independent judgment of the persons chosen to determine and administer its policy, and not on either review by, or pressure from, any sources outside it, political or non-political. This principle holds good in spite of the necessary responsibility of the Commission to Parliament, through the Minister, for the legitimate use of its funds under the terms of the Act, and all the sections of the Act should be read in the light of the above general intent of Parliament in the establishment of the Commission.

Australians are afflicted by government activities at all levels, and the ABC's capacity to plan ahead and to develop policy initiatives was greatly hampered by the requirements that it work to the regulations of a number of government departments, and respond to numerous enquiries, most of which seemed remarkably adept at ignoring first principles. At the same time, Reith's warning about the proliferation of commercial broadcasting interests proved correct; and since these operate upon precisely that concept of audience Rieth rightly rejected, there has long been a conflict between the ideals embodied in the various Broadcasting Acts, and the realities of broadcasting in Australia. (It is interesting that the advent of the ITA in Britain seems to have brought about a 'duopoly', and that both organizations pursue similar ends from a different funding basis.) Commercial stations appeal to the materialism of Australian society, and constitute formidable opposition to the ABC. The position was greatly exacerbated by the advent of television. It was taken for granted - as it should not have been - that the ABC could continue to fulfil its obligations under the Act with one television channel, and at the same time attract a reasonable audience share against the well-funded commercial outlets. It is difficult to imagine that, without the benfit of those clearly stated and firmly implemented principles that earned the BBC a world reputation even in its early years, the ABC could from within have generated the understanding of its role and the will to fulfil it which have, in spite of its critics, marked its operations.

On 1 July 1983 the ABC became a corporation, and was released from the supervision of the Public Service Board in relation to its staffing policies. This represented a real improvement in its capacity to review its staff and to begin to develop the flexibility so long enjoyed by the BBC. Early statements from the new organisation referred to ending the dominance of BBC programmes. (Though not specifically stated, this was a reference to television, and seemed rather like complaining that Shakespeare dominated the theatre.) Since then there have been more discreet references to diversifying sources of imported programmes and increasing Australian content. But what *is* Australian content? Is it, in literary terms, Henry Lawson or Martin Boyd? Patriot or expatriate? Could it conceivably be both? To put the question

this way is to suggest that the debate about literary nationality is political, and that is probably the truth of it. The recent emphasis on popularising the ABC is, in some measure, a reference to unspoken assumption about the nature of the 'real' Australian audience.

Whatever is meant by popularising, however, it implies the pursuit of that 'mass' audience so firmly rejected by Reith. That it *does* mean this is reinforced by the adoption by the new corporation of a commercial image. Now there is much talk of those 'statistically measurable preferences' that had no part in Reith's concept of the public. The difference in outlook is instructive, and might, in its own way, reflect a deliberate, if not entirely rational, tugging at the supposed apron-strings, and a superficial response to the increase in Australia's non-English speaking migrants. In the last decade there has been a marked tendency to confuse Australia's political status (frequently misrepresented as one of dependence on Britain) with its historical legacy.

The literary model and the institutional one, as I have described the, are in conflict at the present time. The 'new directions' of the ABC are, in part, a rejection of the past and of the history of the ABC, in favour of competitiveness and a simply conceived nationalism and populism. Writers - and artists in general - represent the deeper understanding which comes from contemplation of past and present, and of the way individual lives are moulded by knowledge and experience. Both tendencies take distinctive forms in Australia, and their tensions might be productive. But it is difficult not to hope that the artistic impulse will triumph, and in doing so revitalize those institutions which need to respond to change without surrendering to mere novelty. If, to use Hal Porter's words this is 'a country with a fluctuating soul', much of its potential richness will depend on its recognizing and not dismissing that vision.

The University of Sydney,
N.S.W., 2006, Australia.

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M.Sc. Thesis Abstract (University of Sydney): A Study of Hyperiid Amphipods (Peracarida: Crustacea) Associated With a Warm Core Eddy in the Tasman Sea

Jock W. Young

Hyperiid amphipods were sampled from in and around a warm core eddy (J) in the Tasman Sea during three cruises between August and October, 1979. Samples were taken at night to a depth of 400 m using a midwater trawl (RMT-8). In total, 11,891 hyperiids representing 38 species and 10 families were caught, adding 13 new records for Eastern Australian waters. Synoptic information is given for each species on taxonomy, life history data, vertical distribution, geographic range and behavioural associations with gelatinous zooplankton. Seven species contributed 87.1% to the total. Of these, *Phrosina semilunata*, *Brachyscelus crusculum*, *Primno johnsoni*, *Phronima sedentaria* and *Phronima atlantica* were all found in significantly higher numbers outside the eddy. Only one dominant species (*Streetsia challengeri*) was abundant inside the eddy and only during August. Outside the eddy, samples were characterized by large numbers of few species while inside, samples had a more even representation of species. Cluster analysis of species composition and abundance showed broad groupings correlating with depth and temperature (August), position with respect to the eddy (September, October) and the presence of salps (September, October). It is proposed that the differences between the inside and outside hyperiid communities are due to the response of hyperiids not only to the physical environment of the eddy but also to their association with gelatinous zooplankton.

CSIRO Marine Laboratories,
G.P.O. Box 1538,
Hobart, Tas., 7001, Australia.

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Doctoral Thesis Abstract (University of Sydney): NMR Studies of the Uptake and Degradation of Peptides by Human Erythrocytes

G. F. King

The teleologically unsatisfying hypothesis that the cytosolic exopeptidases of the human erythrocyte are merely non-functional vestiges of red-cell differentiation is critically examined. The efficancy of ^{1}H spin-echo NMR spectroscopy for monitoring the kinetics of exopeptidase-catalysed reactions *in situ* in human erythrocyte lysates and intact-cell suspensions is demonstrated. The ability of this technique to simultaneously monitor the substrates and products of such reactions enabled a new procedure to be developed for determining the concentrations of reactants; this involved the calculation of *unique* NMR extinction coefficients for each sample, which obviated the need for preparation of calibration curves. Furthermore, a new, time-efficient method was developed for non-linear regression of the integrated Michaelis-Menten equation, which is an *implicit* function of the substrate or product concentration, onto reactant-versus-time progress curves for the purpose of obtaining steady-state kinetic parameters for the reactions. Derivation of the sensitivity functions for the integrated Michaelis-Menten equation revealed under what initial conditions this procedure yields the most reliable parameter estimates.

Since peptides are not products of normal erythrocyte metabolism, the work presented in the thesis examined the possibility that substrates for the cytosolic exopeptidases of these cells may arise from extracellular sources. It is shown, for the first time, that a range of di- and tripeptides may enter the human erythrocyte via *saturable* membrane transport system(s) which are describable by simple Michaelis-Menten kinetics; all such peptides are shown to be rapidly hydrolysed upon entering the intracellular milieu. However, preliminary evidence revealed that the rate of peptide uptake by human erythrocytes declines markedly as the residue-number is increased beyond three; it is consequently suggested that there may be a critical residue-number or hydrodynamic volume which determines whether peptides are permeant to these cells.

Potential physiological roles were assigned to some of these coupled (in the kinetic sense) peptide transport-intracellular exopeptidase systems. For example, it was demonstrated that glutamate, which is impermeant to human erythrocytes, may be supplied to these cells via the absorption and subsequent intracellular hydrolysis of plasma α-glutamyl-peptides. Calculations based on the experimentally-derived steady-state kinetic parameters for the uptake and hydrolytic processes revealed that the plasma concentration of α-glutamyl-peptides would only need to exceed 7.3μM for this coupled system to provide enough glutamate to sustain the observed rate of intracellular glutathione synthesis.

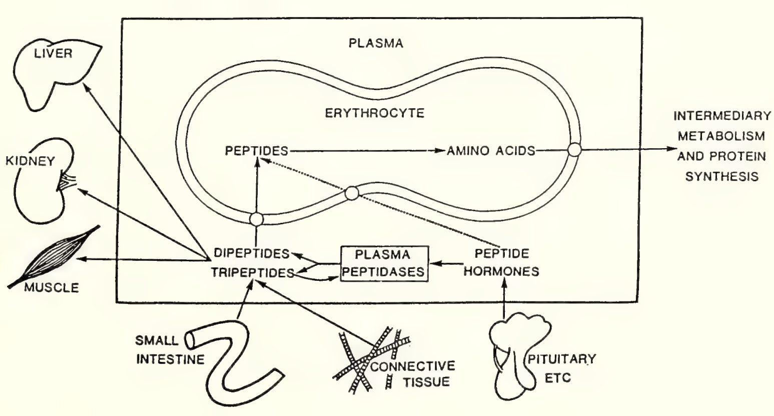

Figure 1: A schematic representation of the putative role of human erythrocytes, and other tissues, in whole-body peptide turnover. The dotted line is meant to portray current uncertainty about the exact role of human erythrocytes in the deactivation of circulating peptide hormones.

However, it was shown that human erythrocytes play only a minor role in the turnover of plasma prolyl-peptides and that absence of erythrocyte prolidase is probably not a major factor in the aetiology of the clinical manifestations of generalised prolidase deficiency. It was demonstrated that the rates of cis-trans isomerisation of imidodipeptide substrates of prolidase can be monitored using inversion-transfer NMR spectroscopy. Data obtained from such a study, combined with progress curves obtained from ^{1}H NMR experiments monitoring *all* reaction species in the prolidase-catalysed hydrolysis of L-alanyl-L-proline, enabled the isomeric specificity of the enzyme to be determined; it was shown that prolidase has exceptionally high, or absolute, specificity for the trans isomer of its substrates. This information, combined with data from the literature, enabled a new model of the active site of prolidase to be constructed.

Finally, it is proposed that human erythrocytes, by virtue of their extensive distribution, large numbers, and peptide and amino-acid transporting capabilities, may play an important role in whole-body peptide turnover by assimilating plasma peptides and distributing the hydrolytic products to other tissues (see Figure 1). The clinical value of the illicit transport of otherwise impermeant drugs into human erythrocytes by way of their peptide transport system(s) is also discussed.

Dept. of Biochemistry,
University of Oxford,
South Parks Rd.,
Oxford, OX1 3QU, England.

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Doctoral Thesis Abstract (University of Sydney): Stochastic Theories of Solution Dynamics

P. Mark Rodger

This work undertakes an investigation into the manner in which the characteristic motion of a molecule changes when it is immersed in a solvent (i.e. solution dynamics, denoted S.D.). Particular attention is paid to the rates of very fast (picosecond) reactions such as occurs in the geminate recombination of iodine, or the dihedral motion of 1,1'-binaphthyl.

The results of some molecular dynamics computer simulations (M.D.) modelling a series of monatomic and diatomic molecules in a different monatomic solvent are presented. It is found that some simple macroscopic concepts, such as solvent viscosity and surface tension, are useful on a molecular scale. Of particular interest to the reaction rate problem is the coefficient describing the friction experienced by the solute molecule, ξ. From the simulations, we have elucidated an easy-to-use three step procedure for evaluating ξ from the solvent viscosity, r : (i) an uncommon, but general form of Stokes' law is used to calculate a value of ξ that is applicable to the long timescale motion of spherical particles, viz. $\xi^\circ=3r/(d^2\ \delta\rho)$, where d is the solute diameter, and $\delta\rho$ is the difference between the solute and solvent densities, (ii) changes in molecular shape were found to induce changes in ξ° that were proportional to the corresponding changes in the cross-sectional area of the solute, and (iii) a functional form for the frequency dependence of ξ due to M.G. Sceats and D.P. Millar [to be published] was found to give a good description of how the frequency of the solute motion modifies ξ°.

An analytic theory of S.D. that incorporates the dynamic structure of the solvent in terms of a locally time dependent friction coefficient is developed. This theory is combined with the computer simulations already cited to provide the basis for an extensive discussion of the existing theories, with particular emphasis on the level of complexity required to adequately explain the experimental observations. A rationale is thereby proposed to explain why the simplest model (variously referred to as the Brownian motion, Langevin, Fokker-Planck, or Kramers formalism) appears to work far better than theory suggests it should.

In the remainder of this thesis, two extensions to the Brownian theory are presented. The first of these allows for the inclusion of long range solute-solvent interactions, which may, in principle, be very sensitive to the conformation of the solute. A model is proposed, that leads to an analytic expression for the rate of escape across a potential barrier under the influence of such long range interactions; by way of example, this is applied to the problem of dihedral motion in 1,1'-binaphthyl. The second extension is a procedure by which the number of degrees of freedom in a system may be

reduced by the incorporation of an effective potential energy surface. The method is applied to the relative radial motion of diatomic molecules, as occurs, for example, in the geminate recombination of iodine; the three dimensional problem is found to reducible to a one dimensional problem by supplementing the intramolecular potential with a -2kT*ln* r term. It is conjectured that this procedure is of general validity, and is not restricted to the Brownian model; this conjecture is shown to be supported by the M.D. simulations of this work.

University Chemical Laboratory,
Lensfield Road,
Cambridge, CB2 1EW, England

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Doctoral Thesis Abstract (University of Sydney): Symmetry Selection Rules Analytic Development and Chemical Application

ALISON RODGER

This work is concerned with the development and application of symmetry selection rules for systems with point symmetry. Relationships between point groups are developed which enable the full structure of a point group to be expressed in terms of relationships between a limited number of generating operations. These relationships are used in two different ways. The first begins with the development of a group augmentation procedure, whereby a group is built up from one of its subgroups by augmenting the subgroup with a new operation. The augmentation procedure then enables tensorial invariants of arbitrary order to be analytically determined for all point groups. The Generalized Selection Rules of P.E. Schipper (GSR) (which require firstly the determination of symmetry invariant operator products and secondly the determination of symmetry invariant matrix elements) can now be implemented. Particular application of the GSR is made to expressions derived for the circular dichroism (CD) of magnetic dipole allowed (mda) transitions in systems where the transition of interest is essentially localized on an achiral chromophore.

The theoretical analysis of mda CD proceeds by using perturbation theory to express the CD induced into a non CD active transition by the presence of a chiral species. When the interaction is written as a multipole expansion, the induced CD appears as a sum of terms involving different multipole transition moments. Each term is then analysed using the GSR. The results of this analysis are used firstly to understand experimental data to be found in the literature, about which there has been much controversy. They are then coupled to the first systematic experimental study of systems in which the CD results from the presence of chiral species which (i) do not preferentially associate and (ii) do preferentially associate to the achiral chromophore. The achiral systems studied were Co(III) metal complexes, and a range of monosaccharides were chosen as the chiral species for (i), and tartaric acid for (ii). The metal complexes were chosen to have a wide range of symmetries so as to comprehensively test the theoretical results which had been developed.

The second way in which the expression of group structure in terms of relationships between generating operations is used, is in the derivation of matrix irreducible representations (i-r's) of cyclic, dihedral and cubic groups. I-r's are not available in the literature for any but the simplest point groups despite their well-known utility for projecting out orthogonal symmetry adapted functions. Another application for which the i-r's are well-suited is the analysis of the symmetry changes a system undergoes in the course of molecular vibrations or geometry changes. In this work this is used to analyse relationships between: (i) symmetry changes and geometry changes, and (ii) between geometry changes and the potential energy of a system; a straightforward symmetry selection rule procedure for generating symmetry allowed reaction mechanisms (i.e. those not precluded by symmetry) is thereby developed. Some attention is also paid to the determining the most probable of the symmetry allowed mechanisms.

Throughout the thesis the philosophical approach taken is a two step one of firstly analytically incorporating and fully exploiting the symmetry of the system, and secondly, where it proves necessary, to determine details by calculation or experiment.

Newnham College,
Cambridge, CB3 9DF, England.

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Doctoral Thesis Abstract (University of Sydney): NMR Studies on the Intracellular Viscosity and Cell Volume of Human Erythrocytes

ZOLTÁN H. ENDRE

Viscosity estimates were made from correlation times calculated from ^{13}C NMR measurements of the spin-lattice relaxation time (T_1) of a constant low concentration of [α-^{13}C]glycine in BSA and glycerol solutions and in intact human erythrocytes. The estimates in erythrocytes were compared with those obtained from T_1 measurements of [glycyl-α-^{13}C] glutathione labelled *in situ* in intact erythrocytes, by utilising the glycine-exchange reaction catalysed by glutathione synthetase. The modulation of glycine exchange was studied in order to optimise incorporation of the ^{13}C-labelled glycine into GSH.

Identical estimates of NMR-glycine-viscosity were obtained when either isotropic sherical rotor or prolate ellipsoidal rotor models were used to calculate probe correlation times from the T_1 measurements. Comparison of the estimates of NMR-viscosity obtained using [α-^{13}C]glycine in free solution with measurements of the bulk viscosity obtained using capillary viscometry demonstrated that : 1) with increasing solute concentration there was progressive deviation of the estimates, and 2) different values of the NMR-glycine-viscosity were obtained in BSA and glycerol solutions with the same bulk viscosity.

The deviation of the NMR- from the bulk viscosity values was found to result from the progressive deviation with increasing viscosity of the measured probe correlation times from the theoretical values predicted for an "ideal" solution. By application of mathematical expressions defining the concentration-dependence of the viscosity of *concentrated* solutions it was possible to calculate phenomenological interaction parameters which allowed both NMR- and bulk viscosity estimates to be defined as functions of the volume fraction of solute. These interaction parameters were then used to quantify the relationship between bulk and NMR-glycine-viscosity estimates phenomenologically, *i.e.*, without the need to consider the nature of the probe-solute-solvent interaction (Figure 1). The method developed here, for calculating bulk from NMR-viscosity and *vice versa*, is applicable to other methods of investigation where correlation times are studied, *e.g.*, ESR. A simple geometrical model, based on the relative sizes of probe and solute molecules, was proposed to explain the differences between the estimates of NMR-viscosity obtained in solutions containing different solutes but with the same bulk viscosity.

The similarity between the estimates of NMR-viscosity, which were obtained in intact erythrocytes by using either [α-^{13}C]glycine or [glycyl-α-^{13}C]GSH as physiological viscosity probes, indicated that the NMR-glycine-viscosity estimates were obtained from intra- not extracellular glycine. Both probes yielded estimates of the NMR-viscosity of ~1.4 mPa.s at 37^{o}C, indicating that the restriction to motion of these small molecules in erythrocyte cytoplasm was ~1.8 times that of isotonic saline. Estimates of NMR-glycine-viscosity in intact erythrocytes were calibrated *in situ* against the intracellular haemoglobin concentration by obtaining viscosity estimates at different cell volumes. It was then demonstrated that cell volume could be estimated from the viscosity measurements and *vice-versa*. This allowed the physiological range of NMR-glycine-viscosities (~1.1 to ~1.7 mPa.s) to be calculated from the physiological range of erythrocyte volumes.

Consideration of the physiological range of NMR-glycine-viscosities indicated that: 1) increases in intracellular viscosity resulting from observed increases in the concentration of haemoglobin are unlikely to account for the experimentally observed decline with age in the activity of erythrocyte superoxide dismutase, 2) these age-dependent increases in intracellular viscosity markedly increase the calculated bulk viscosity of erythrocyte cytoplasm and that the resulting increase in erythrocyte rididity could contribute to he splenic clearing of senescent erythrocytes from the circulation, and 3) increases in NMR-glycine-viscosity associated with physiologically or pathologically-induced changes in erythrocyte volume are unlikely to affect the rates of very fast reactions unless the substrates of these reactions are present in "limiting" concentrations.

The effect of changing intracellular viscosity on the signal intensity of resonances in spectra obtained using ^{1}H NMR was studied by varying the volume of human erythrocytes and obtaining spectra by using either the spin-echo or inversion-recovery spin-echo pulse sequences. In additional studies measurements of T_2 and volume magnetic susceptibility were also performed. These studies demonstrated that marked changes in signal intensity of resonances from intra- and extracellular species occurred as cell volume varied. The changes in signal intensity could not be explained by changes in intracellular viscosity or volume magnetic susceptibility; however, they could be accounted for by variation in the homogeneity of the magnetic field in the erythrocyte suspensions consequent upon the changes in erythrocyte shape which follow alteration of erythrocyte volume. The field inhomogeneity in intra- and extracellular compartments of erythrocytes at different cell volumes was estimated in terms of the average field gradients in these compartments.

In other ^{1}H NMR studies extracellular viscosity and volume susceptibility were altered. These latter studies demonstrated that: 1) signal intensity in spin-echo spectra was a function of the magnitude of both the viscosity and of the magnetic field

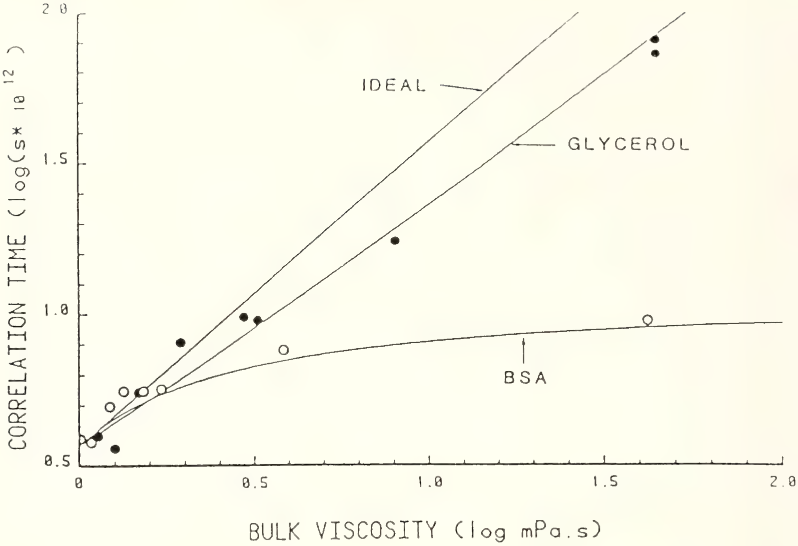

Figure 1. [α-^{13}C]glycine correlation time as a function of normalised bulk viscosity. Temperature, 37^{o}C. Open circles, viscosity adjusted with BSA; solid circles, viscosity adjusted with glycerol. The line marked "ideal" is a plot of $\tau_R = 4\pi\eta r_o{}^3/3kT)$ with r_o ($1.583 * 10^{-8}$ cm) determined from the solution of lowest (normalised) viscosity. The log-log plot was used for compression of the axes. The lines fitted to the empirical data were calculated similarly but using a theoretical function for η such that $\eta_N = \eta_B{}^{\psi}$, where $\psi = \nu_N/\{\nu_B + \ln(\eta_B)[k_B - k_N]\}$. Thus the first equation becomes $\tau_R = (4\pi r_o{}^3/3kT) * \eta_B{}^{\psi}$ which allows τ_R to be calculated in terms of the bulk viscosity using the interaction parameters ν_N, ν_B, k_N , and k_B, which are easily estimated using non-linear least-squares analysis of normalised viscosity-ϕ data.

gradients, 2) matching intracellular susceptibility by adding bovine serum albumin to the extracellular compartment could enhance the intensity of resonances in the intracellular compartment, 3) the magnitude of intracellular field gradients in suspensions of erythrocytes at high haematocrit was determined principally by the field gradients at the external boundary of nearby cells and not those generated within the individual erythrocytes, and 4) the dephasing effects of the field gradients and the subsequent echo attenuation were functions of the restriction to both rotational and translational rates of diffusion calculated *not* from the value of the bulk viscosity of the solution but from that of the NMR-viscosity experienced by the individual molecular species.

Department of Biochemistry,
University of Oxford,
South Parks Road,
Oxford, OX1 3QU, England.

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Doctoral Thesis Abstract (University of Sydney): Acute and Chronic Effects of Lead and Other Metals on GABA Neurochemistry

COLLEEN ANN DREW

The acute and chronic effects of lead salts on central systems related to the neurotransmitter γ-aminobutyric acid (GABA) have been studied (*in vitro*) in untreated adult rat brain and in brains from neonatal and young adult rats following the chronic administration of lead to dams from conception and pups from weaning. Lead was shown to affect GABA systems *in vitro* and to have consistent, binding site specific effects in the chronically treated animals.

1. As divalent lead cations are readily complexed by some of the anions used to buffer physiologcal media, the role played by the choice of buffer in variability of reported results was investigated. The effect of lead and other metal ions on the uptake, release and binding of $[^3H]$-GABA in adult rat brain was studied using buffers with varying capacity to complex divalent ions.

Minimal effects on uptake and release were seen in solutions buffered with either phosphate or carbonate (which complex metal cations) whereas marked effects were observed when Tris-HCl (which does not form stable metal complexes) was used as a buffer.

The effects of lead and other metal salts on calcium-dependent, bicuculline-insensitive ($GABA_B$) and calcium-independent, bicuculline-sensitive ($GABA_A$) $[^3H]$-GABA binding to synaptic membranes of adult rat wholebrain were investigated using Tris-citrate and Tris-HCl buffers. As for the transport studies minimal metal effects were seen in the buffer which complexes divalent cations (Tris-citrate), whereas marked (ion specific) effects were seen when assays were carried out in the Tris-HCl buffer. Calcium-dependent bicuculline-insensitive/$GABA_B$ GABA binding sites were found to be more susceptible to the effects of lead and some metal ions than calcium-independent bicuculline-sensitive/$GABA_A$ sites.

2. The binding of $[^3H]$-(-)-baclofen (a stereospecific agonist for the calcium-dependent, bicuculline-insensitive/$GABA_b$ GABA binding sites), to synaptic membranes of rat cerebella was investigated. $[^3H]$-(-)-Baclofen showed increased specific binding, a higher affinity and lower receptor density than equivalent studies $[^3H]$-(±)-baclofen.

The kinetics of bicuculline-insensitive $[^3H]$-GABA binding to cerebellar membranes and the effects of some conformationally-restricted analogues of GABA on this and $[^3H]$-(-)baclofen binding indicated that folded analogues of GABA may interact with a class of GABA binding sites insensitive to both bicuculline and (-)-baclofen.

3. The development of calcium-dependent bicuculline-insensitive/$GABA_B$ (forebrain and cerebellum and calcium-independent bicuculline-sensitive/$GABA_A$ $[^3H]$ GABA binding (forebrain) at 10, 21 and 84 days postnatally, and the effect of chronic treatment with lead on this development was studied in neonatal animals whose dams received either lead acetate or sodium acetate in their drinking water from conception to weaning and normal animals given only water.

The developmental profiles of the normal animals, for both calcium-dependent bicuculline-insensitive/$GABA_B$ and calcium-independent bicuculline-sensitive/$GABA_A$ binding of the normal animals were similar, showing relatively constant affinity but greatly increased receptor density from birth to 21 days postnatally.

4. Chronic lead treatment produced receptor specific effects on binding to the different sites during development. Calcium-dependent, bicuculline-insensitive/$GABA_B$ binding was reduced during this period in a manner consistent with a decrease in synaptogenesis but appeared to have recovered at maturity. Calcium-independent, bicuculline-sensitive/$GABA_A$ binding, while unaffected during development was increased in the adult animals suggestive of 'acute' rather than structural lead effects.

Department of Pharmacology,
University of Sydney, NSW, 2006, Australia.

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Annual Report of the Council for 1985: Part 1

Awards

CLARKE MEDAL

HUGH BRYAN SPENCER WOMERSLEY

Professor H.B.S. Womersley, PhD DSc (Adelaide), FAA, is distinguished for his research on the botany of the marine plants of Australia. He has studied many aspects of the seaweeds. His early papers were on the intertidal ecology of the South Australian coasts and these were followed by taxonomic and biogeographical studies. He has done extensive work on life history and cultural studies of the marine algae, including the three groups Chlorophyta, Phaeophyta and Rhodophyta. He developed an active school of research in phycology at the University of Adelaide and trained many research workers in this field.

He has had a distinguished career in teaching, research and administration in the University of Adelaide where he has been Professor of Botany since 1974. He has a considerable international reputation and was a member of the Council of the Internation Phycological Society from 1966 to 1971, President in 1970. He was President of the Royal Society of South Australia from 1966 to 1967. Previous recognitions of his work were the Edgeworth David Medal of the Royal Society of N.S.W. (1955), Verco Medal of the Royal Society of South Australia (1969) and election as a Fellow of the Australian Academy of Science (1974).

The award of the Clarke Medal for 1985 is a fitting acknowledgement of his outstanding achievements, particularly in the field of marine botany.

Rutherford Robertson.

COOK MEDAL

DONALD METCALF

Dr. Donald Metcalf, AO, MD, BS, BSc(Med), FRCPA, FRACP, FAA, ERS, is an outstanding Australian medical scientist. He is currently Head of the Cancer Research Unit and Assistant Director of the Walter and Eliza Hall Institute, positions which he has held since 1965.

He graduated from the University of Sydney and spent his internship at the Royal Prince Alfred Hospital in 1953.

As well as his appointment as Associate Professor of Pathology at Harvard University (1956-58) he has held numerous visiting appointments at leading overseas institutes and universities. He has been visiting Professor at the State University, New York at Buffalo (1966-67), a visiting scientist at the Swiss Institute for Experimental Cancer Research (1974), and the Radiobiological Institute Rijswijk, Netherlands (1980), and a Visiting Fellow at Clare Hall, Cambridge (1981). He has written numerous articles in scientific journals on leukaemia and cancer. Contributions to the scientific literature are collated and presented in his books on "The Thymus" 1966). "Haemopoietic Cells" (with M.A.S. Moore) (1971), and "Haemopoietic Colonies" (1977).

The outlook for survival of patients with certain forms of leukaemia and other associated types of malignancy has been altered for the better, dramatically, over the past twenty years. This has come about by increasing knowledge of the fundamental changes which occur in leukaemia and appropriate adjustment of therapeutic approach.

Dr. Metcalf has contributed greatly to this advance and is a researcher of whom Australia can be justly proud. He is a worthy recipient of the James Cook Medal.

Bruce A. Warren

THE SOCIETY MEDAL

DALWAY JOHN SWAINE

The Society's Medal for contributions to the Society and to geochemistry is awarded to Dr. Dalway John Swaine, M.Sc. (Melbourne), Ph.D. (Aberdeen), FRACI.

Dr. Swaine joined the Society in 1973. He served on the Council from 1974 to 1979 and again from 1985, and was President in 1976. His Presidential Address was entitled "Lead in the Environment".

Dr. Swaine was awarded the degrees of B.Sc. and M.Sc. in chemistry at the University of Melbourne. He then travelled to the Macaulay Institute for Soil Research, Aberdeen, where he received a Ph.D. for his research on the distribution of trace elements in soils. He joined the staff of the Macaulay Institute and continued this research which is still regarded as the definitive study of the subject. He returned to Australia in 1959 to work for CSIRO, where he was a Chief Research Scientist and leader of the Geoscience Section of the Division of Fossil Fuels. He has pioneered research on various aspects of trace elements and is recognised as a world authority on trace elements in coal. He retired from CSIRO in 1985 and is now a Professorial Fellow in Inorganic Chemistry at the University of Sydney and an Honorary Research Fellow in CSIRO. In addition to scientific papers in Australian and overseas journals, he has published two books on trace elements in soils and in fertilisers, and is co-editor of a book on bio-geochemical cycling of mineral-forming elements. Among his interests, Dr. Swaine lists lecturing in science. Indeed, he is much in demand both as lecturer and collaborator in Australia and overseas.

For his contributions to geochemistry and to the Royal Society, Dr. Swaine is a very worthy recipient of the Society's Medal.

EDGEWORTH DAVID MEDAL

The Edgeworth David Medal is awarded for distinguished contributions by young scientists, under the age of 35, for work done mainly in Australia. Two Edgeworth David Medals are awarded in 1985, to Dr. Simon Charles Gandevia and to Dr. Brian James Morris.

SIMON CHARLES GANDEVIA

Dr. Simon Gandevia was born in Melbourne in 1953. He was educated at Sydney Church of England Grammar School and the University of New South Wales where he was awarded a B.Sc. in 1975 and MB, BS in 1980. He was awarded a Ph.D. in 1978 for a thesis on "Sensations of heaviness in man", and a M.D. in 1985 for a thesis on "Properties of the cortical projection of cutaneous and muscle afferents in man". He is now a Research Fellow in the Unit of Clinical Neurophysiology at the Prince Henry Hospital. Dr. Gandevia is an exceptional medical scientist with an international reputation. His work encompasses several important areas of human motor control. He has contributed significantly to current understanding of the neurophysiological mechanisms underlying kinaesthetic sensation. He has investigated the neural basis for the clinical investigation of somatosensory potentials and his results here are of major importance for the diagnosis of neurological disorders. In collaboration with Dr. Burke, he has undertaken important studies of human motorneurones and their reflex control. This work has overthrown widely held beliefs about the human tendon jerk. He has also a long-standing reputation as an expert on breathlessness. In summary, Dr. Gandevia is a medical scientist of high calibre who has already made fundamental contributions to our understanding of several facets of human motor control. He is a very worthy recipient of the Edgeworth David Medal.

BRIAN JAMES MORRIS

Dr. Brian Morris was born in 1950. He received his B.Sc. from Adelaide University in 1972 and his Ph.D. from Monash University in 1975 for biochemical studies of the renin-angiotensin system. He spent the next three years in the United States as a Sir Charles James Martin Research Fellow, working at the University of Missouri and than at the University of California. He returned to Australia in 1978 and he is now Head of the Molecular Biology and Hypertension Laboratory at the University of Sydney. Dr. Morris has made significant contributions to science in Australia, particularly in the field of hypertension, a major contributing factor to cardiovascular diseases which are the cause of death of half of our citizens. Hypertension is most likely an endocrine disorder and the most widely studied hormonal system in hypertension, and indeed in the whole of medical science, has been the renin system. Dr. Morris at the outset of his career was the first to discover a precursor activated by proteases which turned out to be present in enormous concentrations in human plasma and led eventually to his discovery of the manner in which the body makes renin and its gene. This work was based on new genetic engineering techniques. He has made important contributions to many aspects of hypertension research, particularly to the basic biochemistry of the systems involved. His recent discoveries include the isolation and sequence of a human kallikrein gene and of human cardiac myosin heavy chain genes. Dr. Morris' major scientific contributions justly merit the award of the Edgeworth David Medal.

Obituaries

WILLIAM BRODERICK SMITH-WHITE

William Broderick Smith-White died on 8th February, 1986, aged 76, after an illness lasting just over a year. He was a fine mathematician, a helpful and friendly colleague, a considerate and careful teacher and one of the kindest of people. His colleagues and the generations of students taught by him will remember him with affection and respect.

Bill Smith-White's long association with Sydney University dates from his undergraduate days and it is of interest to record his education and career in some detail. He attended Petersham Superior Public School and then Hurlstone Agricultural High School (at that time located in Summer Hill on the present site of Trinity Grammar School) where he gained his Intermediate Certificate in 1923. He went to Fort Street Boys' High School for his Leaving Certificate, at which he obtained first-class honours in Mathematics and Physics and was awarded an exhibition to enter Sydney University, which he did in 1926. Initially enrolling in Agriculture, the requirement to dissect a frog in the first week of Zoology caused Bill to transfer to Science, where he distinguished himself in Mathematics and Physics, gaining a First in both - Mathematics in 1928 and Physics (with the University Medal) in 1929. Mathematics had only a 3-year Honours course then, but Physics had already introduced a 4-year Honours course. He was awarded the Barker Travelling Scholarship in Mathematics and, following the pattern laid down and insisted upon by Carslaw, went in May 1930 to Emmanuel College, Cambridge, to settle in over summer preparatory to beginning the Mathematics Tripos Part II in Michaelmas Term. Then, the present Parts II and III were labelled Part II Schedules A and B respectively and were examined together. Bill again distinguished himself, emerging as a "B star Wrangler" in 1932.

Despite this excellent result, there was no prospect of his gaining any scholarship sufficient to provide support for staying on in Cambridge (College scholarships were less in value than the cost of College living) and it is probable that financial reasons alone forced him to return to Australia. The prospect at home, during the Depression, was bleak. Carslaw had nothing to offer, there was no other academic work available and, lacking teaching qualifications, Bill was unable to seek a job in the Education Department. Eventually he found work in the actuarial section of an insurance company, supplementing his pay with fees from coaching. This was a depressing experience for someone eager to continue with mathematics and to begin research (there were no graduate scholarships, nor any research students, until much later). He resigned his job at the end of 1933, living on fees earned from coaching and from part-time work giving evening mathematics lectures at the University. At this time there were many evening students and young teachers studying for a degree, and the mathematics staff consisted of four people, so if anyone was away for any reason, help was sought. Carslaw was ill in the latter part of 1934 and Bill was then also given Carslaw's day lectures - mainly to Honours classes.

In 1935, Bill accepted a two-year appointment as resident tutor at Trinity College, Melbourne - at that time perhaps the only university college Australia employing such people full-time.

T.G. Room, who replaced Carslaw in late 1935, immediately requested additional staff and was granted an Assistant Lectureship (the first increase in departmental establishment since 1920), which Bill was offered and took up in 1937. This was an annual appointment - tenure started at the Lecturer level.

The war years brought several changes. Some senior staff were transferred to military intelligence duties and new people brought in as replacements. The University introduced a "summer term" for Science and Engineering students at the end of 1942 and 1943, to accelerate course completion and thus contribute to the war effort. By 1943, inflation was noticeable and changes were made to conditions of appointment and to salaries. Assistant Lecturers were given tenure, renamed Lecturers, and their salary range increased from (£300 - £500) to (£40C - £600). Bill thus became a Lecturer and continued in this capacity, sharing the enormous teaching load with his four colleagues, until the war ended. This brought only a brief respite, because the Commonwealth Reconstruction Training Scheme (CRTS) brought the 'big rush' to university of those wishing to continue or to begin further education. Thus began the era of the Wallace Theatre and large classes and the need for more teaching staff. Bill, who was equally capable in Physics as in Mathematics, was saved from the temptation of a Senior Lectureship in Physics by being promoted to Senior Lecturer. The Murray Report brought the second expansion of the universities at the beginning of the 1960s and in 1961 Bill was promoted to Associate Professor, a position which he held until his retirement in 1974.

Such, then, was the academic background within which Bill Smith-White developed his own mathematical interests in the broad field of analysis and its applications, ranging from classical physics and partial differential equations to analytical number theory. He was a perfectionist and published relatively little; many of his ideas were enshrined in his advanced lecture notes on differntial equations, electrostatics and other subjects. His lectures in the forties ranged widely over pure and applied mathematics. During the war years 1941-44, he also gave lectures to trainee radar officers on

the radiation from complex aerial systems - a difficult topic and his treatment of it was exceptionally well received by the successive classes. For much of the fifties and sixties he shouldered practically single-handed all the teaching in analysis. He read widely in mathematics and his knowledge of classical analysis was legendary. He became well known as a person to consult, both inside and outside the Mathematics Department. His lectures were prepared with a craftsman's care and skill. He had the gift for finding the right way of looking at things and making his subject seem simple and natural. The same gift contributed to his skill in solving problems. There was an openness and simplicity about his character, which seemed to carry over to his work, and led him to eschew any presentation which obscured the mathematical essentials. For example, he found many of the "elementary" proofs of the prime number theorem not completely to his taste and eventually devised an elegant presentation based on the Moebius function.

Outside the University he took an active interest in school Mathematics in New South Wales and in the work of the N.S.W. Branch of the Mathematical Association, serving on its Executive Committee (including a term as President in 1949-50) and its Editorial Committee from its inception in 1945 (the year the *Australian Mathematics Teacher* first appeared) until 1953, contributing several articles to this journal, including a tribute to Carslaw's work in analysis. For the Leaving Certificate Examination, he acted as Assessor under the successive Chief Examiners Wellish (who followed Carslaw) and Room, and as Chief Examiner in those years when Professor Room was away. He served as a University representative on the Mathematics Syllabus Committee from 1937 until his retirement in 1974. He opposed strongly the introduction of some aspects of the "New Mathematics" in the 1960s - aspects which in his view brought in new topics at a superficial level, replaced plain use of language by jargon, and obstructed the natural development of mathematical understanding. Nothing since then has proven him wrong in thos matters. Bill's friendship with Harry Mulhall, which began in the 1940s, and their mutual interests in the problems of teaching, eventually resulted in the production of "Mulhall and Smith-White" - four books on senior secondary mathematics - copies of which are now zealously guarded and used by those schools who realised their great value in stimulating interested and able students.

Bill joined the Royal Society of New South Wales in 1947 and participated actively in its scientific program as well as in the work of the Society. He served two five-year terms on its Council, 1960-65 and 1971-76 and held office as President in 1962 and Vice-President in 1963-64. He contributed four papers to its Journal and Proceedings and in 1983 received the Society's Medal for 1982, awarded for his contributions to mathematics and to the Society. He joined the Australian Mathematical Society in 1957, soon after its founding and contributed to its Journal.

He married his wife, Sheila, in 1940, thus beginning a long and happy partnership. They had four children and the death of Richard, the eldest, in 1967 was an unexpected and saddening misfortune. His twin brother, Spencer, enjoyed a distinguished academic career as a geneticist, also at Sydney University.

To Bill's wife, Sheila, and to their children, Rosalind, John, and David, remain memories of a kind, friendly, and scrupulously honest man and the respect of many colleagues and students for a fine and helpful mathematician and teacher.

John Mack.

DAPHNE HAYES

Daphne Hayes (nee Goulston), a pioneer in cancer research in Australia, was born in Glebe on 5th April, 1905, and died on 27th June, 1985. She enrolled in Science in the University of Sydney in 1923, and received her B.Sc. with First Class Honours and the University Medal in Physiology in 1927.

She worked as a Research Fellow in Biochemistry with the Cancer Research Committee at Sydney University from 1928 to 1934. During this time she was awarded a Rockefeller Foundation Grant and worked at the Radium Institute in London (with Dr. Mottram) during 1931 and 1932. She was also a Research Assistant with Professor Henry Priestley at Sydney University during 1946 - 1949.

In 1943 Daphne Hayes was elected a member of the Royal Society of New South Wales, and later became a Life Member.

Her research was notable for its meticulous attention to detail and its incisiveness; she had particular success in correcting flaws in established precepts.

The Daphne Goulston Prize is to be awarded to the best student at the end of third year in the Bacteriology Department of the University of Sydney. It is set up in memory of Daphne Hayes (nee Goulson) by her family from a bequest she made to the University.

NORBERT THOMAS WRIGHT

Dr. Norbert Wright, former Chief Dental Officer of the New South Wales Department of Health and a member of the Royal Society of New South Wales for ten years, died on 28th October, 1985, after a twelve month battle against cancer. The following tribute is reprinted in part from the Australian Dental Association, New South Wales Branch's "Newsletter", by kind permission of the Editor.

Norbert Wright graduated Bachelor of Dental Surgery, University of Sydney in 1950 and was in private practice in Newcastle before practising in England in 1953. He returned to Australia in 1954 to take up Itinerant School Dental Duties in Sydney and Newcastle. He subsequently served as a School Dental Officer, Principal Dental Officer - Newcastle Health Region and Relief Institutional Dental Officer in Penal, Schedule V and Child Welfare Establishments.

In 1971 he was appointed Deputy Director, Division of Dental Services and in 1975, Principal Dental Adviser to the Health Commission of New South Wales, later retitled Director of Dental Services. In 1983 he became Chief Dental Officer, Department of Health, New South Wales.

Norbert's Army service saw him commissioned in the rank of Captain in 1966 and appointed to 41 R.A.A.D.C. Army Reserve. He was seconded to the Royal Australian Army Service Corps (16 Company Amphibians), Newcastle in 1968. In 1970 he was appointed to 2 Dental Unit and in 1979 was promoted to Major at the R.A.A.D.C. Directorate, Canberra. He was placed on the Retired List in 1980.

Dr. Wright joined the Australian Dental Association in 1951 and was actively involved in its activities. He was a member of the Newcastle Division from 1965-69 as a member, Treasurer and Vice-Chairman. He served on the Dental Auxiliaries, Dental Care of the Handicapped and Faculty Liaison Committees of the New South Wales Branch.

On the national scene, he was a member of the Australian Dental Services Advisory Committee and an executive member of the Dental Health Education and Research Foundation for fifteen years.

He campaigned strongly for the introduction of fluoridation of New South Wales water supplies and was Chairman of the Fluoridation of Public Water Supplies Advisory Committee.

He is survived by his wife, Elaine and three children, to whom we extend our deepest sympathy and condolences.

TREVOR TAYLOR

Trevor Taylor and his wife Norma were killed in a traffic accident near Sydney on Saturday, 19th October, 1985. A member of the Standards Association since 1969, Mr. Taylor had brought to it a great wealth of experience in many facets of the fields of basic engineering. As an Executive Officer of the Mechanical Engineering Group of the Association he was responsible for several important committees, inclusing Engineering Tolerance Systems Metrology Surface Quality (ME/27), Screw Threads (ME/28), Fasteners (until 1981) (ME/29), and Technical Drawing (from 1981) (MS/32).

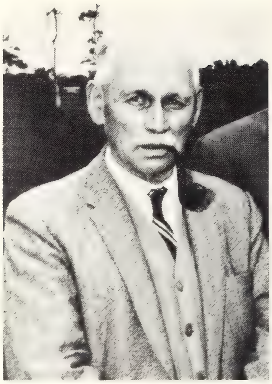
In addition, for over a decade Trevor was the Executive Secretary for ISO/TC 3, Limits and Fits, (the Secretariat of which is held by Australia) and he brought to that task a singular determination and dedication that won him worldwide respect and admiration.

Not only did he assist in preparing the ground for the international standardization of metrology, he also brought immense credit upon his adopted country of Australia with his efforts in the regeneration of activities and interest in this field worldwide.

Trevor Taylor was elected a member of the Royal Society of N.S.W. in 1979. A man of commanding stature, he had a quiet and unassuming manner and was always ready to advise and assist his colleagues. He was unquestionably an acknowledged expert in his chosen fields and his loss can only be described as almost irreparable. He died only a few months after returning from Europe after meetings concerned with ISO/TC3 and at that time was making the necessary arrangements for handing over the Secretariat to Germany F.R.

Trevor and Norma Taylor are survived by their sons Neil and Mark, to whom the Society extends its deepest sympathy.

TORRENCE EDWARD KITAMURA



Torrence Edward Kitamura, "Kit" as he was affectionately known to his many friends and contemporaries, passed away on 1st January, 1986, aged 85. He was the son of a Japanese wool buyer. After graduating B.A. at Sydney University in 1923, he took up farming in the Trundle District of New South Wales. As a result of breaking his leg in a farming accident, he met his future wife, then a a nursing sister, and began a happy family liaison which lasted over fifty years. They had five children.

In 1931 "Kit" joined 2nd Year Agricultural Science at the University of Sydney and graduated B.Sc. Agr. in 1934. He took up teaching at All Saints College, Bathurst, and joined the Department of Agriculture in 1938 at Bathurst. He was later transferred to Temora Experiment Farm as a Plant Breeder and bred the wheat varieties Brolga and Curlew, establishing the tradition of naming new Australian Wheats after the names of Australian birds. Subsequently, he became Manager of the Temora Experiment Station and finished his Departmental career as Special Agronomist Miscellaneous Crops.

Torrence Kitamura gave a talk to the Royal Society of New South Wales at a monthly meeting on 4th September, 1963. It was entitled "Cotton Production in N.S.W." He joined the Society in 1964 and was a member of Council from 1965 to 1968, and Vice-President in 1969 and 1970. He also took a keen interest in the Sydney University Agricultural Science Faculty, and acted as President of the S.U.A.G.A. for several years in his later years.

In addition to his contributions to education and agriculture, "Kit" will be remembered for his practical assistance to his mates during his university course, for his fund of good stories, invariable good humour and his faithful upholding of the best traditions of his joint Japanese-Australian heritage.

P.F. Hanley.

NOTICE TO AUTHORS

A "Style Guide to Authors" is available from the Honorary Secretary, Royal Society of New South Wales, PO Box 1525, Macquarie Centre, NSW 2113, and intending authors *must* read the guide before preparing their manuscript for review. The more important requirements are summarized below.

GENERAL

Manuscripts should be addressed to the Honorary Secretary (address given above).

Manuscripts submitted by a non-member must be communicated by a member of the Society.

Each manuscript will be scrutinised by the Publications Committee before being sent to an independent referee who will advise the Council of the Society on the acceptability of the paper. In the event of rejection, manuscripts may be sent to two other referees.

Papers, other than those specially invited by Council, will only be considered if the content is substantially new material which has not been published previously, has not been submitted concurrently elsewhere, nor is likely to be published substantially in the same form elsewhere. Well-known work and experimental procedure should be referred to only briefly, and extensive reviews and historical surveys should, as a rule, be avoided. Letters to the Editor and short notes may also be submitted for publication.

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Inorganic and Mineral Structures Reconsidered*

B. G. Hyde

ABSTRACT. For 60 years or so the "Ionic Model" has been fundamental to solid state chemistry and mineralogy. It has been useful, but the ideas involved have become sacrosanct, even when they do not work! Quantum mechanical methods are becoming increasingly important and useful, but they lack the simple "physical" approach and, in any case, so far can only be applied to the simpler structures.

An alternative approach, as simple, naive and "physical" as the ionic model, is successful where the latter succeeds and where it fails (e.g. in silicates). It can often be useful for simple and complicated structures; and it avoids the ionic/covalent dichotomy. Like the successful quantum methods, it sees no difference in principle between non-molecular structures and those of small molecules (another unhappy dichotomy). It emphasises that, as in organic chemistry, one "size" for an atom is insufficient for understanding structure; at the crudest level one needs a bonding size (for first nearest neighbour interactions) and a non-bonding size (for second and further neighbours).

The usefulness of this alternative approach is demonstrated in several areas of interest, particularly to the chemist and mineralogist: in (a) determining crystal structures and coordination numbers therein, (b) its effect on the stability/instability (and even non-existence) of simple compounds such as binary oxides, nitrides and carbides, (c) leading to simple descriptions of the structures of some mundane compounds such as sulphates, silicates and carbonates, previously undescribed, but (d) often of interest to physicists because their structures may be incommensurably modulated, (e) accounting for the effect of high pressure on crystal structure.

It transpires that cations, far from being small in size and influence, often dominate crystal structure and behaviour.

PREFACE

It is an honour to have been invited by the Royal Society of New South Wales to give its Liversidge Research Lecture for 1986. I note that is the twenty-fifth such lecture, and that today - September 24th, 1986 - is exactly the fifty-fifth anniversary of the first.

What I have to say is, I believe, consistent with the wishes of Archibald Liversidge - to stimulate the acquisition of knowledge by research. At the early age of 20 he was awarded medals in mineralogy, chemistry and metallurgy, all of which are germane to my theme; and whose structural aspects have been my preoccupation for a number of years. Liversidge was admonished for not being sufficiently interested in the crystallographic aspects of mineralogy but, it should be pointed out, he retired from his Sydney University Chair five years before Friedrich and Knipping's crucial experiment on the diffraction of X-rays by crystals (in 1912).

Nevertheless, as is appropriate (for it has long been a strong field of endeavour in this country) crystal chemistry has figured prominently among the Liversidge Lectures: Stuart Anderson (1942), Lloyd Rees (1952), Ray Martin (1976) and, in somewhat different veins, Hans Freeman (1978) and David Craig (1982). But I would especially wish to recall the late David Wadsley (Figure 1), whose 1958 Liversidge Lecture on "Modern structural inorganic chemistry" (A.D. Wadsley, 1958) was a significant part of his transformation of our appreciation of crystal structures, particularly of inorganic compounds and minerals. During his lecture he pointed out that inorganic chemistry had its origins in mineralogy and "still continues to draw upon minerals as the raw material for study." I can only reiterate that: it links my lecture with Wadsley's, and with Liversidge himself since, in 1880, he was appointed Professor of Chemistry and Mineralogy in the University of Sydney.

INTRODUCTION

The two main facets of crystal chemistry (the science of non-molecular crystal structures) are *description* and *explanation*. The first has a long history (stemming from Goldschmidt, Bragg and Pauling, 50-60 years ago), revolutionised by Wadsley. It now seems to have matured; with the emphasis shifting towards other types of structures, e.g. quasi-crystals and glasses. The second, aimed at trying to understand why a compound has the structure it has and not some other that seems equally (or even more) plausible; and at trying to understand why, with say changing temperature or

* Liversidge Research Lecture delivered before the Royal Society of New South Wales, September 24th, 1986, at The University of Sydney.

Figure 1. David Wadsley, 1918 - 1969.

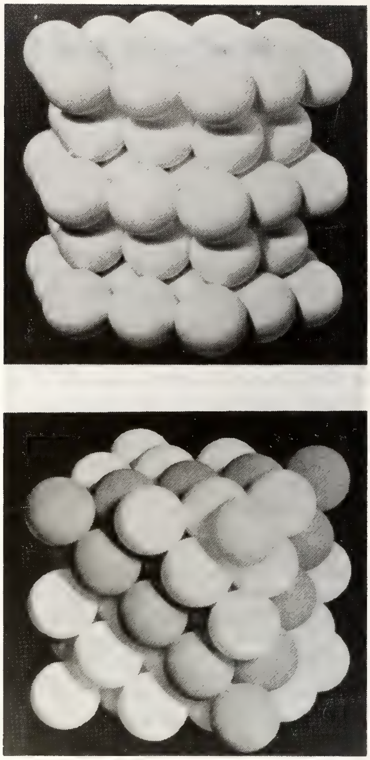


Figure 3. The two simplest forms of close-packing, a) hexagonal, b) cubic.

(especially) pressure, it transforms to a new structure, is not as advanced. It is an active field of research, now developing rapidly in new directions - particularly by the application of quantum mechanics (see, for example, M. O'Keeffe and A. Navrotsky, 1981). It is clearly of concern to fundamental and applied science, and underlies our understanding of mechanisms of solid state (i.e. most) reactions.

Quantum methods are very powerful and successful, but cannot yet handle complex structures. In any case there is a need for a simpler, intuitive approach - with some "physical" feel; and this is what Professor Michael O'Keeffe (of Arizona State University) and I have been attempting to develop in the last few years. It involves a reassessment of the accepted "truth", i.e. the Ionic Model, and a review of at least a part of simple inorganic chemistry, such as the binary and ternary oxides of the Group I elements.

A complete treatment cannot be brief; and so I will consider only a few pieces of the jigsaw which is, in any case, still incomplete. Some reconsideration of the old is followed by some consideration of the new.

THE IONIC MODEL

The sodium chloride structure, shown in Figure 2 (W. Barlow, 1898), is described as a virtually infinite array of Na^+ and $C\ell^-$ ions. It perfectly exemplifies the ionic model, which has been the basis on which non-molecular crystal structures have been described and understood (and misunderstood) for over 60 years. In its simplest terms this employs "*close-packed*" arrays of (negative) anions (cf. Figure 3) - $C\ell^-$ in the case of $NaC\ell$ - into which (positive) cations are inserted in appropriate interstices, commonly octahedral (as in $NaC\ell$) or tetrahedral. The appropriateness is said to be determined by the radius ratio ρ = (cation radius)/(anion radius): ≥ 0.225 for cation coordination number (C.N.) = 4, ≥ 0.414 for C.N. = 6, ≥ 0.732 for C.N. = 8, and so on.

Even though it sometimes works (those cases beloved by text-book authors!), I believe it to be fundamentally wrong. This assertion is justified by making a few simple points:

1. Consider the series of Group II oxides (MgO to BaO), all of which have this $NaC\ell$-type structure. The (oxide) anions cannot be close-packed in every case because there is a large range of distances involved: nearest-neighbour oxygen-oxygen distances, d(O...O) = 2.98 Å in

MgO up to 3.91 Å in BaO. Indeed, BaO, SrO and, possibly, CaO all violate the radius ratio rule.

2. It transpires that the geometry of *close*-packing is also the geometry of *open*-packing (G.O. Brunner, 1971; M. O'Keeffe, 1977). The latter allows anion-anion and cation-cation distances to be a maximum (not minimum) subject to the constraint of a given bond length (i.e. cation-anion distance). Clearly, open-packing is more reasonable for *ion* arrays: it minimises electrostatic energy. Hence, some people now use the term "arranged as in close-packing", for which circumlocution we prefer the neutral term "eutactic" = well arranged (M. O'Keeffe, 1977).

3. Many structures have anion arrays that are not even regular, let alone "close-packed" or eutactic. Their geometries are then very difficult, and often impossible, to describe in the conventional terms of the geometry of their anion arrays, or the articulation of their cation-centred polyhedra of anions. But, as we shall see, in many of these cases the cation array *is* regular, and its geometry familiar.

4. Radius ratio and coordination numbers are often not related by the "radius ratio" rule (cf. above). For example:

   a. ^{VI}Mg in MgO, but ^{IV}Mg in $MgAl_2O_4$ (spinel)*

   b. in $^{IV}Mg^{VI}Al_2{}^{IV}O_4$ the larger cation (Mg^{2+}, $\underline{r}$ = 0.71/0.57 Å) ** is in the smaller, tetrahedral interstice in an almost eutactic array of oxygens, but the smaller cation (Al^{3+}, $\underline{r}$ = 0.675/0.535 Å) is in the larger, octahedral hole.

   c. There are ternary compounds of alkali and transition metal oxides in which various coordination numbers are observed for a given cation, particularly for the alkali metals, e.g. Na whose C.N. ranges from 2 to 12! (R.D. Hoppe, 1980).

5. Calculated (and accurate) electron density maps of MgO (M.S.T. Bukowinski, 1982) show a minimum density along the line Mg-O at 0.91 Å from Mg. Such a minimum is often taken to indicate the dividing surface between cation and anion and, if the density is integrated one finds that a sphere of this radius centred on Mg includes a net charge of +1.9e. But the tangent sphere about the O nucleus includes a charge of only -0.9e. This means that 1.0 electrons (per MgO) are "in the cracks", between such spheres.

6. The size of an Si^{4+} ion is accepted as 0.40/0.26 Å for C.N.(Si)=4. But, if we (reasonably) define its size as the radius of the sphere about the nucleus of a neutral atom that contains 10 electrons then, using the SCF orbital wave functions of Clementi and Roetti (1974), we get a value of 0.61 Å. This is consistent with the experimental value of 0.58 Å for Si^{4+} in α-quartz (R.F. Stewart, M. Spackman, 1981), but completely inconsistent with the accepted ion radius of 0.40/0.26Å (for 4-coordination).

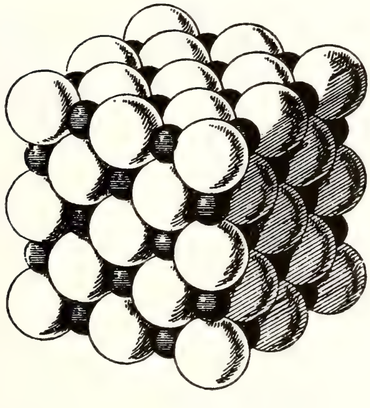


Figure 2. The structure of NaCℓ as a composite of Na and Cℓ atoms or ions (W. Barlow, 1898).

The conclusion is that the ionic model cannot survive close scrutiny, and so we look to an alternative.

THE ALTERNATIVE APPROACH

This treats atoms simply as atoms although, for convenience, we retain the terms cation and anion to distinguish metal from non-metal atoms. It is a simple approach that may be (although it was not) logically developed from an observation made some years ago by Bragg and Claringbull in their important book "The Crystal Structures of Minerals" (L. Bragg, G.F. Claringbull, 1965). Discussing the related structures of the three pyroxene minerals diopside ($Ca_{1/2}Mg_{1/2}SiO_3$), pigeonite ((Ca,Mg,Fe)SiO_3) and clinoenstatite ($MgSiO_3$), they observed that all had SiO_3 chains of corner-connected SiO_4 tetrahedra, and that there were five different conformations of these chains, Figure 4. They pointed out that "The Si-Si distance in a single chain, 3.05 Å, is the same in all three structures"; i.e. in all five chains. But they did not consider what this might imply.

Some years later O'Keeffe and I (M. O'Keeffe and B.G. Hyde, 1978), unaware of this, made a similar observation from an analysis of *all* the (then) well-determined structures of silicates and silicas containing corner-connected SiO_4 tetrahedra - framework, sheet and chain structures. Nearest-neighbour distances $\underline{d}$(Si...Si) showed a remarkably narrow range, Figure 5; especially if one excluded the small "tail" at high $\underline{d}$ values, which could be readily justified (M. O'Keeffe, B.G. Hyde, 1978).

* The coordination number of atom is denoted by a preceding, Roman numeral superscript.
** The two values of the cation radii are first the more recent value of Shannon (1976), stemming from Fumi and Tosi (1964), and second the classical value of Pauling (1960).

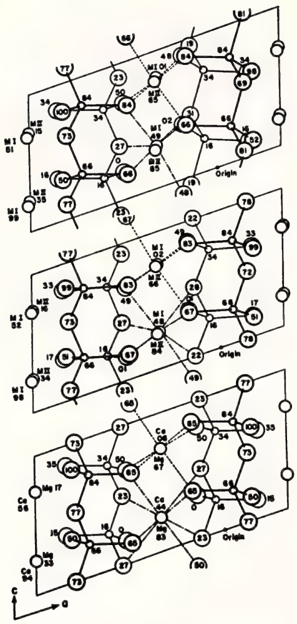


Figure 4. Conformations of $(SiO_3)_\infty^{2-}$ chains of corner-sharing SiO_4 tetrahedra in various monoclinic pyroxenes, $M^{2+}SiO_3^{2-}$ top - clinoenstatite centre - pigeonite; bottom - diopside. Numbers are atom distances above projection plane in units of $\underline{b}/100$ (Bragg and Claringbull, 1965). Small circles are Si, medium circles are M, and large cirles are O atoms.

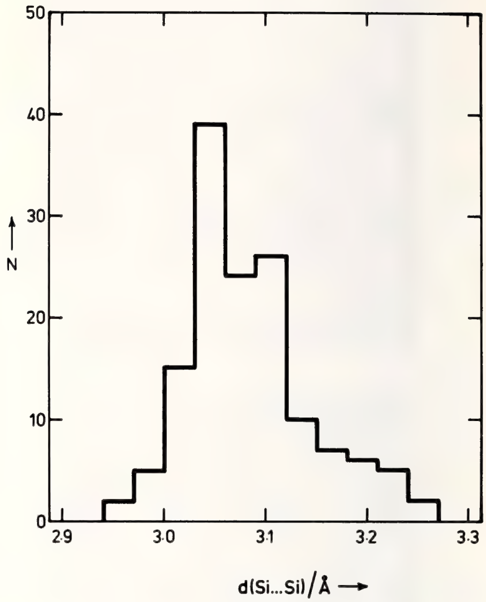


Figure 5. Histogram showing the distribution of of nearest-neighbour silicon...silicon distances, $\underline{d}$(Si...Si), in known structures with corner-connected SiO_4 tetrahedra.

The average separation was <$\underline{d}$(Si...Si)> = 3.06 ± 0.06 Å (cf. Bragg and Claringbull's 3.05 Å); a range of only ± 2%. This may be compared with the distribution of Si-O bond lengths in the same structures: <ℓ(Si-O)> = 1.64 ± 0.06 Å; a range of ± 3.7%. The Si...Si distance is as constant as the Si-O distance!

Furthermore, $\underline{d}$(Si...Si) is about the same in small gas molecules containing the same structure element $\mathrm{Si}\overset{\mathrm{O}}{\wedge}\mathrm{Si}$ (such as $(H_3Si)_2O$), *viz.* 3.10 ± 0.04 Å; and in silica glasses, $\underline{d}$ = 3.00 to 3.10Å. It even seems to be independent of the bridging atom, X in $\mathrm{Si}\overset{\mathrm{X}}{\wedge}\mathrm{Si}$. For X = NH, <$\underline{d}$(Si...Si)> = 3.06 ± 0.05 Å in small gas molecules and, in solids with $\mathrm{Si}\overset{\mathrm{N}}{\wedge}\mathrm{Si}$ such as α-Si_3N_4 and Si_2N_2O, <$\underline{d}$(Si...Si)> = 3.01 Å. And, in the gas molecule $(H_3Si)_2CH_2$, $\underline{d}$(Si...Si) = 3.15 Å. Of course, if the bridging atom X is different then so is the bond length, ℓ(Si-X), and the bond angle, θ(SiXSi). The approximate values of these parameters (ℓ/θ) are, for X = O, N and C respectively, 1.65 Å/147°, 1.70 Å/120° and 1.88 Å/109°.

All this suggested that, in some sense, the bridged Si atoms were touching: a sort of van der Waals contact;* and that *with only one bridging atom* (the "*one-angle*" case) the silicon radius for such *non-bonded*, Si...Si interactions was $\underline{R}$(Si) ≃ 3.06/2 ≃ 1.53 Å, Figure 6.

Similar observations were made for borates (with $\mathrm{B}\overset{\mathrm{O}}{\wedge}\mathrm{B}$ bridges), phosphates (with $\mathrm{P}\overset{\mathrm{O}}{\wedge}\mathrm{P}$ bridges), and so on. Furthermore, such radii were additive; e.g. in borosilicates with $\mathrm{B}\overset{\mathrm{O}}{\wedge}\mathrm{Si}$, <$\underline{d}$(B...Si)> = 2.80 Å, compared with $\underline{R}$(B)+$\underline{R}$(Si) = 1.26 + 1.53 = 2.79 Å (Tables 1 and 2).

Such an approach had previously been put forward for small gas molecules both organic and inorganic, especially by Bartell (1968), but not for non-molecular solids. The extension to the latter is consistent with the more recent approaches by quantum methods in which the likely structures of solids are deduced by calculations on small gas molecules with similar structure elements (G.V. Gibbs, 1982; M. O'Keeffe, M.D. Newton and G.V. Gibbs, 1980; J.K. Burdett and D. Caneva, 1985). A crystal is just a molecule with a large (very large!) molecular weight.

* Dubbed by Ted Summerville "close encounters of the second kind" (E.W. Summerville, 1977)

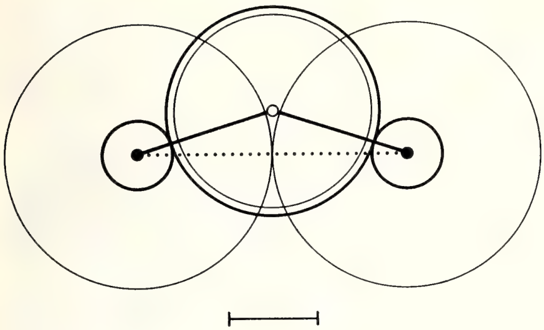


Figure 6. Geometry of the $\mathrm{Si}\overset{\mathrm{O}}{\wedge}\mathrm{Si}$ link between a pair of corner-connected SiO_4 tetrahedra: ℓ(Si-O) = 1.61 Å, d(Si...Si) = 3.06 Å. Heavier circles are the "crystal radii" of Shannon (1976), lighter circles are non-bonded radii, R, for Si and O. (Small filled circles = Si, small open circle = O. Bar = 1 Å.

TABLE 1

'ONE-ANGLE' RADII (IN Å) APPROPRIATE TO BB' CONTACT IN B-X-B' 'CONFIGURATIONS'

Be	B	C	N	O	F
1.39	1.33	1.25	1.14	1.13	1.08
	Al	Si	P	S	Cℓ
	1.63	1.55	1.46	1.45	1.44
	Ga	Ge	As	Se	
	1.63	1.58	1.58	1.58	

TABLE 2

OBSERVED BOND ANGLES IN CRISTOBALITES COMPARED WITH THOSE CALCULATED FOR NON-BONDED CATION-CATION CONTACT.

	Observation	Calculated
Si-O-Si	147°	149°
Ge-O-Ge	128	129
B-O-P	133	138
Be-O-S	136	134
Al-O-P	145	143
B-O-As	128	134
Si-O-Al		143
Al-O-Al		139
Ga-O-P	135	
Be-F-Be	~140*	127

* Estimated from lattice constant.

The new notion is therefore that non-bonded *interactions* between second (and perhaps more distant) nearest neighbour atoms can be an important factor in determining the geometry (and, as will be suggested, the thermodynamic stability) of a structure; and that *cation...cation interactions may well be the more important*, because the R values of cations are usually larger than those of anions, certainly of the first-row anions, N, O and F.

As I indicated earlier, organic chemists have long realised the importance of non-bonded interactions, and developed the science of conformational analysis. Inorganic molecules - even the giant ones that are "non-molecular" crystals - are, in principle, no different.

The hard-sphere model implied by a non-bonded radius should, of course, be replaced by a potential energy curve which shows the interaction energy as a function of (non-bonded) interatomic separation; also a well known concept, Figure 7.

Thus, qualitatively at least (we do not yet have such potential energy curves for cations), we can understand why for example α-quartz and cristobalite have their observed structural geometries. In both cases (topologically different, but flexible) frameworks of corner-connected SiO_4 tetrahedra are collapsed from their most open possible geometries (which they would be expected to have if they were ionic). The extent of the collapse is just that which reduces d(Si...Si) to ~3.06 Å. And, furthermore, in SiO_2 one has SiO_4 tetrahedra rather than SiO_6 octahedra, not because one cannot pack more than four oxygens around a silicon (as the ionic model would have it) - one can, cf. $^{VI}Si^{IV}P_2{}^{II}O_7$ - but because if one does one must then pack more than two silicons around an oxygen: $^{>IV}Si^{>II}O_2$. That means a minimum of three, $^{VI}Si^{III}O_2$. But that is not possible because then d(Si...Si) will be significantly less than 3.06Å, and so extremely repulsive (cf. Figure 7). Compare a maximum θ(SiOSi) = 120° for C.N.(O) = 3 with the observed θ ≈ 147° for C.N.(O) = 2 in quartz etc. (High pressure can overcome such repulsions, and SiO_2 then becomes rutile type, $^{VI}Si^{III}O_2$; but with strong Si...Si repulsion).

SUMMARY

The above has been sketchy and incomplete, but it leads to the following simple (somewhat over-simplified) picture of a crystal structure: nearest neighbour atoms are held together by very strong attractive forces (bonds, which are strong enough to overcome nearest neighbour interatomic repulsions); and these bonds are somewhat stretched by the repulsions between the second-nearest neighbour atoms surrounding a central atom (cations around a central anion, and *vice versa*) Thus, in a structure there is a balance between tensile forces (in the bonds) and compressive forces (in the non-bonded interactions) - a normal state of affairs in all structures, including the Sydney Harbour Bridge. The repulsions arise from the Pauli exclusion principle (due to electron orbital overlap). For non-bonded interactions these will correspond to much lower electron densities than occur across bonds - hence non-bonded distances are larger than bonding distances. They do

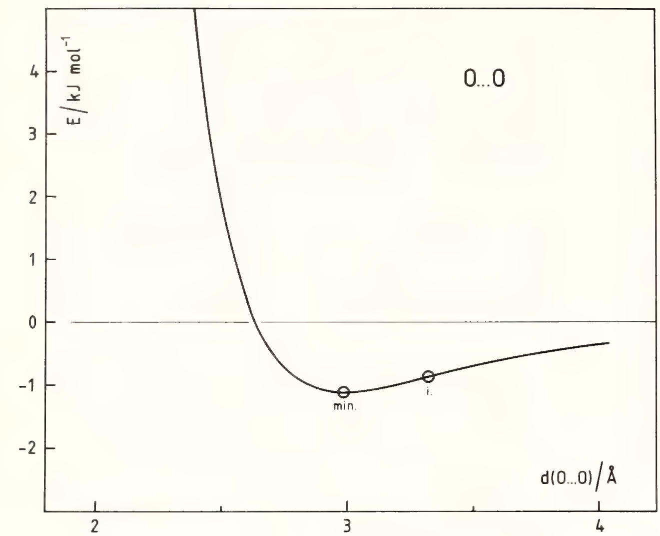


Figure 7. Typical potential energy curve characteristic of non-bonded interactions

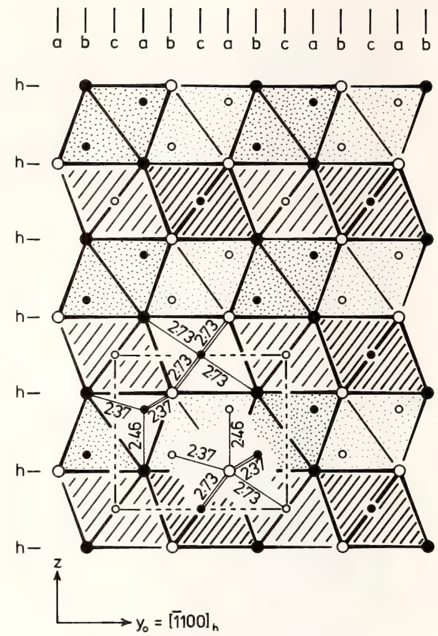


Figure 8. The structure of A-type La_2O_3 projected on $(11\bar{2}0)$ of its hexagonal unit cell, and emphasising O-centre La_6 octahedra and La_4 tetrahedra. The cations (larger circles at heights 0 (open) and 1/2 (filled)) are hcp. a,b,c indicate stacking positions of eutactic layers in the usual way; h indicates hexagonal eutaxy.

not correspond to electrostatic repulsions between ions (which have a different form): this we can see by the inability of the electrostatic ionic model to account for the observed geometries of flexible structures, such as quartz and cristobalite (R. Fischer and J. Zemann, 1975), calcium-chloride-type structures (as compared with the topologically identical rutile types (H. Bärnighausen et al, 1984), etc.

The relative importance of the two sets of second-nearest neighbour interactions (cation... cation and anion...anion) depend not only on the relative (non-bonded) sizes of the atom types but also on their relative numbers in the crystal, i.e. on the stoichiometry of the compound. Clearly, a preponderance of cations (e.g. Li_3N) will tend to make cation...cation repulsions the more important *because there are more of them* and therefore their average separation is less (than that of the anions). The converse is likely to be true for a crystal of e.g. $A\ell F_3$. But in less extreme cases the interplay of stoichiometry and size is a subtle problem.

We are now ready to consider some consequences of this new way of thinking about structures.

THE BULK MODULUS OF DIAMOND

Diamond is one of the "hardest", most incompressible substances known. It is also one of the best thermal conductors (better than Ag and Cu) but, under normal conditions, metastable with respect to graphite. The first two facts are often wrongly attributed to rigidly oriented sp^3 hybrid orbitals, used for C-C bonding. But all follow from the fact that the second-nearest neighbour distance, $d(C...C) \simeq 2.52$ Å, is very close to $2R(C) = 2.50$ Å; and therefore non-bonded contacts are strongly repulsive (cf. Figure 7).

From the equation for the potential energy of C...C contacts (A.I. Kitaigorodsky, 1973) one calculates $E \simeq 17$ kJ per mole of contacts, $\simeq 100$ kJ per mole of carbon. (Each C atom has 12 C...C contacts, and so there are 6 moles of C...C contacts per mole of C.) This is very high: greater than many enthalpies of transformation between structures (or even chemical reactions). Any deformation or compression of the crystal must cause some shorter distances d(C...C), and therefore a steep increase in the repulsive energy, E. Hence the high bulk modulus (4.4 Mbar), and stiffness (which causes the high thermal conductivity), and instability with respect to graphite (in which the lower C.N., ^{III}C instead of ^{IV}C, reduces the repulsion energy-mainly by reducing the number of second-nearest neighbours by a factor of four) are all readily accounted for. A more sophisticated analysis (M. O'Keeffe and B.G. Hyde, 1981) reveals that ~80% of the bulk modulus arises from these non-bonded repulsions (and also that our picture of stretched bonds and compressed non-bonded contacts is correct).

High pressure is necessary to overcome these repulsions and synthesise diamond. The same is true for the synthesis of the isostructural BN (with $d(B...B) = 2.56$ Å, cf. $2R(B) = 2.52$ Å). SiC is

similar (d(Si...Si) = 3.07 Å, cf. $2R$(Si) = 3.06 Å). And these are three of the most incompressible substances known. (In contrast all other isostructural compounds, in which $d >> 2R$, are relatively soft and compressible, e.g. ZnS.)

HIGH PRESSURE PHASE TRANSFORMATIONS

Most high-pressure transformations involve an increase in coordination number, e.g. tetrahedral ZnS types transform to octahedral NaCℓ types: $^{IV}Zn^{IV}S \xrightarrow{P} {}^{VI}Na^{VI}Cl$. (Italic letters indicate a structure type (as distinct from a compound)). This is to be interpreted in the following way. In such cases the strongest repulsions are between *first*-nearest neighbours, i.e. bonded atoms (cf. the relatively short distances involved, i.e. bond lengths). And it is these repulsions which are relieved by an increase in coordination number for, paradoxical though it may seem at first sight, it results in an *increase* in bond length: more bonds are formed; they must therefore each be weaker, and therefore longer - and this in spite of the decrease in volume (which Le Chatelier's Principle tells us must occur in a high-pressure transformation). For *ZnS* → *NaCl* the change in linear dimensions (unit cell edge, and cation...cation = anion...anion distance) is ~-2.2%, but the change in bond lengths is ~+5.6%.

But there are some cases, e.g. *olivine* $\xrightarrow{P}$ *spinel* (of great geophysical interest) and *zircon* $\xrightarrow{P}$ *scheelite*, where there is a change of structure without a change of coordination number: both the olivine and the spinel forms of magnesium silicate are $^{VI}Mg_2{}^{IV}Si^{IV}O_4$. In such cases the transformation clearly does *not* relieve first-neighbour repulsions. So (we might suggest) perhaps it relieves second-nearest neighbour repulsions? In the olivine/spinel case there is a volume change of ~ -10%; but the anions in the former are approximately in hexagonal eutaxy and in the latter in cubic eutaxy and so anion/anion C.Ns. are unchanged: on the average, anion...anion distances are reduced in the transformation (thereby probably increasing their repulsive energies). At the same time, d(Mg...Mg) is also reduced (by ~ 8%), but d(Mg...Si) is increased by ~ 3%. (See Table 3) This suggests that the transition relieves strong Mg...Si repulsions (at the cost of stronger Mg...Mg repulsions) which, amongst other facts, is consistent with an increase in the C.N. of Si by Mg from 9 in olivine to 12 in spinel.

Other explanations of the effect of pressure on the relative stabilities of olivine and spinel polymorphs have previously been offered. That of Kamp (1968) is widely accepted. Although it involves Pauling's rule about the relative instability of shared (anion...anion) polyhedral edges it is also perfectly consistent with the explanation given above, which implies that olivine-type Mg_2SiO_4 is destabilised by Mg...Si repulsions (at high pressure) and its spinel-type by Mg...Mg repulsions (at low pressure).

CATION ARRAYS IN CRYSTAL STRUCTURES

That cation...cation repulsion is often more important than anion...anion repulsion is consistent with the regularity often observed in the cation arrays of many structures, especially those in which no regularity (simple geometrical pattern) can be detected in their anion arrays. This leads to simple descriptions of many (chemically simple) compounds, such as sulphates, carbonates and silicates of large (especially Group 1 or Group II) cations, for which no simple geometrical description was previously available (R.W.G. Wyckoff, 1968). We give only a few examples of the many that are possible (M. O'Keeffe and B.G. Hyde, 1985).

1. La_2O_3

The A-type rare earth sesquioxide structure, of which La_2O_3 is the prototype, is far from straightforward in terms of its anion array. This consists of eutactic layers of anions normal to its hexagonal c axis, but the interlayer spacings are far from eutactic. The cations are 7-coordinate, in the form of a monocapped octahedron. On the other hand the cations also form eutactic layers normal to c, and their layer spacing is very close to that in ideal hexagonal eutaxy. The structure is therefore most simply described as hexagonally eutactic La, with alternate interlayers occupied by O in all the octahedral sites or O in all the tetrahedral sites, Figure 8. This is rather satisfying, as the former layer is *NaCl* type (to be expected for LnX compounds of the lanthanides, e.g. LaN) and the latter is *CaF₂* type (to be expected for LnX_2 compounds of the lanthanides, e.g. LaOF). It is therefore not surprising that La_2O_3 (of intermediate stoichiometry) is a simple intergrowth of alternating layers of the two extreme stoichiometries.

2. $BaSO_4$

This is less obvious than the previous example, and a puzzle (R.W.G. Wyckoff, 1968) until now. The anion array is quite irregular; only SO_4 tetrahedra can be recognised. But the BaS array is a simple alloy structure, the *FeB* type, cf. Figure 9.

3. β-K_2SO_4

This is a structure type of considerable interest in a number of ways (some of which we shall see later), and is the structure of many compounds. But, as with $BaSO_4$, the only apparent regularity in the structure is in the cation array. The K_2S part is the well-known $Ni_2Si/PbCl_2$ structure type, Figure 10.

4. Ca_2SiO_4

This substance is polymorphic being able, at different temperatures, to occur in five different structures: α, α'_H, α'_L, β and γ. It is an important component of cement, to the production of which the γ/β polymorphism (close to room temperature) is important. The β-form, shown in Figure 11, is rather close to the β-K_2SO_4 type (above), the difference being mainly a very slight deformation of the unit cell and tilt of the BX_4 tetrahedra (SiO_4 and SO_4 respectively). The α'_L structure is even

TABLE 3

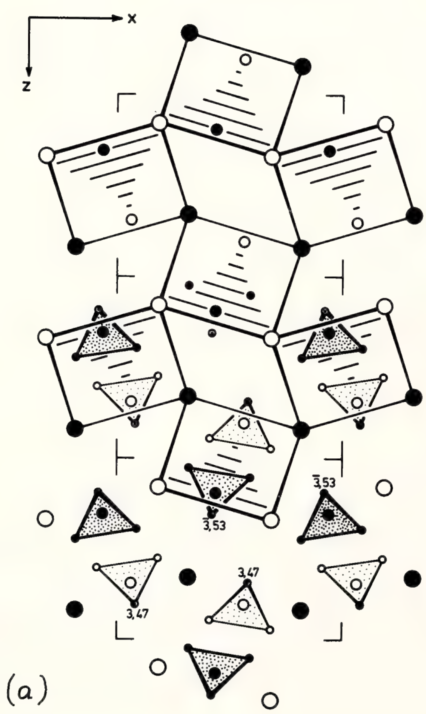
MEAN SHORTEST NON-BONDED DISTANCES (IN Å) IN THE α- AND γ-POLYMORPHS (OLIVINE AND SPINEL FORMS) OF Mg_2SiO_4

	α-form	γ-form	Difference, Δ(γ−α)
Mg...Si	$3.26_5(2.69_2-3.28_3)$	3.36_6	$+0.10_1$
Mg...Mg	$3.11_5(2.99_3-3.86_2)$	2.87_0	-0.24_5
O...O	$2.91_2(2.55_8-3.39_3)$	$2.85_2(2.70_3-3.00_0)$	-0.06_0

TABLE 4

EXPECTED AND OBSERVED ℓ(M-O) IN ANTIFLUORITE-TYPE ALKALI METAL OXIDES, M_2O

M =	Li	Na	K	Rb
Expected ℓ/Å	1.9_3	2.2_4	2.4_3	2.5_4
Observed ℓ/Å	2.00	2.40	2.79	2.93
Difference/Å	0.0_7	0.1_6	0.3_6	0.3_9
Difference/%	3.5	7.2	14.8	15.0



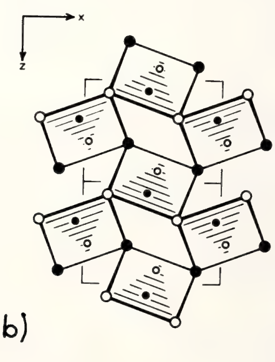


Figure 9. (a) The structure of $BaSO_4$ as SO_4 tetrahedra and Ba atoms (below) and SBa_6 trigonal prisms (above).

(b) The structure of FeB: compare with BaS in (a)

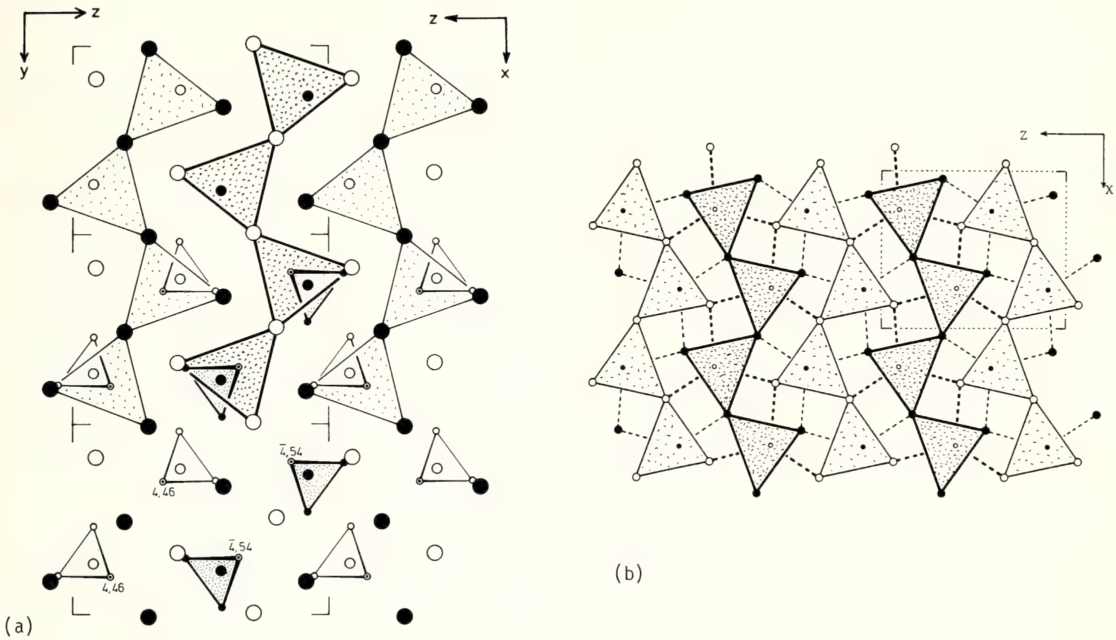


Figure 10. (a) The structure of β-K_2SO_4 shown as SO_4 tetrahedra and K atoms (below) and SK_6 trigonal prisms above. (The unit cell settings are Pnma for the axes at top right, and Pmnb for those at top left.)

(b) The $PbCl_2$ structure: compare with K_2S in (a)

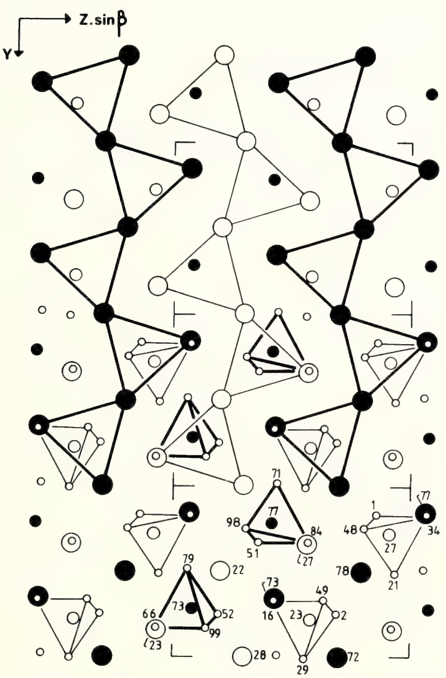


Figure 11. The structure of β-Ca_2SiO_4: compare Figure 10.

closer to β-K_2SO_4; and the α'_H structure *is* β-K_2SO_4. In all three (β, α'_L and α'_H), the only regularity is in the cation array which is, at worst, a very slightly deformed *Ni_2Si/$PbCl_2$* type.

This case is particularly interesting as the structure of the compound Ca_2Si is also *Ni_2Si* type; i.e. the Ca_2Si arrays in Ca_2Si and in β'-, α'_L- and α'_H-Ca_2SiO_4 are virtually identical. Furthermore, their molar volumes are also almost the same, 49.9 and 51.8 $cm^3\ mol^{-1}$ for Ca_2Si and β-Ca_2SiO_4 respectively. These two facts seem at first sight surprising. The first at least appears to be another consequence of cation... cation repulsion, as does the complex modulated structure of α'_L-Ca_2SiO_4 (by analogy with the experimental results for α'_L-Sr_2SiO_4 (Hyde, Sellar and Stenberg, 1986)). But this is too specialised a problem to be dealt with here. Instead, we turn to some elementary chemistry.

THE CHEMISTRY OF SOME SIMPLE BINARY FLUORIDES, OXIDES ETC. (M. O'KEEFFE AND B.G. HYDE, 1984)

Consider the "normal" oxides of the alkali metals, Li to Cs; i.e. those in which the metal and the oxygen have their normal valences (of 1 and 2 respectively) and in which there are only metal-oxygen bonds (which means we exclude peroxides etc. with O-O bonds and suboxides with metal-metal bonds). Their stoichiometry is M_2O: twice as many cations as

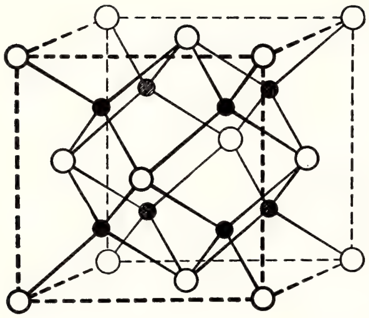


Figure 12. The fluorite-type structure: smaller circles are anions, larger ones are cations.

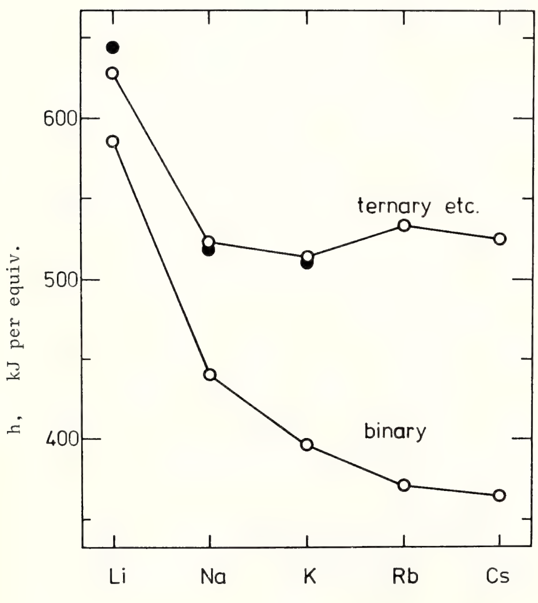


Figure 13. Equivalent bond enthalpies (h) of alkali metal-oxygen bonds in binary (M_2O) and ternary oxides. (The filled circles are for peroxides, e.g. Na_2O_2: note that their h values correspond to those for the ternary compounds, i.e. the Na-O bonds are stronger in Na_2O_2 than in Na_2O.)

anions; and "large" cations too, so that cation... cation repulsions are likely to be important - as turns out to be the case.

All except Cs_2O have the antifluorite structure (Figure 12), in which each oxygen is 8-coordinate and each metal is 4-coordinate, ${}^{IV}M_2{}^{VIII}O$.* A striking fact is that these oxides are quite difficult to prepare: instead of the normal oxides one readily gets peroxides M_2O_2 (with $O-O^{2-}$) and/or superoxides MO_2 (with $O-O^-$). And, if prepared, they are extremely reactive - with CO_2 and H_2O in the atmosphere, etc. Put formally, their reactions are extremely exothermic, due to their enthalpies of formation being low. Less formally, they are relatively unstable, and this shows clearly from their low bond enthalpies (M. O'Keeffe and B.G. Hyde, 1984), (Figure 13), and correspondingly long bond lengths (N.K. McGuire and M. O'Keeffe, 1984). For the same formal bond valence** the bond enthalpies are larger (and the bonds correspondingly shorter) in ternary compounds with lower stoichiometric ratios cations/anions, and also in peroxides etc.

This decreased stability we attribute to excessive cation...cation repulsion in the normal binary compound. One can imagine the M_2O compound being formed by shrinking an infinitely spaced, anti-fluorite like array of cations and anions. As the eight cations approach the anion to which they are coordinated (Figure 14) the bond lengths decrease (and bond enthalpies increase) as do the non-bonded distances and repulsions. Before the cations attain a distance from the anion appropriate to a bond valence of 1/4, the attractive (bonding) forces are balanced by the repulsive (M...M) non-bonded interactions, and the bonds remain stretched and weak. The deficit in bond enthalpy is ~ 60 kJ mol^{-1} in Li_2O increasing to ~ 330 kJ mol^{-1} in Rb_2O (Figure 13). Hence the high ΔH^o values for reactions of these oxides which involve a decrease in anion C.N.

Using accepted bond valence/bond length relations (those of I.D. Brown *et al*, 1985, but see also N.K. McGuire and M. O'Keeffe, 1984) one can deduce expected bond lengths in M_2O, and compare them with the observed values (Table 4). As with the bond enthalpies, the discrepancy increases with increasing atomic number of the cation.

A similar, though less drastic, effect is observed in the oxides of the Group II (alkaline earth) metals. This mitigation is a result of the lower stoichiometric ratio cations/anions in the latter; due to the cation valence being higher than for Group 1. Similarly, the stoichiometric ratio is reduced for Group I cations if the anion valence is reduced, i.e. for fluorides instead of oxides; and the bond enthalpy deficit is correspondingly smaller (M. O'Keeffe and B.G. Hyde, 1984).

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* Note in passing the high C.N. for the small O and low C.N. for the large M; and compare the radius ratio rules.

** Bond valence (or bond strength) is, for equivalent bonds (as in fluorite types), simply the formal valence of an atom divided by its C.N. In the case of e.g. ${}^{IV}K_2{}^{VIII}O$ it is 1/4 at K = 2/8 at O. This shows how coordination numbers and stoichiometry are unavoidably related.

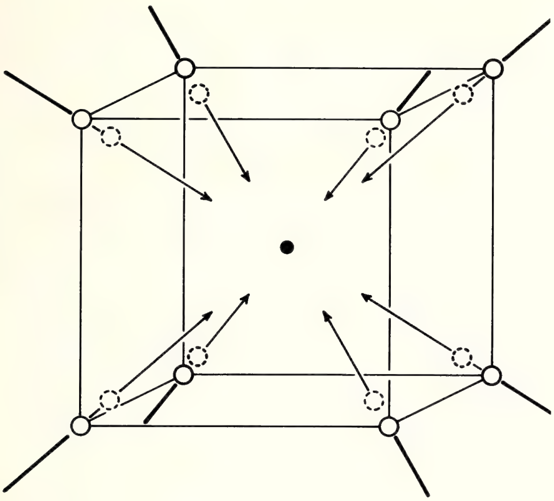

Figure 14. Notional formation of OM_8 group in antifluorite-type (normal) alkali metal oxides, M_2O, by contracting an infinitely separated atom array. Filled circle = O; open circles = M, continuous at actual positions, broken at ideal positions (bond valence = 1/4).

Conversely, increasing the valence of the anion should increase the severity of the problem. (There will now be even more cations per anion.) So consider the nitrides of Group I, M_3N: for the heavier M atoms the bonds are so weakened that the compounds do not form: only Li_3N is known. (Instead one gets azides, $M(N_3)$, with the polyanion N_3^-: cf. the reference to peroxides etc. above.) And for carbides, M_4C, the situation is of course worse: none is known. For Group II metals only one "normal" carbide is known, Be_2C. And even for Group III one has only boron carbides and Al_4C_3. (Instead one tends to get polyanions, e.g. "acetylide" C_2^{2-}, or metal-metal bonding, i.e. polycations, e.g. Ca_2N, $Ca_{11}N_8$ etc.)

Thus, a lot of simple chemistry falls into place. And there is much more (M. O'Keeffe and B.G. Hyde, 1984), all of which appears to be a natural consequence of "*cation crowding*", which gets worse as the stoichiometric ratio (cations/anions) gets higher.

I submit that what has been said is more than enough to indicate that cation size is a significant factor in solid state physics and chemistry and mineralogy; and that this notion is worth pursuing in the future.

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Research School of Chemistry,
The Australian National University,
G.P.O. Box 4,
Canberra, ACT, 2601, Australia.

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Doctoral Thesis Abstract (University of Sydney): Pancreatic Fluid and Electrolyte Secretion

K. T. F. P. SEOW

The isolated, vascularly perfused rat pancreas requires adequate perfusion in order to achieve a secretory response that is compatible with the *in vivo* secretory rate. Perfusion rates of 7.4 - 5.2 ml/min is adequate for this purpose. However, with perfusion rates lower than 5.2, (3.4 ml/min was tested), secretory responses of the glands were lower than both the *in vivo* pancreas and glands that were perfused at higher rates.

The rat pancreatic ducts, simulated by secretin (4.0 nmol/l), require the simultaneous presence of extracellular HCO_3^- and Cl^- for maximum secretion. Acetate alone or when used as a replacement for either HCO_3^- or Cl^-, was only capable of supporting about 53% of control fluid output during secretin stimulation. Cl^- alone supported only minimal secretion (7%). The pancreatic ductal secretion in the rat is not sensitive to furosemide (1 mmol/l). Therefore, the inhibition of fluid secretion during Cl^- withdrawal cannot be due to a non-operative Na-Cl symport. This symport, so well defined in tissue such as shark rectal gland, kidney tubule and mandibular gland is sensitive to furosemide. This results can best be explained by postulating a system of paired antiports, Na^+/H^+ and Cl^-/HCO_3^-, located in the basolateral membrane, an anion channel in the luminal membrane (probably HCO_3^--selective and of limited conductance) and tight junctions of poor cation-selectivity. Studies with specific inhibitors like amiloride, (Na^+/H^+ blocker) and SITS (Cl^-/HCO_3^- blocker) show that rat pancreatic secretion is sensitive to these inhibitors (Evans & Young, *Proc. Aust. Physiol. Pharmacol. Soc.*,*1985*).

The rat acinar was stimulated with an analogue of cholecystokinin, caerulein (17.7 pmol/l). In contrast to the ducts, it shows no requirement for extracellular HCO_3^-. However, Cl^- is essential for fluid secretion at this site its requirement is specific as other halides (Br^- and I^-) were unable to support secretion to any significant extent when used to replace Cl^-. Other anions used such as isethionate, gluconate and methylsulphate were similar to the halides substitutions. The fluid secretion in the acini is sensitive to furosemide and its analogues, piretanide and bumetanide, suggesting that a Na/Cl^- symport is responsible for salt translocation and thus fluid secretion. Whatever the furosemide sensitive transport process may involve, this transport protein(s) is more sensitive to furosemide in the absence of extracellular HCO_3^- than in its presence. The half maximal inhibitory concentrations for the "loop" diuretics studied

were increased by some 10 fold in the presence of HCO_3^-. In the absence of extracellular HCO_3^- amiloride, methazolamide and SITS were equally effective in inhibiting fluid secretion. However, in the presence of HCO_3^- amiloride was more potent than SITS or methazolamide. This change in inhibitor sensitivity is probably due to the dependence of secretion on the production of HCO_3^- from metabolic CO_2 when the extracellular HCO_3^- is absent. It is proposed that acini also secrete fluid like the ducts by a system of paired antiports (Na^+/H^+ and Cl^-/HCO_3^-) but with a Cl^--selective luminal channel. The inhibitory action of "loop" diuretics on secretion can be attributed to the inhibition of Cl^-/HCO_3^- antiport and carbonic anhydrase for which furosemide has been shown to act.

In addition to the change in sensitivity of acinar secretion to "loop" diuretics by extracellular HCO_3^-, the inhibitory effect of these drugs are different, at least for piretanide. Instead of a simple one step inhibition, a biphasic inhibition was observed. Furosemide appeared not to have such an action while that of piretanide was pronounced and bumetanide was intermediate.

When isethionate was used as extracellular Cl^- replacement, some pancreases showed large secretory responses 30 minutes after the start of caerulein stimulation. Isethionate entered the cytoplasm of pancreatic tissue, at least some compartment not accessible to inulin. The occupation of this space was unaffected by the Cl^-/HCO_3^- antiport blocker, SITS (0.1 mmol/l). Its distribution in the pancreatic tissue appeared to be passive diffusion of the undissociated acid similar to weak acids such as acetic acid. With the aid of an alternate experimental protocol which reduced the exposure time of the pancreas to isethionate solution, it was shown that isethionate was not capable of supporting acinar secretion. The large secretory responses seen in some pancreases were experimentally induced artifact due to the loss of epithelial integrity. This allowed ultrafiltration to contribute significantly to secretion, if not entirely.

c/- Department of Medicine,
Clinical Science Building,
Repatriation General Hospital,
Concord, NSW, 2139, Australia.

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Doctoral Thesis Abstract (University of Sydney): Fitness Components of Chromosomal Homozygotes In *Drosophila melanogaster* In Age Structured Populations

LEIGH PASCOE

This thesis is concerned with the measurement of fitness components of second chromosome homozygotes of D. melanogaster in population cages. The fitness components are of intrinsic interest as an explanation of the observed fitness depression in chromosome homozygotes. Additionally however, the population cages are a suitable system in which to investigate the problems of estimating fitness components in populations with continuous generations.

The principal experimental result of this thesis is that relative viability of larvae is a major contributor to the lowered fitness of chromosome homozygotes. The result was unexpected as test crosses of non lethal bearing chromosomes give relative viabilities close to unity. Relative viability appears to be density dependent and under cage conditions is considerably lower than values observed in low density test crosses. For a sample of lines studied the relative viability under cage conditions was $.432 \pm .113$ compared with relative viability under sampling densities of $.901 \pm .040$.

Analysis of cage sample frequencies shows that the cage test is a repeatable fitness measure, however it may give biased estimates if the relative viability estimates differ from that actually experienced in samples or if an inappropriate model is being used. In two lines studied parental crowding leads to a reduced relative viability among progeny and if this observation is a general one past estimates of fitness may be downwardly biased. The parental crowding effect could also be the reason for consistently negative fitness estimates in some lines, although the use of inappropriate models can lead to the same result.

A further observation in all lines studied was that the complex inversions in the balancer chromosome, or some other factor, leads to a reduced absolute viability of progeny from SM1/+ females. This observation could be of some relevance to the low fitness of SM1/+ genotypes relative to random heterozygotes.

Adult fitness components were also found to contribute to the lowered fitness of the homozygotes. Mean male mating ability in 3 lines studied was

$.413 \pm .054$ while mean female fecundity of 5 lines was $.482 \pm .044$.

In tests of male mating ability the conditions of the test and the method of scoring can markedly affect the results. A simple mating model is proposed to explain these observations and the scoring of first matings in a number of test is the recommended method. Three methods of assessing female fecundity were examined for efficiency by the maximum likelihood method and subsequent experiments indicated that relative fecundity is not unduly sensitive to frequency, but is affected by density. Experiments were also carried out on age specific rates of mortality and fecundity for three of the lines studied. The fecundity of homozygotes was consistently below that of heterzygotes, but relative values could be expected to increase with age. Adult mortality did not differ between the sexes for two of the lines but in a third line females suffered a much higher death rate. The latter line also experienced a heavier mortality overall than the other two.

For the lines in which sufficient information was obtained to predict overall fitness from the components a value of .193 was predicted. This compares favorably with the observed mean fitness of .194 and implies an inbreeding depression of 81%. Of the observed inbreeding depression, 51% may be attributed to reduced relative viability in both sexes and 49% is due to adult components. The latter figure may be further subdivided into 23% in female and 26% in male adult fitness components.

In the thesis I have also looked at age specific models of selection in populations subject to density dependent control of numbers. The control of population size is assumed to affect all genotypes equally so that selection could be described as density independent. Nevertheless the relative fitnesses in the population are dependent on the specific model of population regulation assumed. This result arises because of changes in the age structure of the equilibrium population (Charlesworth and Giesel 1972a,b). In a regulated

population the weighting given to reproduction at a particular age may be greater than, equal to or less than that in an expanding population and consequently early reproduction will vary in importance in the different models. Specifically, a gene which causes a shift forward in the whole reproductive schedule, but which has no other effect on the vital rates, will not always be favored by natural selection.

The model is also extended to include density dependent control in two critical age groups in the population. This model, in which mortality in each group depends on a limiting resource unique to each group, is used to explain the experimental observations of Ayala (1970) that population size is independently affected by variation in food and living space.

It is argued in the thesis that the measurement of fitness in populations with continuous generations requires studies on the manner in which population numbers are regulated and measurement of age specific vital rates.

Memorial Sloan-Kettering Cancer Centre,
1275 York Ave.,
New York, New York 10021, U.S.A.

(Manuscript received 26.2.1987)

Journal and Proceedings, Royal Society of New South Wales, Vol. 119, p. 169, 1986
ISSN 0035-9173/86/020169 — 01 $4.00/1

M.Sc. Thesis Abstract (University of Sydney): Photographic Measurements of Bubble Populations From Oceanic Whitecaps

ANDREW WALSH

A photographic technique was used to measure bubble size spectra at depths of 0.5 - 2.0 m in windspeeds of 2-14 ms^{-1}. Sampled bubble populations were found to be highly variable with time. The bubble density was found to be a strong function of windspeed, W, varying as $W^{3.3}$ and few bubbles were found below 6 ms^{-1}. For windspeeds between 6 ms^{-1} and 14 ms^{-1} the total air volume entrained by bubbles increased as $W^{4.9}$. The state of development of the wave height spectrum also appeared to influence the bubble density. Bubble size spectra varied with radius, r, on average, as r^{-4}. The more limited oceanic results of previous workers are compatible with these findings.

32c Torrens Street,
Braddon, ACT, 2601, Australia.

(Manuscript received 5.1.1987)

Journal and Proceedings, Royal Society of New South Wales, Vol. 119, pp. 171-178, 1986
ISSN 0035-9173/86/020171 — 08 $4.00/1

Annual Report of the Council for 1985: Part 2

MEETINGS

Nine general monthly meetings and the annual meeting were held during the year. The average attendance was 22 members and visitors (range 13 to 44). Abstracts of the addresses were published in the Newsletter. The meetings were all held at the Macquarie University.

The Clarke Memorial Lecture for 1985 was delivered by Professor R.L. Stanton on "Stratiform Ores and Geological Processes" on Monday, 30th September, 1985, at the Stephen Roberts Lecture Theatre, University of Sydney.

Two seminars in the series entitled "Scientific Sydney" were held on Saturday, 18th May and 12th October, 1985, in conjunction with the Royal Australian Historical Society, at History House, Macquarie Street. The titles were "Culture and Learning in the Colonial Metropolis", and "Professor John Smith (1821-1885) and Victorian Sydney: A Centennial Retrospect", respectively.

Eleven Council meetings were held during the year at the Society's office, 134 Herring Road, North Ryde.

ANNUAL DINNER

The Annual Dinner was held in the Sydney Cove Room at the Hilton Hotel on the evening of Tuesday, 18th March, 1986. Forty members and guests were present. The guest speaker was Professor Dame Leonie Kramer, who spoke on "Broadcasting to the nation?" A vote of thanks was moved by Dr. R.S. Bhathal, Vice-President.

PUBLICATIONS

The Journal and Proceedings, Volume 118, Parts 1 to 4, were published, incorporating 19 papers and the 1984-85 Annual Report of Council. Several lectures given at the Faculty of Science Centenary at the University of Sydney, and papers from the "Scientific Sydney" seminars were published. Council is again grateful to the voluntary referees who assessed papers offered for publication. The assistance of Miss H. Basden in processing the material for printing is gratefully acknowledged.

Ten issues of the Newsletter were published during the year, and Council thanks the authors of short articles.

MEMBERSHIP

The membership of the Society at 31st March, 1986 was:

Honorary Members	13
Company Member	1
Life Members	31
Ordinary Members	273
Absentee Members	12
Associate Members	28
Total	358

During the year the deaths were announced with regret of the following members: Sir Frank Macfarlane Burnet (31.8.85), Daphne Lydia Hayes (27.6.85), William Broderick Smith-White (8.2.86), Trevor Taylor (19.10.85), and Norbert Thomas Wright (29.10.85).

AWARDS

The following awards for 1985 were made:

James Cook Medal: Dr. Donald Metcalf
Clarke Medal: Professor Hugh Bryan Spencer Womersley
Society Medal: Dr. Dalway John Swaine
Edgeworth David Medal: Dr. Simon Charles Gandevia and Dr. Brian James Morris

SUMMER SCHOOL

A most successful Summer School on "Computing and Science" was held from 13th to 17th January, 1986, at the University of Sydney. It was attended by 50 school students about to enter Year 12. The Summer School was organised on the Society's behalf by Mr. H.S. Hancock. The Society's appreciation is extended to Mr. Hancock, to Mr. E.D. O'Keeffe who assisted him in the organisation, to Council members who chaired sessions, and to Mrs. Winch who organised the morning and afternoon teas. Council also wishes to thank the speakers and organisers of site visits, whose talks and demonstrations helped to make the School a success. Visits were made to the Australian Atomic Energy Research Establsihment at Lucas Heights, and to QANTAS at Mascot.

LIBRARY

The new arrangements described in 1984 and 1985 Annual Reports, whereby most of the Library Collection was transferred to the Dixson Library, University of New England, are very satisfactory. The acquisitions by means of gift and exchange are being maintained by the Society in cooperation with the Dixson Library, thereby ensuring the continued supply of those journal titles which are currently active. The older part of the Collection is now housed in a special room of the Dixson Library that bears the name of the Royal Society. The active journals are available on the open shelves for ease of access, and bound volumes are distinguished by a bookplate on the inside cover and the Society's crest on the spin. Apart from its value to

researchers at the University of New England, the Royal Society Collection attracts requests from other organisations. Since its holdings are recorded in the national union list, *Scientific Serials in Australian Libraries*, the Dixson Library receives regular inter-library loan requests for access to the rich resources of the Collection.

The formal transfer of custody of the Royal Society Collection to the Dixson Library took place on 23rd March, 1986, to coincide with the 25th anniversary of the New England Branch. The proceedings were chaired by Mr. Karl Schmude, University Librarian, who welcomed guests. The President, Associate Professor J.H. Loxton, then gave an account of the steps leading to the transfer of the Collection to the Dixson Library, and he paid special tribute to Mrs. Grace Proctor for her voluntary work. He closed by transferring the Collection to the custody of the University. The Chancellor, Dr. R.C. Robertson-Cuninghame, accepted the transfer with gratitude, on behalf of the University of New England. Mr. Schmude concluded the ceremony by inviting those present to view displays of recent acquisitions and newly bound volumes as well as early volumes.

OFFICE AND LIBRARY AT NORTH RYDE

The office is now operating at Herring Road, North Ryde, and has been open on Thursdays during 1985. The library which the Society retained, has been set up at North Ryde, and includes a wide range of serials and monographs on the history of Australian science. It is hoped to re-catalogue the collection during 1986.

NEW ENGLAND BRANCH

The New England Branch held four very well attended meetings during 1985. The meetings were:

July 5th, 1985: Dr. D.J. Swaine: "The deposition of trace elements in the environs of a power station".
August 29th, 1985: Professor Kurt Lambeck, Director of the Research School of Earth Sciences, A.N.U.: "Motions of Continents and Satellites: an examination of the plate tectonics hypothesis through observations".
September 12th, 1985: Dr. P.G. Flood, Department of Geology, University of New England: The Great Barrier Reef: its nature history of formation and future outlook".
October 3rd, 1985: Professor L.W. Nichol, Vice-Chancellor, University of New England: Protein interaction patterns: the biological consequences of macromolecular stickiness"

On 23rd March, 1986, the Branch celebrated its 25th Anniversary by holding a Dinner at the University of New England Union. About 80 members and guests were present. On Monday, 24th March, a symposium on "Biological Evolution" was held and the speakers were Professor K.S.W. Campbell, Dr. George Miklos and Associate Professor R.H. Crozier. In the evening Professor D.P. Craig delivered the 25th Anniversary Address entitled, "Science, its private and its public face". The Society's gratitude is extended to Professor R.L. Stanton for arranging these most successful functions.

FINANCE

The Society's financial year extends from January to December. In 1985 a deficit was incurred of $1613. General income increased by nearly $3000 but expenses increased by $9400 compared to 1984. The largest increase in expenditure was for the Journal ($3200); other increases were for the Newsletter ($900) and Office Relocation ($800). The Society's office was open at its new location for the whole year, resulting in a return of rental and salaries costs to their previous levels, increasing by $2400 and $1800 respectively.

An 'Extraordinary item' appears in the Statement of Accumulated Funds. $20,849 was received at last from the Liquidators of Science House Pty. Ltd. A return of this order was foreshadowed in the Report for 1984; it represents only five percent of the capital originally invested and can only be described as a disastrous conclusion for the Society of a chain of events extending over some twelve years. A Society such as ours should clearly not venture into such activities without even greater caution and better than the "expert advice" that were adopted in the early 1970s.

The acquisition of the liquidation proceeds and maturing of some investments will enable the Council to consolidate the funds into a larger mortgage investment. This was intended to proceed in the current year but has been delayed, and accounts for the substantial increase in the Interest Bearing Deposits entry in the Balance Sheet.

The Library Fund benefited by a generous donation of $500.

The Trust Funds' income is apportioned according to the overall earning rate of the Society's investments for the year and is therefore essentially consistent from year to year. The expenditure of individual funds fluctuates considerably depending on the activity or lack of it in a given year. In 1985 the grossed result showed a decline of $1250.

Mr. A.M. Puttock F.C.A., the Society's accountant and auditor again gave valuable advice and assistance which it is a pleasure to acknowledge.

THE ROYAL SOCIETY OF NEW SOUTH WALES

AUDITORS REPORT

In our opinion:

(a) the attached accounts, set out on pages 2 to 10 which have been prepared under the historical costs convention, are properly drawn up in accordance with the Rules of the Society and so as to give a true and fair view of the state of affairs of the Society at 31st December 1985 and of the results of the Society for the year ended on that date; and

(b) the accounting records and other records, and the registers required by the Rules to be kept by the Society have been properly kept in accordance with the provisions of those Rules.

WYLIE & PUTTOCK
Chartered Accountants.

By ALAN M. PUTTOCK
Registered under the Public Accountants Registration Act, 1945 as amended.

BALANCE SHEET as at 31/12/85

	RESERVES			
7310.57	Library Reserve (note 2(a))			7310.57
5405.97	LIBRARY FUND (note 2(b))			6162.04
21454.25	TRUST FUNDS (note 3)			20104.21
78496.67	ACCUMULATED FUNDS			97879.74
112667.46	TOTAL RESERVES AND FUNDS			131456.56
	Represented by:			
	CURRENT ASSETS			
117.30	Petty Cash Imprest		169.30	
2849.00	Debtors for Subscriptions	2547.60		
2849.00	Less Provision For Doubtful Debts	2547.60		
0.00			0.00	
3933.37	Other Debtors & Prepayments		5644.03	
13911.11	Interest Bearing Deposit		0.00	
7259.91	Cash at Bank		9706.92	
25221.69				15520.25
9967.10	Less: CURRENT LIABILITIES Sundry Creditors & Accruals		11384.00	
27.37	Life Members Subscriptions - Current Portion		27.37	
77.88	Membership Subscriptions Paid in Advance		76.91	
979.71	Subcriptions to Journal Paid in Advance		1343.79	
11052.06				12832.07
14169.63	NET CURRENT ASSETS			2688.18
	Add: FIXED ASSETS			
1397.66	Furniture, Office Equipment, etc.- at cost less Depreciation		1229.66	
13600.00	Library - 1936 Valuation (note 4)		13600.00	
10.00	Pictures - at cost less Depreciation		10.00	
15007.66				14839.66
29177.29				17527.84
	Add: INVESTMENTS			
8700.00	Commonwealth Bonds & Inscribed Stock		8700.00	
60000.00	Loans on Mortgage		60000.00	
15000.00	Interest Bearing Deposits		45411.18	
83700.00				114111.18
112877.29				131639.02
	Less: NON-CURRENT LIABILITIES			
209.83	Life Members Subscriptions - Non-Current Portion			182.46
112667.46	NET ASSETS			131456.56

J.H. LOXTON — President

A.A. DAY — Honorary Treasurer

STATEMENT OF ACCUMULATED FUNDS
For the Year Ended 31st December 1985

(4819.24)	OPERATING DEFICIT for year		1612.78
0.00	EXTRA-ORDINARY ITEM (note 5)		20849.50
4819.24	DEFICIT & EXTRA-ORDINARY ITEM		19236.72
388.92	Donations & Interest to Library Fund		902.42
0.00	Transfer from Library Fund		146.35
354.50	Recouped on Disposal Painting		0.00
73322.93	Accumulated Funds - Beginning of Year		78496.67
78885.59	AVAILABLE FOR APPROPRIATION		98782.16
388.92	Transfer to Library Fund	902.42	
388.92			902.42
78496.67	ACCUMULATED FUNDS Current Year		97879.74

NOTES TO AND FORMING PART OF THE ACCOUNTS
For the Year Ended 31st December 1985

1. SUMMARY OF SIGNIFICANT ACCOUNTING POLICIES

Set out hereunder are the significant accounting policies adopted by the Society in the preparation of its accounts for the year ended 31st December, 1985. Unless otherwise stated, such accounting policies were also adopted in the preceding year

(a) Basis of Accounting

The accounts have been prepared on the basis of historical costs

(b) Depreciation

Depreciation is calculated on a written down value basis so as to allow for anticipated repair costs in later years.
The principal annual rates in use are:

Furniture	7.50%
Office Equipment	15.00%

2. MOVEMENTS IN PROVISIONS AND RESERVES

(a) Library Reserve

7310.57	Balance at 1st January		7310.57
7310.57			7310.57
0.00	Transfer to Accumulated Funds	0.00	
0.00			0.00
7310.57	Balance at 31st December		7310.57

(b) Library Fund

5017.05	Balance at 1st January		5405.97
388.92	Add Donations and bank interest		902.42
5405.97			6308.39
0.00	Less Library purchases and expenses	146.35	
0.00			146.35
5405.97	Balance at 31st December		6162.04

3. TRUST FUNDS

	Clarke Memorial Fund - Capital			
5000.00	Balance at 1st January	5000.00		
5000.00	Balance at 31st December		5000.00	
	Clarke Memorial Fund - Revenue			
894.00	Revenue Income for Period	758.57		
110.19	Less Expenditure for Period	1965.83		
783.81		(1207.26)		
334.37	Balance at 1st January	1118.18		
1118.18	Balance at 31st December		(89.08)	
6118.18				4910.92
	Walter Burfitt Prize Fund - Capital			
3000.00	Balance at 1st January	3000.00		
3000.00	Balance at 31st December		3000.00	
	Walter Burfitt Prize Fund - Revenue			
536.00	Revenue Income for Period	455.14		
586.45	Less Expenditure for Period	0.00		
(50.45)		455.14		
2036.24	Balance at 1st January	1985.79		
1985.79	Balance at 31st December		2440.93	
4985.79				5440.93

	Liversidge Bequest Fund - Capital			
3000.00	Balance at 1st January	3000.00		
3000.00	Balance at 31st December		3000.00	
	Liversidge Bequest Fund - Revenue			
536.00	Revenue Income for Period	455.14		
39.45	Less Expenditure for Period	16.65		
496.55		438.49		
577.54	Balance at 1st January	1074.09		
1074.09	Balance at 31st December		1512.58	
4074.09				4512.58
	Olle Bequest Fund - Capital			
4000.00	Balance at 1st January	4000.00		
4000.00	Balance at 31st December		4000.00	
	Olle Bequest Fund - Revenue			
715.00	Revenue Income for Period	606.86		
366.00	Less Expenditure for Period	1643.27		
349.00		(1036.41)		
1927.19	Balance at 1st January	2276.19		
2276.19	Balance at 31st December		1239.78	
6276.19				5239.78
21454.25	Total Trust Funds			20104.21

4. LIBRARY

During the 1983 year the Society gifted the serials collection component of the library to the University of New England. The Society has retained that section of the library which is of historical significance. At the 31st December, 1985 a current valuation of the library had not yet been obtained.

5. EXTRAORDINARY ITEM

	During the year the following revenue items were received which were outside the ordinary operations of the Society.	
0.00	Received from Liquidator Science House Pty Ltd	20849.50

INCOME AND EXPENDITURE ACCOUNT
For the Year Ended 31st December 1985

	INCOME			
7540.80	Membership Subscriptions - Ordinary			7486.20
27.37	Membership Subscriptions - Life Members			27.37
42.80	Application Fees			22.80
7610.97				7536.37
4825.86	Subscriptions and Contributions to Journal Publication Costs			8535.41
12436.83	Total Membership and Journal Income			16071.78
13132.79	Interest Received			12952.30
194.30	Sale of Reprints			13.70
0.00	Sale of Back Numbers			131.80
8.00	Sale of Other Publications			43.00
1200.00	Donations - General			0.00
0.00	Annual Dinner Surplus			17.48
26.86	Summer School Surplus			949.01
411.23	Other Income			182.00
27410.01				30361.07
	Less:EXPENSES			
1080.00	Accountancy Fees		1160.00	
63.65	Annual Dinner		0.00	
540.00	Audit Fees		580.00	
27.34	Bank Charges & Government Duties		25.45	
150.00	Branches of the Society		150.00	
171.00	Depreciation		168.00	
146.07	Entertainment Expenses		404.83	
203.22	Insurance		102.45	
6655.00	Journal Publication Costs	9880.00		
1622.20	Wrapping & Postage	1648.96		
8277.20			11528.96	
0.00	Library Expenses		146.35	
1851.25	Library and Office Relocation		2670.44	
76.50	Miscellaneous Expenses		186.54	
1660.45	Newsletter Printing & Distribution		2545.85	
280.70	Postage		260.29	
740.43	Printing & Stationery - General		692.02	
1373.40	Provision for Doubtful Debts		753.60	
927.08	Rent		2783.00	
117.60	Repairs & Maintenance		186.60	
4824.12	Salaries		7224.17	
80.76	Telephone		405.30	
22590.77				31973.85
(4819.24)	DEFICIT for the year			1612.78

STATEMENT OF SOURCE AND APPLICATION OF FUNDS
For the Year Ended 31st December 1985

	SOURCE OF FUNDS		
4819.24	Operating surplus for the year	0.00	
	Add:		
	Items not involving the outlay of funds in the current period:		
171.00	Depreciation of fixed assets	0.00	
443.40	Provision for doubtful debts	0.00	
5433.64	Funds derived from operations		0.00
388.92	Donations and interest to library fund		902.42
2681.00	Trust fund income		2275.71
0.00	Reduction in working funds		11782.67
0.00	Liquidators Distribution Science House Pty Limited		20849.50
354.50	Proceeds disposal painting		0.00
8858.06			35810.30
	APPLICATION OF FUNDS		
0.00	Operating deficit for the year	1612.78	
	Less:		
	Items not involving the outlay of funds in the current period:		
0.00	Depreciation of fixed assets	168.00	
0.00	Provision for doubtful debts	(301.40)	
0.00	Funds applied to operations		1746.18
27.37	Reclassification of life members subscriptions in advance		27.37
0.00	Increase in investments		30411.00
1102.09	Trust fund expenses		3625.75
7728.60	Increase in working funds		0.00
8858.06			35810.30

ABSTRACT OF PROCEEDINGS 1985-86

The Annual General Meeting and nine General Monthly Meetings were held at Macquarie University. Abstracts of the proceedings of these meetings are given below.

In addition the Clarke Memorial Lecture was delivered on 30th September, 1985, by Professor R.L. Stanton, at the University of Sydney. The title of the Lecture was "Stratiform Ores and Geological Processes". Two seminars in the series entitled "Scientific Sydney" were held on 18th May and 12th October, 1985, in conjunction with the Royal Australian Historical Society at History House, Macquarie Street. The titles were "Culture and Learning in the Colonial Metropolis", and "Professor John Smith (1821-1885) and Victorian Sydney: A Centennial Retrospect", respectively.

APRIL 3rd

965th General Monthly Meeting. Location: Room T4, Building E7B, Macquarie University. The President, Dr. R.S. Bhathal, was in the Chair and 27 members and visitors were present. David Donald Sheumack was elected to membership.

Paper read by title only: "Proposed Physical Mechanism Linking Changes in Solar Activity with Some Aspects of the Weather", by V. Kastalsky, C.B. Kirkpatrick and A.T. Daoud.

The death of Dr. Florrie Mabel Quodling on 16th February, 1985, was announced.

118th Annual General Meeting. Followed the 965th General Monthly Meeting. The Annual Report of Council and the Annual Financial Report were adopted.

The following awards for 1984 were announced and presented: Clarke Memorial Medal to Associate Professor Michael Archer; the James Cook Medal to Professor Ronald Lawrie Huckstep; the Royal Society Medal to Dr. Robert Sylvester Vagg; the Edgeworth David Medal to Dr. Alan James Husband; and the Archibald D. Olle Prize (shared) to Mr. R.A.L. Osborne, and T.J. Goodwin, R.S. Vagg and P.A. Williams.

Messrs. Wylie and Puttock, Chartered Accountants, were elected Auditors for 1985.

The following Office-Bearers were elected for 1985/86:

President:	Associate Professor J.H. Loxton
Vice-Presidents:	Dr. R.S. Bhathal
	Professor T.W. Cole
	Mr. W.H. Robertson
	Professor R.L. Stanton
	Dr. R.S. Vagg
Hon. Secretaries:	Mr. D.S. King
	Mrs. M. Krysko v. Tryst (Editor)
Hon. Librarian:	Dr. F.L. Sutherland
Hon. Treasurer:	Dr. A.A. Day

Members of Council: Miss P.M. Callaghan, Mr. H.S. Hancock, Professor R.M. MacLeod, Mr. E.D. O'Keeffe, Mr. M.A. Stubbs-Race, Dr. D.J. Swaine, Dr. W.J. Vagg, and Associate Professor D.E. Winch.

The retiring President, Dr. R.S. Bhathal, delivered his Presidential Address entitled "Science Centres and/or Science Museums in Australia".

The incoming President, Associate Professor J.H. Loxton, was installed and introduced to members.

MAY 1st

966th General Monthly Meeting. Location: Room T4, Building E7B, Macquarie University. The President, Associate Professor J.H. Loxton, was in the Chair, and 24 members and visitors were present. Dr. Ian Malcolm Davison was elected to membership.

Papers read by title only: "Science Centres and/or Science Museums for Australia" (Presidential Address) by R.S. Bhathal; "Why Bother About Science?" (Annual Dinner Address) by R. Hanbury Brown.

An address entitled "Aqueous Chemistry in the Desert: A Mineral Bonanza" was delivered by Dr. P.A. Williams of the Department of Chemistry, University College, Cardiff.

JUNE 5th

967th General Monthly Meeting. Location: Room T4, Building E7B, Macquarie University. The President, Associate Professor J.H. Loxton, was in the Chair, and 44 members and visitors were present. Michael Archer, Martin Laurence Stubbs-Race and Ronald Ralph Fenton were elected to membership.

A lecture entitled "City Living and Photochemical Smog" was given by Mr. Graham Johnson, Senior Research Scientist at the CSIRO Division of Fossil Fuels, North Ryde.

JULY 3rd

968th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The Vice-President, Dr. R.S. Bhathal, was in the Chair, and 19 members and visitors were present.

Papers read by title only: "The Electromagnetic Pinch: from Pollock to the Joint European Torus" by R.S. Pease, FRS (Pollock Memorial Lecture, 1984); "Biology at the Frontier" by Charles Birch (Address to the Faculty of Science Centenary, University of Sydney).

An address on "The Acquired Immune Deficiency Syndrome (AIDS)" was given by Dr. Denis Wakefield of the School of Pathology, University of N.S.W.

AUGUST 7th

969th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Associate Professor J.H. Loxton, was in the Chair, and 13 members and visitors were present.

Papers read by title only: "Genetic Engineering - By Man For Man" by J. Peacock; "Science versus Law: the Next Century" by M.D. Kirby (two papers delivered at the Faculty of Science Centenary, University of Sydney.

The death was announced of Life Member, Mrs. Daphne Lydia Hayes, who joined the Society in 1943. She died on 27th June, 1985.

A revision of the By-Law relating to the election of members was announced.

An address entitled "Ceramics in N.S.W." was given by Mr. John Wade, Curator of Ceramics at the Power House Museum.

SEPTEMBER 4th

970th General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Associate Professor Loxton, was in the Chair, and 12 members and visitors were present.

Papers read by title only: "Does Technology Need Science?" by T.W. Cole; "Geography: An Integrative Science" by B.G. Thom; and "Science and Truth" by L. Reinhardt. (Lectures given at the Faculty of Science Centenary, at the University of Sydney)

A lecture entitled "Archives of Science" was given by Mr. Ken E. Smith, Archivist of the University of Sydney.

OCTOBER 2nd

971st General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Associate Professor Loxton, was in the Chair, and 23 members and visitors were present. Drs. Simon Charles Gandevia and Andrew John Michael Krzyszton were elected to membership.

Papers read by title only: "Science and Gambling: Psychological Perspectives" by M.B. Walker (Paper delivered at the Faculty of Science Centenary at the University of Sydney); "?Permian Palaeokarst at Billys Creek, New South Wales" by R.A.L. Osborne and D.F. Branagan.

A lecture entitled "Ocean Currents in Australian Waters" was given by Mr. Bruce Hamon.

NOVEMBER 6th

972nd General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Associate Professor J.H. Loxton, was in the Chair, and 20 members and visitors were present. Veronica Jean James was elected to membership.

Papers read by title only: "Beneficent Providence and the Quest for Harmony: the Cultural Setting for Colonial Science in Sydney, 1850-1890" by Gregory Melleuish; "The Agricultural Society of New South Wales and its Shows in Colonial Sydney" by B.H. Fletcher; and "The Architecture of Scientific Sydney" by Joan Kerr. (Three papers delivered at the Scientific Sydney Seminar on 18th May, 1985).

A lecture illustrated with videos on "The Cochlear Implant" was delivered by Professor Bill Gibson, Professor of Otolaryngology at the University of Sydney.

DECEMBER 4th

973rd General Monthly Meeting. Location: Room 100, Building E7B, Macquarie University. The President, Associate Professor J.H. Loxton, was in the Chair, and 17 members and visitors were present.

Papers read by title only: "The Volatile Leaf Oils of Two Cultivars of *Callistemon viminalis*" by J.J. Brophy, E.V. Lassak and R.F. Toia; Papers delivered at the Scientific Sydney Seminar on 12th October, 1985.

A lecture entitled "Richard Threlfall's Australian Career" was delivered by Professor R.W. Home, Professor of History and Philosophy of Science, Melbourne University.

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NOTICE TO AUTHORS

A "Style Guide to Authors" is available from the Honorary Secretary, Royal Society of New South Wales, PO Box 1525, Macquarie Centre, NSW 2113, and intending authors *must* read the guide before preparing their manuscript for review. The more important requirements are summarized below.

GENERAL

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Manuscripts submitted by a non-member must be communicated by a member of the Society.

Each manuscript will be scrutinised by the Publications Committee before being sent to an independent referee who will advise the Council of the Society on the acceptability of the paper. In the event of rejection, manuscripts may be sent to two other referees.

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Manuscripts should be arranged in the following order: title; name(s) of author(s); abstract; introduction; main text; conclusions and/or summary; acknowledgements; appendices; references; name of Institution/Organisation where work carried out/or private address as applicable. A table of contents should also accompany the paper for the guidance of the Editor.

Spelling follows "The Concise Oxford Dictionary".

The Systeme International d'Unites(SI) is to be used, with the abbreviations and symbols set out in Australian Standard AS1000.

All stratigraphic names must conform with the International Stratigraphic Guide and must first be cleared with the Central Register of Australian Stratigraphic Names, Bureau of Mineral Resources, Geology and Geophysics, Canberra.

Abstract. A brief but fully informative abstract must be provided.

Tables should be adjusted for size to fit the format paper of the final publication. Units of measurement should always be indicated in the headings of the columns or rows to which they apply. Tables should be numbered (serially) with Arabic numerals and must have a caption.

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Note: There is a reduction of 30% from the master manuscript to the printed page in the journal.

Maps, diagrams and graphs should generally not be larger than a single page. However, larger figures can be printed across two opposite pages.

Drawings should be made in black Indian ink on white drawing paper, tracing cloth or light-blue lined graph paper. All lines and hatching or stripping should be even and sufficiently thick to allow appropriate reduction without loss of detail. The scale of maps or diagrams must be given in bar form.

Half-tone illustrations (photographs) should be included only when essential and should be presented on glossy paper.

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References are to be cited in the text by giving the author's name and year of publication. References in the reference list should follow the preferred method of quoting references to books, periodicals, reports and theses, etc., and be listed alphabetically by author and then chronologically by date.

Abbreviations of titles of periodicals shall be in accordance with the International Standard Organization IS04 "International Code for the Abbreviation of Titles of Periodicals" and International Standard Organization IS0833 "International List of Periodical Title Word Abbreviations" and as amended.

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The Journal is printed by offset using pre-typed pages. When a paper has been accepted for publication the author will be supplied with a set of special format paper. The text may either be typed by electric typewriter directly on to the format paper or a word-processor print-out assembled on it. Details of the requirements for text production will be supplied with the format paper.

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THE ROYAL SOCIETY OF NEW SOUTH WALES

The Society originated in the year 1821 as the Philosophical Society of Australia. Its main function is the promotion of Science through the following activities: Publication of results of scientific investigation through its Journal and Proceedings; the Library; awards of Prizes and Medals; liaison with other Scientific Societies; Monthly Meetings; and Summer Schools for Senior Secondary School Students. Special Meetings are held for the Pollock Memorial Lecture in Physics and Mathematics, the Liversidge Research Lecture in Chemistry, and the Clarke Memorial Lecture in Geology.

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25th Anniversary of the New England Branch of the Royal Society of New South Wales

R. L. Stanton

To mark the Twenty-fifth Anniversary of the establishment of the New England Branch of the Society, and the occasion of the re-opening of the Society's Library within the Dixson Library of the University of New England, a symposium on "Biological Evolution" was held in the Department of Physics, University of New England, during the day on Monday, 24th March, 1986. This was followed in the evening by the Twenty-fifth Anniversary Lecture.

The symposium was led off by Professor K.S.W. Campbell, FAA, of the Australian National University, who gave a general introduction to the principles of biological evolution, and then went on to show how the mechanisms of evolutionary change may themselves have evolved over time. His talk, "Evolution Evolving", was designed to introduce the layman to some of the more recent ideas in evolutionary biology, and showed that the emerging picture is not one of stately progression, but rather one of great bursts of change and diversification followed by episodes of extinction of varying severity.

Associate Professor R.H. Crozier of the Evolutionary Biology Laboratory, University of New South Wales then spoke on "Selection, adaptation and evolution", examining evolutionary trends from the point of view of population genetics and "adaptive evolution".

This was followed by a contribution from Dr. G.L.G. Miklos of the School of Population Biology, Research School of Biological Sciences, Australian National University, entitled "Molecular facts and evolutionary theory". As had Professors Campbell and Crozier, Dr. Miklos introduced his subject to the layman, and then went on to discuss ways in which DNA may be manipulated, the ways in which genomic components control the morphology of organisms, and aspects of molecular embryology and the role of these in biological evolution.

In the evening, Professor D.P. Craig, FAA, FRS, of the Research School of Chemistry, Australian National University and Honorary Member of the Society, gave a very thoughtful and topical Twenty-fifth Anniversary Lecture on "Science: its private and its public face".

Journal and Proceedings, Royal Society of New South Wales, Vol.120, pp. 3-8, 1987
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Science: The Private and the Public Faces

D. P. Craig

ABSTRACT

Science as an activity and an enterprise is seen differently by practitioners, who are the scientists themselves, and by those outside. The differences in perception are nowhere greater than in the views taken of fundamental research. Is it the jewel in the science crown, and the kernel from which grow advances in the physical conditions of our lives, or is it a self-indulgent hobby of a few pampered intellectuals?

Examples abound to show that the forward movement through the applications of science always depends in the beginning on fundamental research. Progress from that start rests on cooperative effort by pure and applied scientists. The laboratory and the marketplace both act as stimuli.

It is a singular fact that there is little understanding that the training of scientists in research methods is mainly through their participation, as students, in programs of pure research in universities. We rely on such research to produce trained research workers as well as new science. These are the men and women who will carry forward new technologies in Australian industry and elsewhere.

INTRODUCTION

We are fortunate on an occasion like this to be able to reflect on the state of our subject, not so much on its technical advancement, but, if one may use an already much-used term, on its 'health'. My particular focus is on the balance between its fundamental and applied aspects. Because I have the impression that the nature and characteristics of fundamental or basic scientific research are best understood by scientists themselves, and obscure and mysterious outside their ranks, I see it as 'private' or 'inward' science. Applied research, which bears on the conditions and possibilities of everyday life, is understood by people at large; I see that as 'public' or 'outward face' science.

Fundamental research provides the discoveries, such as Clerk Maxwell's electromagnetism, which after some time appear in applications such as radio, television, and radar. We do not in the usual way talk of fundamental research results in science as a commodity that is produced by one set of people (for example Maxwell) and then acquired and consumed by another set (the inventors of television). But stating it in that way is to bring to attention distinctions familiar to all of us. Let me illustrate.

When Professor Bullen spoke on a similar Royal Society of New South Wales occasion 25 years ago on "The internal constitution of the earth" he was speaking as a *producer* of new knowledge. His motivation was curiosity. He was looking at the private or inward face of science - knowledge for its own sake, science as a cultural and intellectual activity. Those who, coming after Bullen, have taken over his findings and put them to work are, in my sense, the *consumers* of fundamental research, building in this case on knowledge of the constitution of the earth. They developed new ways of exploring for mineral deposits and oil. They are showing us the public or outward face of science.

My subject is the coming together of producers and consumers. It is a very old talking point among scientists. It is now also a political subject taken into account by government, as government decides where to spend our money. How do we get a framework within which discoveries give birth to new industries, or revive old ones? I doubt that we can handle it better now than 100 years ago, when the literature of the time had the familiar mix of success and failure, of wise diagnosis and enlightened therapy.

The balance of the argument over the support of fundamental versus applied science swings back and forward. At the time of the Murray Committee on Australian Universities (Murray 1957) the Commissioners recommended that we should build up fundamental science, with the prime aim of enlivening tertiary education and enhancing the training of scientists for university and industry. Nowadays much popular thinking has gone the other way: fundamental science is treated as a self-indulgent fringe activity, very little connected with what the critics take to be the pressing task of the moment. This task is to solve practical problems and propel our technology away from merely following, to leading, or at least performing in the front line, in this or that area of industry and high technology.

If I focus for the most part on the private face, namely the pure research end of the spectrum, I do so for the reason that it is pure research which comes more under fire. Funding for pure research is viewed with increasing scrutiny and scepticism, even at a time when awareness of the importance of science generally has grown.

The Minister of Science, Barry Jones recently made the point in his Masson Memorial Lecture when he said: 'Some bureaucrats go white and rigid at the idea of "curiosity-led research", which they see as a complete self-indulgence following whims and will-of-the-wisps at public expense, whereas ... I (that is Barry Jones) take up the point that the branch of

Research and Development	:	'Creative work undertaken on a systematic basis to increase the stock of technical knowledge and to use this stock of knowledge to devise new practical applications.'
Basic (pure) Research	:	'Original investigation undertaken in order to gain new scientific or technical knowledge and understanding.'
Applied Research (Strategic mission-oriented research)	:	'Original investigation undertaken in order to gain new scientific or technical knowledge. It is, however, directed primarily towards a specific practical aim or objective.'
Experimental Development	:	'The use of scientific knowledge in order to produce new or substantially improved materials, devices, products, processes, systems or services.'

Fig 1. The OECD definitions

physics that has given us the transistor radio, the Sony Walkman, digital watches, pocket calculators, microcomputers and programmable washing machines has been based on what used to be regarded as the most esoteric and remote of the intellectual pursuits - quantum mechanics'. It would however be a reckless exaggeration to claim that this is more than the view of an informed minority, paddling against a stream which runs in the direction of 'relevance' seen, I am afraid, as relevance only to immediate purposes and ends, and not to means.

THE PRIVATE FACE

Research activities across the complete span from the most fundamental to the most applied are described in the definitions used by the Organization for Economic Cooperation and Development (Fig. 1).

The new awareness of the importance of scientific discovery and invention has had some effect in improving the crossing of barriers between the OECD categories of research. Government initiatives in the recent past have included the 150% tax deduction on R and D expenditure; The Commonwealth Scientific and Industrial Research Organization (CSIRO) now has its many channels to industry supplemented by SIROTECH, a technology transfer and commercial assessment company, and universities can embrace industry through affiliates like UNISEARCH in the University of NSW and ANUTECH in the Australian National University. But the new awareness has a darker side. It has stimulated too much attention on the products of applied research, seeing them as ends that have no beginning. Thus it has undermined the former acceptance that fundamental research is required to improve knowledge and understanding, which must come before applications. One even hears extreme suggestions, such as that we need not concern ourselves at all with basic research in Australia, but should buy the right to be second in the race, by buying our science from overseas.

What are we talking about when discussing pure research, as opposed to development and applications of research? Pure research, basic research, fundamental research - what do these words mean? Einstein in a charming aside, in autobiographical notes, refers to the 'Holy curiosity of enquiry' (Pais, 1982, p 50).

> " ... the holy curiosity of enquiry; for this delicate little plant, aside from stimulation, stand mainly in need of freedom; without this it goes to wreck and ruin without fail. It is a very grave mistake to think that the enjoyment of seeing and searching can be promoted by means of coercion and a sense of duty."

There are not so many Einsteins about, and even if there were, I doubt that our grant-giving bodies would spring open their purses to give free rein to 'the holy curiosity of enquiry'. Yet the most successful granting bodies, and we have examples in Australia, do hand out some funds to scientists whom enquiries show to be of the highest ability and tell these people to get on with it in their own way. To view this as a licence 'to follow whims and will-o-the-wisps at public expense' is to miss entirely the point that pure research is an activity proceeding within its own rigid discipline. Not the discipline of starting work at 9, and stopping at 5, but the discipline that comes from having to satisfy internationally accepted standards of quality in published research work, from having one's work scrutinized by colleagues and rivals. Einstein's own great contributions in relativity and quantum theory were not accepted at once as the enormous steps that we now see them to be. Many years passed before the scientific community was convinced. The great physicist Heisenberg wrote at the time that "Einstein 'outraged many leading philosophers and physicists, and turned them into bitter opponents'" (Pais, 1982, p 82).

This is the inward face of science, in contrast to the outward or worldly face. Let us pause for a moment to consider some of the characteristics of this 'inward face'. Universities are its most characteristic home. It is special and peculiar to research in universities that it goes on in conjunction with undergraduate teaching and research training.

These activities impress upon research generally features not found elsewhere, not even in the largest and most distinguished industrial laboratories.

First there is the association of research with undergraduate teaching. Lecturing to undergraduates confers a double benefit. The undergraduate gains by contact with maturer minds while the lecturer for his part also learns. To give his lectures he is forced to go back to the fundamental basis of his subject, in order to convince himself that he really understands, and is able to express ideas with clarity and lucidity. Of course, we are not all perfect. Clarity and lucidity, in various cases, come in smaller or larger packages. I had a colleague whose lectures were described by an acid critic as 'illuminating any subject he touched with shafts of darkness'. But lucid or not, the lecturer has to convince enquiring minds, many the equal of his own in sharpness and quality. He faces questions by those who need to be convinced. Moreover the elementary parts of sciences like chemistry and physics are in a sense harder than the advanced parts. The reason for the apparent paradox is that once the mind is prepared, by familiarity, with the concepts and basis of a subject, the rest is development and extension, and comes more easily. And so it is, I would argue, that the teaching-research nexus is a leading source of university strength in fundamental research.

Now we go to the question of training in research. The familiar pattern is that many of the best undergraduates choose to go on to post-graduate work. They attach themselves to a member of an academic staff, who is established as a research worker. It is a relation of teacher and learner. The teacher proposes a research topic. He teaches the pupil how to do the work, how to make justified deductions from what is found, and how to write up the results. At the end of the course, leading to a doctorate, the learner is expected to be trained in research methods. But again, what has been the effect on the senior partner? - His attention is focussed on fundamentals; he is forced to concentrate on the next step in the research, and he profits from interaction with an able younger person in an atmosphere of free exchanges of views.

I have gone through all this to establish a second main feature of university research in science. The first is the concentration on pure and fundamental science. The second is the relationship of learner and teacher. It always has been to me a striking fact that ideas pass much more readily between two people cast in a situation of leader and led, of teacher and learner, than between two people of equal status, working together in a laboratory. We find that the learner readily adopts or takes over lines of research from his teacher, and in turn, when he becomes a senior worker, passes to his students lines of research and attitudes which have grown out of his own experience.

One can point to four features of the inward or private face of science:

1. Motivation by curiosity.

2. Importance of the teacher-pupil relation and the inheritance of ideas.

3. Importance of the laboratory tradition in preserving attitudes and methods.

4. Institutional factors in promoting and fostering fundamental research, the intellectual 'climate'; teaching.

THE GENEALOGY OF IDEAS

A leading and distinctive attribute of academic research is the inheritance of ideas through successive generations of research leaders. I will give an example of this inheritance (Craig, 1972). It is one with strong Australian overtones (Fig. 2).

We start with the people concerned. This is a family tree in the sense of research fathers and sons. William Jackson Pope was professor in Cambridge before and during the first war. Pope had many students who later became eminent. Two of them enter the story. F.G. Mann continued the line in Cambridge, and among his students was (now Professor) Joseph Chatt, whose career has included industrial research (with Imperial Chemical Industrys) and university research. The Australian line runs through Eustace Ebenezer Turner, who began as Pope's assistant in Cambridge, and accepted in 1920 a lectureship

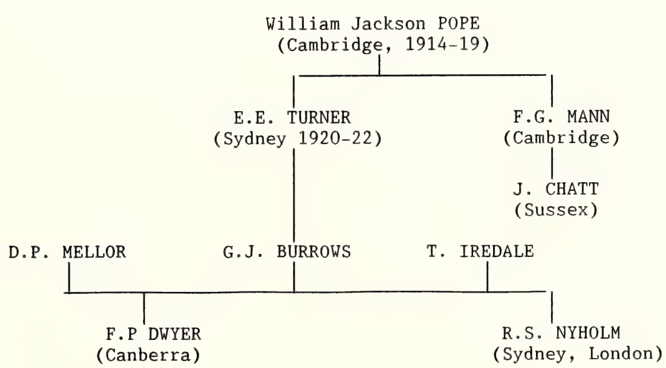

Fig 2. The genealogy stemming from Pope

in the Sydney Department of Chemistry. His student and co-worker was George Burrows. Burrows, together with Mellor and Iredale, is the founder of what is sometimes called the Sydney School of coordination chemistry, and which produced such celebrated scientific sons as Frank Dwyer, and Ronald Nyholm. Mellor did post-doctoral work with Linus Pauling and brought into the famous group of three new ideas on bonding in coordination chemistry. Iredale provided the thermodynamics, and Burrows provided the main line chemistry.

Let us see how the ideas developed. During the first war Pope was a member of the British Chemical Warfare Committee. He worked on the arsenical war gases, notably mustard gas. At the end of the war F.G. Mann was engaged with Pope on Lewisite, another arsenical gas. Afterwards, still with Pope, he turned to the basic chemistry of coordination compounds formed by metals with arsenic and phosphorus ligands. Chatt, as a pupil of Mann, extended the work into transition metals. On the Sydney side Turner started from the same background as Mann. With Burrows he made methyl and phenyl arsines and used them as ligands to produce transition metal complexes. Dwyer, at first working closely with Mellor and Burrows, extended arsine coordination chemistry to new metals such as palladium and platinium. Later he continued his studies of coordination more generally into biological systems. Nyholm, at first in the junior learner-teacher role with Burrows and Dwyer, began with new arsenic ligands and then moved independently outward into a wide spectrum of coordination chemistry, and broadened into the territory of new valence states of transition metals. So much for the family tree of ideas in this particular case. You might think that - apart from the war gases part of the story, which is better forgotten - it is a story of pure research still awaiting application.

On the contrary, several of those in the chain were involved in applications of great commercial and community importance. I add the name of B.D. Steele who does not appear in the family tree of intellectual genealogy, but belongs at one remove. Bertram Dillon Steele became Professor of Chemistry in the University of Queensland. During the first war he was drawn into the manufacture of explosives. When picric acid was replaced by TNT, the factory he managed was converted to the manufacture of arsenical war gases, using the methods of Pope and the Cambridge group. It was this experience which allowed him, on coming to Australia, to pioneer the use of arsenicals in agriculture. He led a program of attack on prickly pear by arsenical sprays, which his biographer notes as the only one to have any substantial success before the supremely successful attack by the insect cactoblastus managed by CSIRO.

Joseph Chatt extended his use of complexing and chelating agents to include phosphines as well as arsines, developing deep understanding of coordination generally. He used it to open a window on to a central problem in agriculture, the fixation of atmospheric nitrogen. Dwyer exploited his knowledge of coordination chemistry in biology by developing an agent against tooth decay; it was for a time used commercially as an additive to toothpaste.

My examples show how, in this narrow field, marketplace science has sprung from laboratory research. One can think of such research as 'speculation-led'. The motivation is the desire to find out more, and to extend and spread pure natural knowledge. Again it is the 'inward face'.

This 'inward face' is the side of scientific life which more and more has to justify itself as the pressures grow for the application of science to improve our economic situation. In the words which were once used to describe political liberty, fundamental science seems more and more to be leading a dangerous and fighting life. Even universities, which we should have thought would be the first to bar their doors, are forced to a degree to wean their scientists away from fundamental research towards applications. Universities must provide the intellectual climate in which the most original minds can follow the Einstein path towards the new ideas and new discoveries without which we have no science to apply. It is one thing to encourage vigilance in perceiving in new basic science ideas which may lead to applications, but quite another to make applications a prime objective.

AN EXAMPLE OF PUBLIC FACE RESEARCH

I turn now to the 'outward' or 'public face' of science which looks out on the world of applications broadly to improve life, and more narrowly to secure advances in primary and secondary industry, and in commerce. In academic research the source of ideas is often the forward movement of a 'line' of research, each discovery opening up new problems, often in the frame of a laboratory tradition, with techniques and aims passing from one worker to another.

It is not the case that all the applications of science directly arise from the ferment of new knowledge gained by basic research, though all ultimately depend on it, even if indirectly. At the other end of the spectrum is the applications-led research done by large industry responding closely to demands in the marketplace. For my example I have chosen the famous case of fuel detonation in engines. It is far enough in the past for us to get a perspective. The major design limitation on motor-car engines in the late twenties and thirties was engine knock or detonation. In an engine with too high a compression ratio, the fuel detonates instead of burning, causing power loss and possibly engine damage. Low compression on the other hand means poor fuel economy. An account appeared in Chemical and Engineering News at the time of the work on knock in an industrial laboratory. Thomas Midgley and his co-worker Boyd did their work by trying out, as additives to petrol, the contents of every bottle in the laboratory, or discovered in the catalogues of the chemical suppliers, that seemed even remotely possible. Some thousands of compounds were tested, and the results placed on a scale. The scale is the octane rating. We would think of it now as a shot-gun style of research, but it was then the only possible approach as there was then no understanding of detonation, nor even of the normal burning process in the engine.

It is history that in the course of this wide examination of fuel additives the remarkable potency of tetra-ethyl lead was discovered. A whole new generation of higher compression, fuel-efficient, engines was developed. Many of the later useful knock-suppressants were themselves fuels, like benzol and alcohol, and were used in Australia in the last war when tetra-ethyl lead was required for use in aircraft.

At the same time, and later, an entirely independent line of curiosity-driven pure research had started in university laboratories concerned with the burning of hydrocarbons in normal combustion under laboratory conditions, in glass apparatus for the purpose of finding the mechanism of gas phase decomposition of hydrocarbons and halogenated hydrocarbons. Then the study moved on to decomposition in the presence of added oxygen gas, and so to combustion, in which the hydrocarbon reacts with oxygen. Thus there were two parallel advancing line of enquiry. The pure research was elucidating the reaction mechanisms in radical-chain hydrocarbon combustion; the applied research was showing how, in practice, normal combustion could be promoted by additives. The pure and applied strands were soon drawn together.

I am being selective in choosing incidents out of a very long sequence. Workers in a development laboratory, long famous for applied research on internal combustion engines, now sought to establish the connection between combustion in engines and the more fundamental work on combustion in laboratory-type apparatus. They showed that formaldehyde was the only intermediate product having anti-knock action and that formaldehyde and organic peroxides yielded keys to the mechanism of chemical detonation. This important conclusion is the basis of current understanding. This understanding provided the framework for the long forward steps in engine design over the last four decades.

I describe my first example, which was the use of academic coordination chemistry leading into applications to herbicides, fixing atmospheric nitrogen, and stopping tooth decay, speculation led, 'inward-face' led, velopments into market-place science. My second example of the improvement of the octane rating of fuels, I think of as 'outward face' or applications-led research, inasmuch as the motivation was generated within the market-place and largely satisfied in industrial laboratories.

PURE RESEARCH IN AUSTRALIAN UNIVERSITIES

Critics of the research effort state that while Australian scientists produce some 2% of the world output of published papers in basic science, their output of patentable discoveries in technology is just a few tenths of one percent. The first figure puts us up with the world industrial leaders, the second leaves us near the bottom of the class. These are not new insights. The Murray Committee (Murray, 1957) reported that 'The total research effort supported by Government funds is of the same order in relation to the national income as in Britain and the US, and the really striking difference is in the low level of industry-supported research which in similar terms is about one-fifth of that in Britain.'

The critics are right when they deplore the low level of innovation in technology. They are wrong when they say we should do less in basic research. They are wrong because they misunderstand the role of universities as sources of the research.

The activities of undergraduate education, research training, and scientific research go on together. It might be possible to separate the cost of undergraduate education and that of research, but I would even there argue that the two are mutually supporting, and ought to be treated as one enterprise. But there can be no question of dividing up research funding between training research workers and generating the research output. Research training is learning by doing. It proceeds in the course of performing research tasks and is inextricably bound up with them.

This perspective shows that the pure research coming from Universities has to a large degree been performed by research trainees in the course of their training. To give an illustration. In 1982, in one particular School of Chemistry, 132 scientific papers were published. One-hundred and seventeen of them were based in part and, in some cases, totally on the work of students and postdoctoral workers in the course of their training. Fifteen papers only came from members of staff by their effort alone.

I do not know how one would extrapolate such figures over the entire university scene. Circumstances vary, according to the numbers of research students and staff members. It is certainly true that a large part of research funds from all sources, government and private, is spent in universities on the combination of research training and research output. Without the training element the work would not be done. It follows that the question whether the level of fundamental research in Australia is 'justified' must be debated from a wider standpoint. It is accepted by most scientists that the current level of research output is needed to generate innovation. Furthermore research scientists, trained in methods of research, will be required for the new initiatives in advanced materials, manufacturing industries, information technology, biotechnology, high technology electronics, and so on. The Murray Commission was already saying it in 1957: 'The numbers of scientists, and especially chemists, are much too small for present needs in Australia. The Universities cannot look forward, without immediate and generous financial help, to satisfying the need.' It is spelled out in the clearest way in the 1985 Report by the OECD Examiners:

> 'We consider the role of education and training to be essential in Australia's efforts to fulfill its scientific and technological potential .... We favour increased support for university research, and a sustained effort to improve the quality of university equipment .... In our discussion of science, technology and industry, we

urged the government to ensure that sufficient technically trained people be made available for future industrial requirements.'

In their statement is no mention of the role of universities as sources of basic research. The emphasis on supporting research is for the purpose of producing the people required for science and technology. Behind all the current initiatives for mobility from laboratory to marketplace is the need for trained scientists to do the work. As manufacturing industries step up their R and D under the 150% scheme they will look to Universities and Colleges for staff. All moves towards technological sophistication through SIROTECH and other ways imply the same needs.

It may at first seem perverse to focus on scientific manpower at a time when able and highly trained young scientists cannot find jobs to match the level and quality of their experience. All depends on the success of efforts to move R and D back along the chain, from mere laboratory control and process-minding through process improvement toward technological innovation.

CONCLUSION

I have developed two themes, both bearing on pure research, one indirectly and one directly. The first theme is the problem of promoting the currents of information to flow between the pure scientists, the mission-oriented research workers and the appliers of research. History shows, and my examples illustrate, that there is no golden road. chance contacts between people in laboratories or conferences can light the touchpaper. Some of the most striking developments have been by individual entrepreneurs with commercial instincts and a willingness to risk. In other cases, in increasing numbers, technology transfer comes through agencies set-up for that especial purpose, such as SIROTECH and ANUTECH.

Common to all agencies is the fact that scientists, having minds prepared by research training for novelty and innovation, are essential on both sides of the transfer. The higher level of scientific sophistication required in industrial scientists to enable them to grasp at possibilities that someone brings them from a laboratory can only be built by actual research experience. Universities provide such experience through their programs of fundamental research. The second theme is the pure research activity itself. The 'private', pure research, face of science is not popular with administrators and governments. It is administratively untidy. Scientists cannot make discoveries to order, but are at the mercy of chance. The primary drive is curiosity. The idea that such science prospers best when practitioners are left to themselves in conditions of free enquiry is not agreeable to those who believe in intervention, in their ability to improve by regulation. Therefore scientists, especially those in Universities, must go on asserting the value of free enquiry, and keep acting in ways that enlarge the opportunities for creative fundamental research.

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D.P. Craig
Australian National University
Department of Chemistry, The Faculty of Science
GPO Box 4
Canberra ACT 2601

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Evolution Evolving

K. S. W. Campbell

By the time he wrote the Origin of Species, Darwin had reached at least four main conclusions from his palaeontological studies:

1. Most fossils represented the hard parts (skeletons) of species that are not now found living - that is, a large number of species had become extinct.

2. Most fossils could be seen to have features in common with species that are still living - for example, skeletons that were obviously similar in many respects to those of modern reptiles could be found extending back in the record for some considerable time.

3. Overall it was possible to recognise a progression of complexity in organisms through geological time.

From these three conclusions, together with observations on living animals, he drew the following inferences: fossils are related to living organisms by descent, or as he says "species have been produced by ordinary generation: old forms having been supplanted by new *and improved* forms of life..."(Darwin, 1859); or elsewhere "The inhabitants of each successive period in the world's history have *beaten their predecessors* in the race for life, and are, in so far, higher in the scale of nature: and this may account for that vague yet ill-defined sentiment, felt by many palaeontologists, that organisation on the whole had progressed" (author's italics).

He was, however, only too well aware of a fourth point:

4. Although organisms from one period were different from, but related to, organisms from preceding and succeeding periods, it was not possible to find the graded sequences of organisms he had predicted.

From this he concluded something quite different from what might have been expected - *not* that his idea of gradual transitions between species might be incorrect, but rather that the geological record was incomplete. After a long discussion of the matter he concluded "... all these causes taken conjointly, must have tended to make the geological record extremely imperfect, and will to a large extent explain why we do not find interminable varieties, connecting together all the extinct and existing forms of life by the finest graduated steps" (Darwin, 1859).

In other words he accepted that the record overall was good enough to show the main patterns of evolution - that is, it established the fact of morphological change in a number of directions that he would have predicted, such as increasing complexity; but he considered that the record was not good enough to support the view that change had been as gradual as he thought it should be if his theory of natural selection was correct. This obviously caused him concern because he wrote: "Passing from these difficulties, all the other great leading facts in palaeontology seem to me simply to follow on the theory of descent with modification through natural selection" (Darwin, 1859, p. 343).

It is clear from this and his previous statements that he introduced a serious confusion because he did not distinguish between the concept of evolution as *descent with modification* on the one hand, and the means by which this modification was thought to have taken place - namely, *natural selection* on the other. Of course the theory of natural selection was Darwin's main contribution to evolutionary studies, but the efficacy of natural selection could not be established or confirmed from the fossil record. All that he could have done was to show that (a) morphological change had been gradual; (b) that, by devising suitable measures, successive species had become more efficient; and (c) that these observations were consistent with, but did not establish, the hypothesis of natural selection. Therefore Darwin's statements left two bad legacies - criticism of natural selection was taken by many protagonists as rejection of evolution, and the record was widely accepted to be very inadequate (Fig. 1).

NEO-DARWINISM

For the next forty years or so there was little effort on the part of palaeontologists to follow up this problem of continuously evolving sequences. This seems to have been the result of two factors: geologists were convinced that the record *was* so bad that such changes could not be found and it was useless searching for them; and many palaeontologists, while accepting that evolution had occurred, could not accept the idea that natural selection *by itself* could produce the changes observed. There were a few examples of gradual change forthcoming, such as *Micraster* in the Cretaceous chalk (Fig. 2) and *Zaphrentis* in the Carboniferous shales, but these were exceptional. Note that this is not to say that sequences could not be found, but rather that species transitions appeared at that time to be rare.

The rediscovery of Mendel's genetics, and

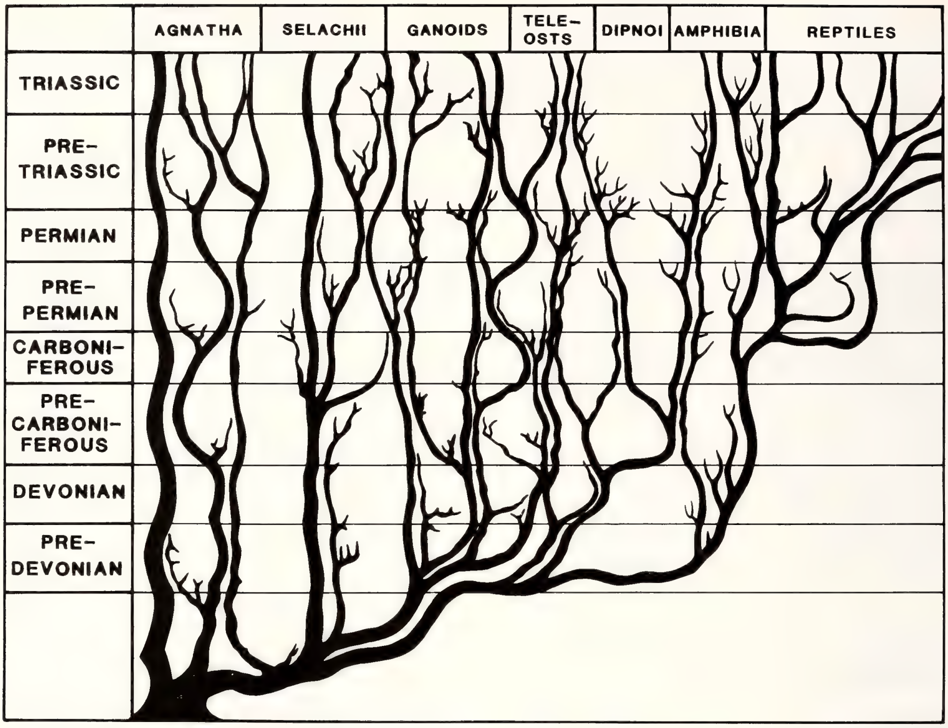

Fig. 1 Part of a figure produced in 1866 by the German biologist Haeckel. It shows his conception of the relationships between jawless fishes (Agnatha), sharks, skates and rays (Selachii), primitive ray-finned fishes (Ganoids), advanced ray-finned fishes (Teleosts), lungfishes (Dipnoi), amphibians and reptiles. This diagram illustrates the extent to which major branchings in the tree of life were considered to have taken place in time intervals unrepresented in the fossil record. Not only are the initial branches shown in undifferentiated pre-Devonian time; many of the subsequent ones are in the pre-Carboniferous, pre-Triassic etc., time intervals without rock records.

the great expansion of evolutionary thinking in the first thirty years of this century produced little impact on palaeontology, but during this period the concepts took shape that have dominated evolutionary thinking almost up till the present. These are known collectively as Neo-Darwinism or the Synthetic Theory. The essential tenets of this theory are:

1. Change took place by small steps.

2. Mutation is the raw material of evolution.

3. Mutation is not directed but occurs in a 'random' fashion.

4. Direction and rate of change were imposed by the environment.

5. Rapid evolution took place in isolated populations of moderate size.

6. Rapid evolution took place in areas of geographic differentiation.

7. Evolutionary trends resulted from unidirectional selection.

Note that this is a theory of evolutionary *mechanisms*. Clearly it will have implications for the course of evolution, and hence should be consistent with the fossil record. For example, it should be possible in many instances to correlate changes in structure with changes in environment. It is true that some structures will presumably be selected for in a constant environment, simply because they enable the organisms bearing them to perform functions more efficiently. However, other structures will change because they are required to perform more efficiently in changing environments. The classic fossil example is the evolution of the horse which shows a number of 'trends' all of which are consistent with a change from soft to hard ground, a change from soft nutritious food to hard less nutritious food, and a change from slow to fast movement, all at a time when the central North American region, where the horses were evolving, was changing from wet forest to dry prairie (Stahl, 1974).
Although it played some part in the formulation of the synthetic theory, palaeontology was essentially reduced to providing possible examples of the theory of natural selection in action.

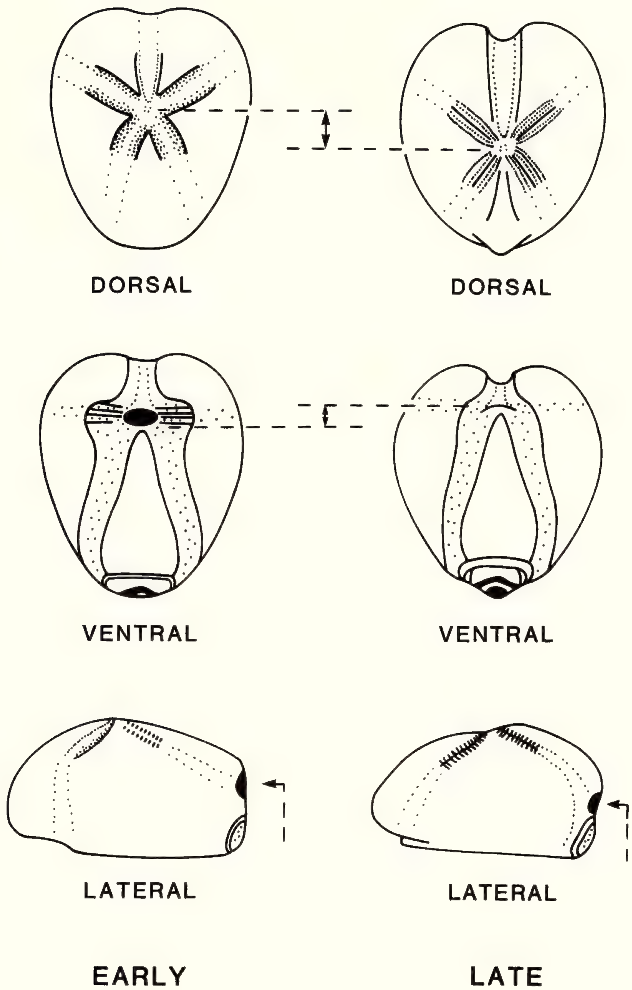

Fig. 2 Early and late representatives of the sea-urchin *Micraster* from the Chalk of England. They are separated by about 8 million years. During this time the conditions of sedimentation over the chalk basin remained relatively constant, and the genus was probably able to inhabit some part of the basin during that entire period. The main changes that took place involve proportions of the whole skeleton, the shapes of furrows and ridges, the position and shape of the mouth and its lip, the shape and division of the food-gathering (ambulacral) areas, and the general granulation of the whole surface. Several species have been recognised over the time interval; these have been arbitrarily defined because successive populations show intergradation.

SOME DISSENTERS

Now this synthetic theory or Neo-Darwinism, was generally accepted by biologists, and it is still accepted by the majority today. However, in the 30's and 40's there were two groups who were uneasy about it.

The first group was typified by one of the most influential evolutionary thinkers of this century - George Gaylord Simpson, an American. He wrote a book entitled "Tempo and Mode in Evolution" in 1944, and a revised and expanded book "The Major Features of Evolution" in 1953. He was not worried about the basics of the synthetic theory - in fact he was one of its main supporters; in particular he was convinced that natural selection was *the* directing force in

evolution. His main reservation was that the theory did not explain all the observed phenomena. Though in his view the synthetic theory could explain gradual change from species to species, he doubted whether it could explain the evolution of groups such as families or even higher taxa, which often appear in the fossil record abruptly. Is it reasonable, he was asking, to expect that very rapid small-scale evolution could and did produce the large-scale abrupt effects that we observe? He concluded that such an explanation was not acceptable. Instead he decided that evolution had several modes. These came to be known by a variety of names, but we can consider them here as microevolution, macroevolution and megaevolution. Subsequently he retracted the term megaevolution.

Microevolution was the kind of evolution that resulted in gradual transition between species, produced by a gradual spread of new genes through a population. *Micraster* provides a good example from the fossil record. Macroevolution on the other hand, was the kind of evolution that produced higher taxa. He considered that the observed gaps between these taxa were not due to gaps in the record, but real evolutionary phenomena requiring a special explanation. This explanation involved especially high rates of evolution - not just rates at the upper end of the microevolution range, but a special class of rates. However, the 'determinants', as he called them, were the same. They just came together in unusual ways at various times to produce high rates. Simpson still wanted *external* controls on tempo and mode of evolution in the usual manner of the Neo-Darwinians.

The second group of dissenters had no one scientist to act as a focus. Nor did they have a thought-out, unified, position to advocate. Among them were numbered the German palaeontologist Schindewolf, the French zoologist Grasse, the American geneticist Goldschmidt, and the English biologist L.L. Whyte. What enables us to consider these people as a group is the fact that they considered external factors to be inadequate to explain tempo and mode, and that therefore some factor internal to the organism (preferably some genetic factor) played a major role, particularly in introducing evolutionary novelty in an irregular fashion. Unfortunately for them, they were never able to put their fingers on this elusive factor. As a result it became fashionable to pour scorn on them especially in the English-speaking world. In any historical science an hypothesis without a mechanism has traditionally been an object of pity, as witness the fate of Wegener's hypothesis of continental drift until the mechanisms of plate tectonics were discovered.

So by the mid-1950's there were three groups of evolutionists differing in their views of the course of evolution and the mechanisms that controlled it:

1. Neo-Darwinists who thought that change was gradual, perhaps a little faster here and a little slower there; that the apparent jumps between major groups were the result of imperfections in the fossil record; and that direction and rate were the result of natural selection, acting on phenotypes that differed slightly as the result of small mutations.

2. Simpsonians who were essentially Neo-Darwinists but who considered that not all change was gradual - some was very rapid and took place under special environmental conditions.

3. Internalists who thought that change was very variable; that the jumps between major groups were real; and that direction and rate were at least in part the result of some internal factor as yet undiscovered.

The conflict between these three groups remains to be resolved, and it is interesting to note a rough correlation between adherents of each of the groups, and areas of scientific endeavour. For example, population geneticists tend to belong to the first group; palaeontologists are over represented in the second group; and molecular biologists of various persuasions are main contributors to the third group. This in itself should alert us to the possibility that evolution is not a uniform process, and that scientists working in different fields tend to emphasise different aspects of the process. We should not conclude, as some non-scientists do, that this disagreement casts doubt on the whole concept of 'evolution'. To do so is an elementary mistake in logic.

Numerous contributions to the resolution of the conflict have been made, and obviously they come from a variety of quarters. The subsequent papers in this symposium deal with biological contributions, and I will now discuss three quite different palaeontological approaches, all of which relate to metazoan organisms.

THREE PALAEONTOLOGICAL ARGUMENTS

First Argument

The first approach is to examine the fossil record to see if major groups of animals converged towards their times of origin. This has been important because the Neo-Darwinian model would have produced patterns as in Figure 3A whereas the Simpsonian model would have resulted in patterns as in Figure 3B.

The results of this kind of work have been ambiguous. Analysis of the various classes of molluscs shows that when the critical changes took place the animals concerned were small and were very similar to one another in all the observable characters (Runnegar, 1987). The echinoderms on the other hand seem to show that most of the classes are distinct from the time they appear in the record, and they are not represented by unusually small species. One thing the two groups have in common is the fact that most of the classes were established very early in the Palaeozoic - that is by about 470 million years ago. Also several of the classes survived for relatively short periods of time (Campbell & Marshall, 1987).

Another way of looking at this same problem would be to take some function of a whole phylum, such as the echinoderms, and see how long it took for all the known structures that serve that function to evolve. Take respiration, for example. This was achieved by four basic means, all of which had been evolved by the Early Ordovician (Campbell & Marshall, 1987). These involve oxygen

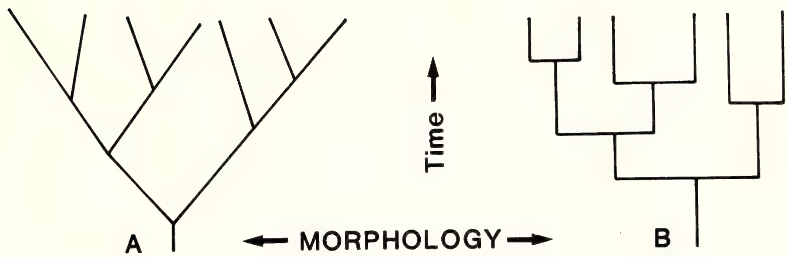

Fig. 3 Diagrammatic representation of the Neo-Darwinian (A) and the Simpsonian (B) models of morphological change involved in the production of taxa at suprageneric levels.

exchange between sea water and the fluids of either the water vascular system or the coelomic cavity. Exchange with the water vascular system involved the use of tube feet with their thin membranous walls and internally circulating fluids. Tube feet evolved very early in echinoderm history, being known in the Helicoplacoidea from the Early Cambrian (Fig. 4A). With coelomic fluid exchange there seems to be only three possibilities (see Fig. 4B-D):

(i) across an unmodified body wall, an arrangement that seems to have been present in at least some of the 'carpoids' of the Early Cambrian;

(ii) across membranes within the coelomic cavity, the sea water being pumped in through slits or pores in the wall, an arrangement found in Early Ordovician rhombiferans;

(iii) across membranes external to the main body wall, the coelomic fluids being pumped out through pores in the wall, an arrangement found in the Late Cambrian diploporitan cystoids.

The examples quoted are only the earliest known representatives of the three modes, and modes (ii) and (iii) seem to have been evolved subsequently several times. However, the point is well made that all the possible major styles of respiration had appeared by about 500 million years ago. The basic plans were established early in evolution, and subsequently only modifications or eliminations of these plans occurred. This suggests a pattern of the Figure 3B type. Much more work of this kind on a variety of organisms is required to provide an adequate data base from which secure conclusions may be drawn.

Second Argument

The second approach is an attempt to find unusual faunas in the early Palaeozoic with a view to filling some of the gaps between classes. This step is a response to the suggestion that if the Neo-Darwinian model is correct, we may find some evidence of intermediate animals among soft bodied faunas that are normally not preserved. An excellent example is the Middle Cambrian Burgess Shale fauna which has been studied in great detail by Whittington and his students at Cambridge (Conway Morris & Whittington, 1985). The organisms were marine and were preserved by clouds of muddy sediment that descended from the adjacent shallow water shelf. Whole biota were engulfed, and were preserved *in toto*. Because of the mode of entombment and the reducing environment within the mud cloud, little decay of the biological soft tissues took place. Thus it has been possible to extract specimens that show details of fine soft supporting tissues such as arthropod appendages and even parts of the alimentary tracts. Reconstructions of two of these animals are given in Figure 5. As might have been expected, some of the fauna had hard skeletons, and these turned out to be normal trilobites, brachiopods, sponges, etc. However, the part of the fauna without mineralised skeletons produced some unexpected results. Details are shown on Figure 6. Not only are 17% of the genera unassignable to any previously known phylum, a high percentage of those that can be so assigned (e.g. to the Arthropoda) cannot be assigned to a previously known class. What is more, no gaps between known phyla or known classes have been diminished; rather, more problems have been created. These early faunas are much more divergent and show more body plans than anybody would have predicted on the basis of the hypothesis that major taxa should converge morphologically towards their times of origin. Serious work on the implications of such faunas for the understanding of evolutionary theory is just beginning (Gould, 1985).

Third Argument

A third approach is represented by the work done on periodic extinctions, and results from an an examination of the time ranges of organisms in the fossil record (Raup & Sepkoski, 1984). These

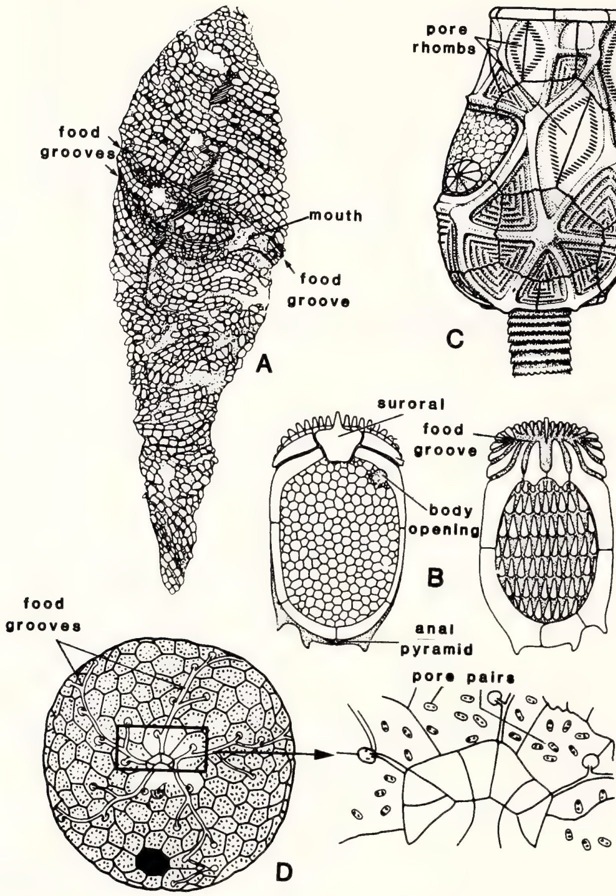

Fig. 4 Examples of different types of echinoderm respiration

(A) *Helicoplacus* from the Early Cambrian; a form with a flexible skeleton made of overlapping plates, and three spiral ambulacral areas with pores for tube feet in the sutures between the ambulacral plates.

(B) A primitive carpoid *Ctenocystis*. This is not an Early Cambrian representative, but one from the Middle Cambrian. There is no evidence of pores for tube feet, and no clear evidence of any means for coelomic respiration. It is possible that the whole skeleton was sufficiently flexible for water to be drawn in through the mouth and oxygen to be exchanged across membranes similar to those in the modern *holothurians* (sea cucumbers). However, it is most probable that the whole surface of the organism was respiratory because most modern echinoderms function in that way.

(C) The Early Ordovician diploporitan *Glyptosphaerites* showing the pairs of pores in the skeletal plates through which coelomic fluids passed into thin membranous tubes, allowing oxygen exchange to take place.

(D) The Middle Ordovician rhombiferan *Homocystites* showing the rhomb-shaped groups of slits that permitted sea water to pass deeply into the coelomic cavity and allow oxygen exchange across thin layers of tissue.

(A is modified from Paul & Smith, 1984;

B from Robison & Sprinkle, 1969; C & D from Kesling, 1967).

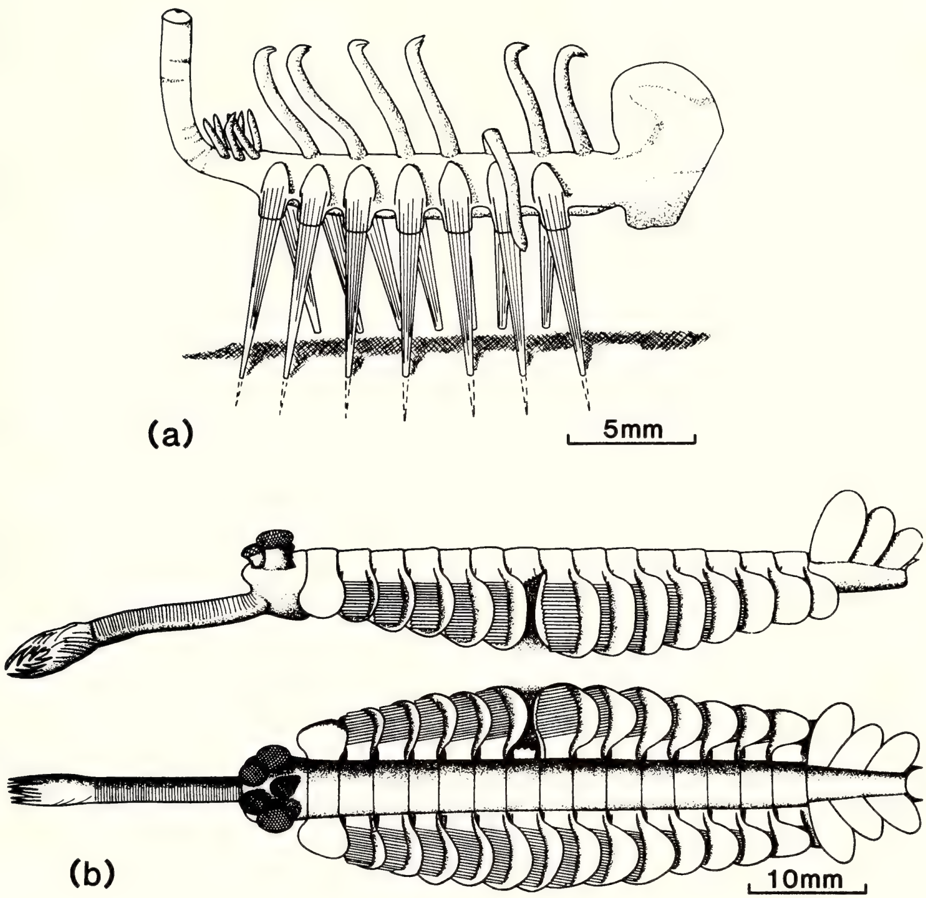

Fig. 5 Two unusual genera from the Burgess Shale, Middle Cambrian, British Colombia. (a) *Hallucigenia sparsa* (Walcott) see Conway Morris (1977). It is difficult to orient this organism. The finger-like extensions on the dorsal surface seem to connect with the alimentary canal. The appendages were apparently stiff and moved as units activated by a number of muscles that have left traces on the fossils. (b) *Opabinia regalis* (Walcott). This is a segmented animal with lateral flaps of soft tissue on each segment. Between the flaps, layers of lamellate tissue, thought to be gills, were regularly arranged. Two pairs of compound eyes and a median eye were developed on the ill-defined head. No jointed appendages were present. Dorsal and lateral views are shown. (Modified from Conway Morris, 1977, and Whittington, 1975).

authors concluded that there have been catastrophic extinctions approximately every 26 million years (Fig. 7). Some of these have been more extensive than others. The cause or causes of these extinctions are irrelevant for present purposes. The important point is that organisms seem to have been wiped out willy-nilly. That is, at many times in the past organisms have disappeared *not by natural selection of the fittest,* but by elimination of the fit as well as the unfit. Obviously this introduces another important factor into the evolutionary process because after such an extinction event evolution has had to make do with what is available in the way of genetic material, and selection pressures would have changed drastically. In other words, the evolutionary clock has been reset periodically through geological time.

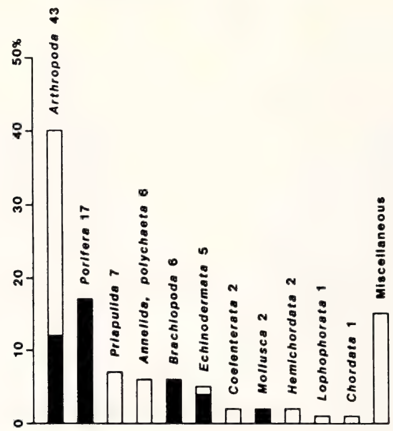

Fig. 6 The taxonomic distribution of organisms from the Burgess Shale. The percentage scale indicates the percentage of the total fauna represented by each phylum; the number at the end of each bar indicates the number of species from each phylum represented in the fauna. (After Conway Morris & Whittington, 1985).

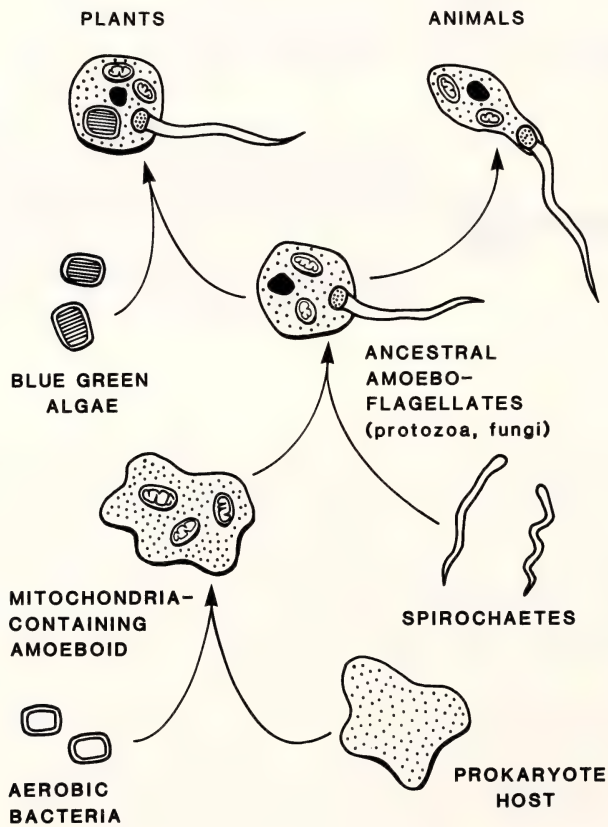

Fig. 7 Diagrammatic representation of the symbiotic origin of eukaryotic cells from prokaryotic cells. (Modified after Margulis, 1970).

A WIDER PERSPECTIVE

At this point we should note that most of the work on which all the above views were based was done on metazoan animals or, in rare instances, on vascular plants. But a great deal of evolution, perhaps the main part, had taken place before such higher organisms appeared. Hence it is worth asking if the broad sweep of the fossil record in association with what is known of recent organisms, rather than an examination of the detail of its later parts, provides any reason for us to conclude that the mechanisms of evolution may have changed through geological time. To do this we would have to establish that the modes of transmission of genetic information, or the nature of biological processes changed *in kind* at one or more times during the earth's history. An investigation along these lines would have to take into account a mass of evidence from the Precambrian, most of which has become available only in the last twenty years (for summaries see Schopf, Hayes and Walter, 1983; Glaessner, 1984). For example, the oldest known fossil organisms were prokaryotes that appeared in the record some 3.5 Ga. ago. The genetic material of living prokaryotes is carried in strands of DNA which are not organised into chromosomes; DNA is synthesised and passed to descendant cells without complex processes such as mitosis or meiosis. Organisation of the genetic material into a nucleus with a membrane and the advent of mitosis must surely have introduced *new factors* into the evolutionary process when the first eukaryotes appeared about 1.5 Ga. ago. Similarly, sexual reproduction, which on palaeontological evidence is unlikely to have occurred before about 1.0 Ga. ago, provided another *kind* of process involving the redistribution of genetic novelty through populations. Not only were genetic factors affected at that time; sexual reproduction is vital to the process of speciation which is central to Neo-Darwinian evolutionary theory. Associated with these biological changes were environmental changes that expanded or contracted the opportunities for organisms, and some of these produced revolutions in the biota. For example, the earliest prokaryotes were probably anaerobic chemoheterotrophs, and a series of stages must have been passed through before aerobic photoautotrophs could survive in the available water on the earth's surface. With the advent of the various levels of metabolism and the necessary environmental changes, new worlds became available to conquer in new ways (Schopf, Hayes & Walter, 1983).

In the light of this discussion we might have expected the processes of evolution to have changed character through geological time. However, at all stages it would have been necessary to have means for introducing heritable novelties, be they biochemical or morphological, and these novelties would have been accepted or rejected by the prevailing environments. In this sense 'mutation' and 'selection' would always have had a vital role in any evolutionary process; on the other hand the *kinds* of mutations, the *kinds* of biological processes and the *kinds* of selection would have changed through time.

These considerations, which depend primarily on studies of prokaryotic structure and molecular genetics, lead us to different types of approach to the study of evolutionary mechanisms. And as so often happens in science some of them were developed by people who were attempting to solve problems outside the immediate field of evolutionary studies. I will mention just three of these as examples.

First Example

First is the suggestion by Lyn Margulis (1970) that the change from cells without a nucleus (prokaryotes) to cells with a nucleus (eukaryotes) probably took place, not by a series of small, imperceptible steps, but rather by a series of symbioses (Fig. 8).

In brief, she suggested that the initial step took place when a very primitive prokaryote, capable of anaerobic metabolism, symbiotically acquired another aerobic prokaryote. Associations of this kind exist today and so there is nothing unusual about this hypothesis. What is unusual is the suggestion that the association became permanent, the anaerobe providing the ground nucleus and cytoplasm within which the aerobes were able to oxidise carbohydrates; this would be the expected function of protomitochondria. The next step is thought to have involved a symbiosis between this new type of amoeba-like structure with a spirochaete-like prokaryote which formed a flagellum. This suggestion is based largely on the observation that spirochaetes and flagellae have longitudinal protein threads arranged in a characteristic 9+2 pattern. This symbiotic structure was ancestral to all subsequent mitotic eukaryotes. The plants gained their photosynthetic capacities by symbiosis with prokaryotes such as blue-green algae which already possessed that capacity.

Although this hypothesis has not gained universal support for all its details, there is sufficient confirmation from the work of other scientists for it to be accepted in its essentials. Here then is an example, albeit at a lowly level of biological organisation, of rapid changes without transitions. Nobody has suggested that this type of mechanism would be valid for organisms at a higher level of organisation, but it is now possible to say that at least one major evolutionary change may have been different *in kind* from what we could have expected according to the synthetic theory. Thus, a possible mechanism to support the proposal has been produced.

Second Example

Second, it has been found that by measuring the genetic distance between living organisms it is possible to determine a sequence of points marking their relative times of divergence. If this pattern of divergences can be calibrated against two of three points on a time scale by reference to the fossil record, it should be possible to calculate the times of divergence of the other groups of organisms in the system. This has been done now for many groups. A good example is the bird study by Sibley and Ahlquist (1985) (Fig. 9) which I use not because it confirms some palaeontological hypothesis, but because it uses Australian material to great effect. The above authors have shown by the method known as DNA/DNA hybridisation that most Australian passerine birds are more closely related to one another than to their European look-alikes. By this means it may be possible to estimate the divergence times of major groups that have living representatives and check these against the divergence time estimated from the

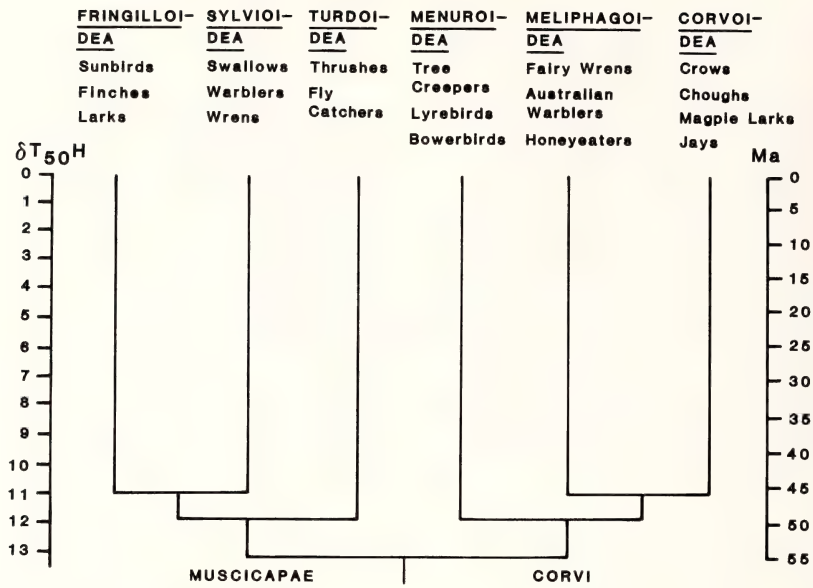

Fig. 8 Representation of the relationships between several superfamilies of the Corvoidea that have apparently differentiated in the Australian region, and the Muscicapae that differentiated possibly in Africa and Europe. On the left is the measured value of Delta $T_{50}H$, the temperature at which 50% of the hybrid DNA of representatives of component genera becomes dissociated. On the right is the geological time scale in millions of years. For details of the method refer to Sibley & Ahlquist (1985) from whose work the above diagram has been modified by removing all the generic and family taxa. The whole group as shown is considered to have separated earlier than 55 million years ago from the Old World groups that include morphologically similar taxa such as honeyeaters, warblers, larks, etc.

fossil record. Of even greater importance is the prospect of determining the divergence times of organisms that have left little in the way of fossil record. In this way it will be possible to augment currently available information on phylogenetic patterns.

Third Example

A third advance has come from a study of the great variety of new ways of introducing new genetic features into an organism. This will be discussed to some extent by the later contributors to the symposium, but I must mention one example - that of multigene families. These are sets of identical, or almost identical, homologous genes that vary in number from two to thousands. They have many properties that single genes lack. They can vary without affecting the phenotype; genetic variation may accumulate without the influence of external selection, and this variation may subsequently be expressed phenotypically; elements of a family may combine to produce new hybrid genes; they can code not only for one protein, but a collection of closely related proteins, that may have different properties. It is now apparent that with increasing complexity, the number of possible novelties increases dramatically. As John Campbell (1983) says "We are discovering that the complex phenotype is the creation of very different sorts of determinants. These complex, individualistic, active, profane, internally organised, and self governed genes are 'smart' machines in the current vernacular sense. Smart genes suggest smart cells and smart evolution." Direction and rate of evolution may be more under internal control than the Neo-Darwinians thought.

CONCLUSION

New advances in all these areas suggest that evolution is likely to have been episodic rather than uniform, and that while the synthetic theory may remain unchallenged as an explanation of part of the evolutionary story (microevolution), it is unlikely to be the explanation of the whole. The genetic determinant must have been changing through geological time, and occasional events, such as symbioses, may have played vital roles in addition to mutation. External selection may not have been the sole determinant of direction and rate of evolution - internal direction may be an important control on both at some levels. And finally, selection of the fittest may have been based on remnants left after episodes of major extinction that took little account of the fittest. The modes of evolution have not been constant

over time but themselves have been changing. The mechanisms of evolution also have changed at all levels of the evolutionary scale, from the earliest phases when emphasis must have been on the biochemical organisation of genetic material to the phases of metazoan evolution when emphasis must have been on modification of established genetic patterns and on phenotypes. Evolution, in this sense, has been evolving.

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K.S.W. Campbell,
Department of Geology,
Australian National University,
G.P.O. Box 4,
CANBERRA, A.C.T. 2601, AUSTRALIA

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Selection, Adaption and Evolution

R. H. Crozier

Nothing in biology makes any sense except in the light of evolution. (Dobzhansky, 1973)

In a way, evolution proceeds like a tinkerer who, during millions of years, has slowly modified his products, retouching, cutting, lengthening, using all opportunities to transform and create. Jacob (1983)

ON THE SHOULDERS OF A GIANT

As Dobzhansky (1973) so rightly stressed, the realization of evolution forms the very core of modern biology. We are used to understanding how good "design" of organisms comes about through evolution, but sometimes forget that design flaws are also understandable as arising the same way. As Jacob stresses, the processes of evolution work with what has gone before rather than producing new organisms afresh when faced with new niches or other evolutionary opportunities. Dawkins (1982) notes two examples of poor design whose origins are readily understandable in evolutionary terms: the contorted faces of flatfish, and the long detour taken by the recurrent laryngeal nerve in a giraffe's neck. It is no wonder, even leaving aside the evidence from natural and experimental populations, that scientists abandoned special creation of any kind as a general explanation for the living world.

As Stent (1972) notes, progress in science, as in art, has a certain inevitable aspect to it: an advance not made by person A today will be made by person B tomorrow. But occasional individuals make their advances with such thoroughness that we can be certain that, without them, we would have to have waited much longer to reach where we are today. Such a person was Charles Darwin, who not only generated an explanatory theory for evolution that is quite modern, but amassed such a treasure trove of data that the survival of the new paradigm could not be long in doubt. In Simpson's (1964) phrase, modern biology is truly the "world into which Darwin led us".

Darwin's vision and breadth of knowledge were awesome, as anyone reading through his works can testify. Not only did he establish the main outlines of evolution, but he clearly foreshadowed such modern topics as variation in evolutionary rates (as discussed by Penny, 1983), and even the whole field of sociobiology (not only and obviously in "The descent of man" but also in his discussions of social insects in "The origin of species").

Darwin's vision *was* far-reaching, but today, standing on his shoulders (to paraphrase Newton), we see much farther than he could possibly do (a point sometimes missed by philosophers of science). To acknowledge that our vision is different, even for concepts such as natural selection, clearly does nothing to reduce our appreciation of Darwin: stasis in evolutionary understanding would be a failure, and Darwin did not fail.

GENETICS, CHANCE, AND NECESSITY

The greatest single cause of advance in understanding evolutionary mechanisms since Darwin has been the rise of genetics. Evolution is clearly understood now as genetic change in a lineage, and evolutionists generally engage in studying the causes and consequences of such change. Both cause and consequence can, of course, be complex, as I will try to make clear below.

Is there a role for chance in this view of life's history? Yes, and not just one role. One role is that of constant origin through mutation of new alleles differing so trivially in effect from others that their fixation is due to chance. Such a process is envisaged by the Neutralist school of molecular evolutionists as the main one giving rise to molecular change (Kimura, 1983), whereas other evolutionists suggest that the neutralist domain is relatively minor.

The other role for chance concerns the ultimate effects of early choices: whether a population takes one fork in the evolutionary road instead of another may be a matter of chance, yet the two roads may lead

to quite different results. Such early choices have a considerable effect, not only on the adaptations that are later available to the population, but also on the *way* in which different organisms adapt. Monocotyledons and dicotyledons have both evolved tree growth forms on occasion, but in different ways: palms are of necessity constructed very differently from true trees because they are monocotyledons, and monocotyledons lack the ability for secondary strengthening expressed by dicotyledons (Maynard Smith et al., 1985).

Monocotyledons have to some extent overcome the basic constraints of their architecture by evolving tree forms in their own way, but Gould (e.g., 1980) has frequently stressed that such constraints will often be insuperable, limiting the possible array of forms. The concept of constraint is a useful one, in that it must sometimes be correct (Bull and Charnov, 1985) and in that it can lead to tests and be falsified on occasion. For example, LaBarbera (1983) showed that, contrary to general belief, the scarcity of wheels among higher organisms is not because they can't evolve them, but because they are less efficient than the observed forms of transport (moreover, where they *are* more efficient, they have in fact evolved). Apart from neutralist explanations for morphology and life-pattern, there are therefore three basic kinds of explanation of why certain features are seen and others are not (Crozier and Page, 1985): *structuralist* (absent features are either impossible or the way to them is blocked by impossible intermediates), *historicist* (there has not been sufficient time for the missing features to have been evolved, or only some of the adaptive peaks are occupied so far), and, of course, *adaptationist* (missing features confer less fitness than the ones that are present). Structuralist and adaptationist examples overlap, because an impossible phenotype is certainly also lethal, and nearby phenotypes, even if not impossible, are almost certainly of low fitness. For example, a clam shell which cannot open would be a lethal combination, and ones that can only open a tiny fraction would be of low fitness.

Of necessity, I will not spend further time here discussing chance, but rather concentrate on the process of adaptation, the most interesting aspect of evolution to most people.

ADAPTATION AND RAPID EVOLUTION

Adaptation is the evolution of characteristics better suiting an organism to its environment. Or perhaps, in view of considerations to be dealt with further below, adaptation is best describable more vaguely as evolution better suiting whatever is evolving to its environment. [*Of course,* we can also speak of "an adaptation", as a characteristic that strongly favors the survival of an organism in its environment, and even as "adaptation" as being the state reached by adaptive evolution, but I will try to stick to just the one meaning in this essay.]

Clearly, adaptation is close to just being evolution driven by natural selection, which in the longer term is the replacement of one favorable mutation by another even more favorable (in Monod's (1972) terms, the action of necessity on the products of chance). The distinction is worth retaining because evolution can lead to lower fitness for a lineage or population (Blick, 1977; Crozier 1979; Paquin and Adams, 1983); I won't discuss these cases here either, save to point out that Darwin, lacking knowledge of modern genetics, did not realise that selection could lead to lower fitness. Here especially we see further than Darwin, standing on his shoulders.

Darwin had, of necessity, to infer that adaptation occurs by documenting the end results and by considering the Malthusian pressures certain to bring it about. For humans to observe adaptation in progress, it has to be rapid; since Darwin's time both strong selection and rapid adaptive evolution have been observed many times. It is worthwhile looking at some of these instances.

Most of the cases of rapid evolution that we know of have been driven by human activities. This happenstance is understandable from standard evolutionary theory, because a changing environment is the simplest cause of strong selection, and we are the major perturbing force on the planet today. Some of these cases involve introductions, such as of the house sparrow, *Passer domesticus,* into North America.

Winter stresses, particularly due to storms, form a major component of selection acting on North American sparrow populations (Bumpus, 1899; Fleischer and Johnston, 1982; Lande and Arnold, 1983). These winter stresses lead to both stabilizing and directional selection on the birds, and fall differently on males and females (Fleischer and Johnston, 1982).

From their reanalysis of Bumpus's (1899) data, Lande and Arnold (1983) estimated that, in females, departure by one standard deviation from the mean in either direction for the first principal component led to a decrease in relative fitness by 45%, and directional selection showed similar but

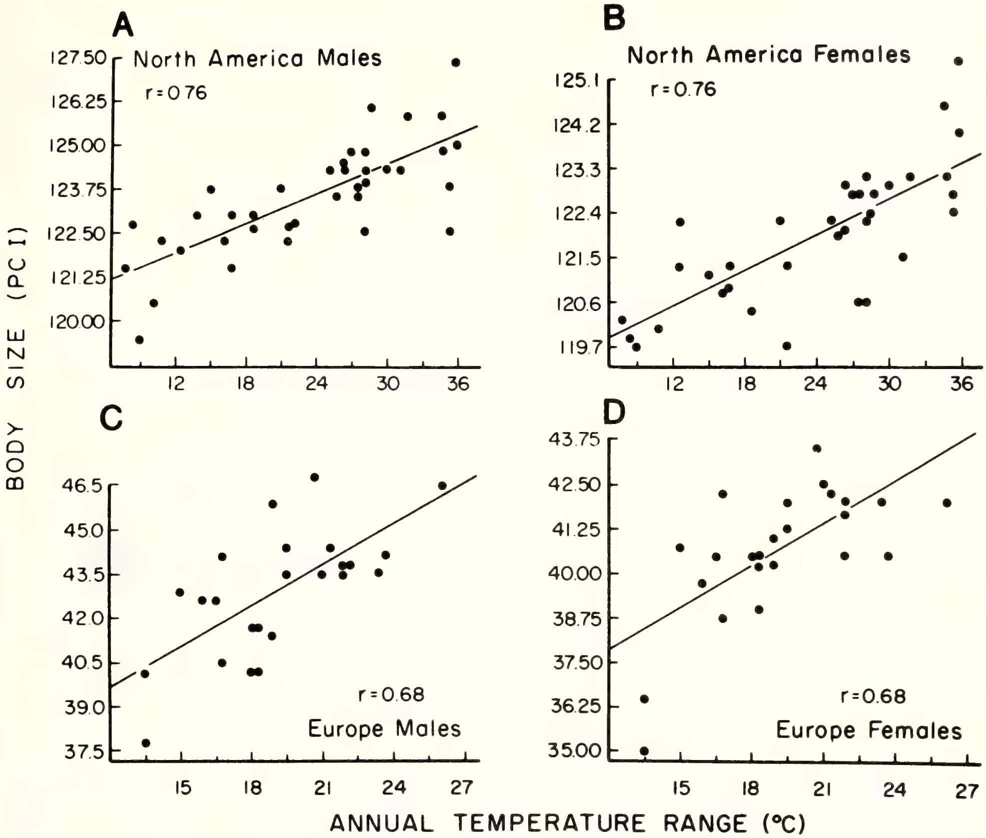

Fig. 1. Body size of house sparrows (divided according to origin and sex) versus annual temperature range. Size was measured by the first principal component derived from 14 skeletal measurements. Each point plotted represents the mean for a locality at which at least ten individuals were measured. Taken from Murphy (1985), reprinted by permission.

lesser strength for several morphological measures (Lande and Arnold, 1983; Fleischer and Johnston, 1982). We would therefore not be surprised to find that significant changes had emerged in house sparrow populations since their introduction, and indeed such changes have occurred. Not only have the various North American populations differentiated markedly since introduction (Johnston and Selander, 1971), but also similar (but not yet identical) trends in variation with climate have emerged to those seen in the ancestral European populations (Murphy, 1985; Figure 1).

Although this does not appear to be so, the house sparrow case should be as famous as the text-book case of rapid evolution, that of the large-scale replacement of the normal white-peppered phenotype of the moth *Biston betularia* by a dominant allele for essentially black moths. The *Biston* case provides a further example of human-induced changes, although this time through the modification of the environment: the killing by air pollution of encrusting lichens yielded black rather than light-colored tree-trunks, resulting in a shift in bird predation pressure in favor of the previously-unknown allele.

Many other cases are known of both strong selection and the response to it. Of course, the peppered moth and house sparrow cases are almost exclusively scientific in their appeal, but natural selection can be of practical importance as in the many cases of the evolution of resistance by insects to insecticides (Figure 2) or by bacteria to drugs. Resistance to biocides usually arises with dismaying speed, as perusal of the figure will show. The speed is both understandable (we *are* applying extremely strong selection pressures) and serious: we are in an evolutionary race between our ability to produce new compounds and that of the pest species to produce

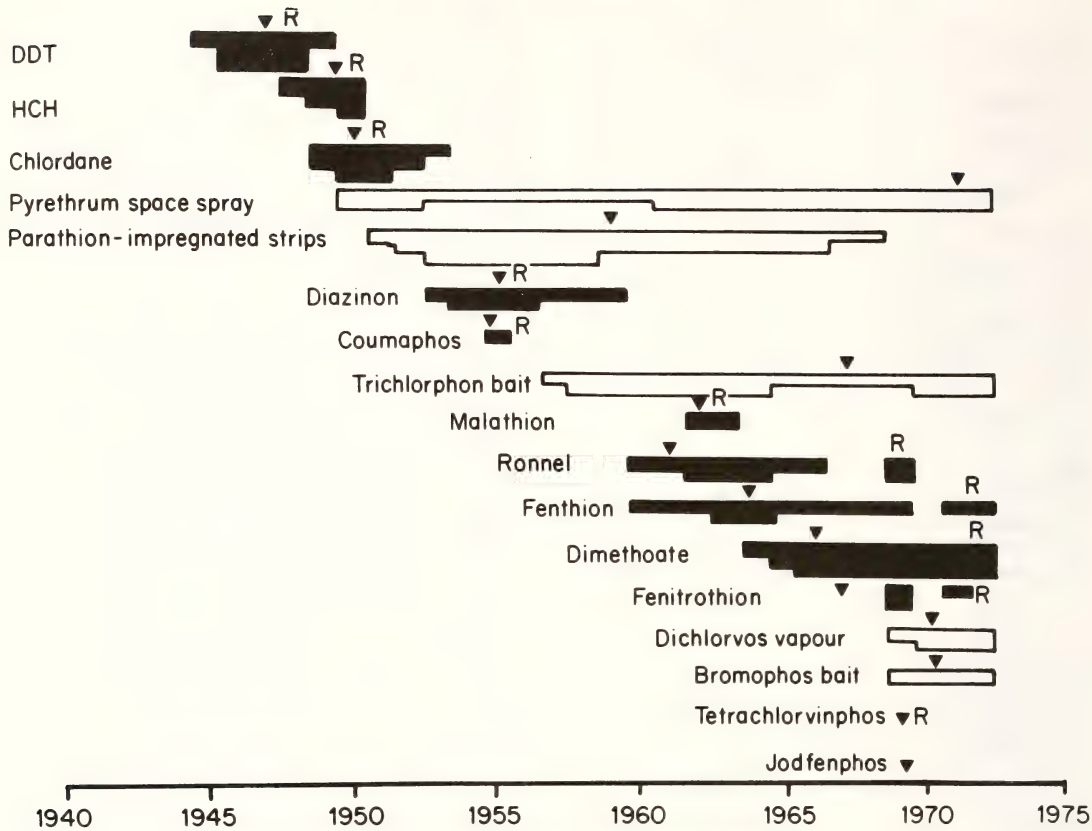

Fig. 2. Evolution of resistance to insecticides by houseflies on Danish farms. The width of each bar reflects the extent of use, the inverted triangle symbol the date of the first confirmed case of resistance of a given insecticide, and the letter R the date when most populations were resistant. Open bars refer to residue sprays and solid ones to other methods of application. From Wood and Bishop (1981), reprinted by permission.

counteracting mutations in its degradative enzymes. I'll allow a somber thought to intrude here: as we become an ever-more important part of the planet's total biomass, we thereby increase the selection payoff for pests to attack *us*, as well as our crops.

PRIMING THE PUMP: FAVORABLE MUTATIONS

Now, as I mentioned above, evolution can only continue for so long by the replacement of one existing allele by another. If the response to selection involves only the shuffling of pre-existing genetic variation, without new alleles arising by mutation and being selected for, then evolution will not continue long. But this is not the case: the occurrence of favorable mutations has been repeatedly demonstrated. This demonstration is a classroom exercise for prokaryotes, because one can start with a single cell, known to lack a favorable trait (such as resistance to a given drug), and then demonstrate the occurrence of the trait in the progeny. Because prokaryotes have only one copy of each gene, the experimenter can be certain that the genes for resistance were not hidden in the initial bacterial cell. Long-running experiments of this kind can further produce significant improvements in the metabolic prowess of bacteria (Mortlake, 1983); these changes mimic the impressive powers of natural populations to evolve the abilities to use new carbon sources, of which a prime example is a wholly new enzyme attacking nylon (Ohno, 1984)!

The detection of favorable mutations in eukaryotes is a little harder, because the habitual diploidy of many eukaryotes means that it is much harder to be certain that genes selected for were not present originally as concealed recessives, rather than newly-arisen by mutation. Yet, here too, the

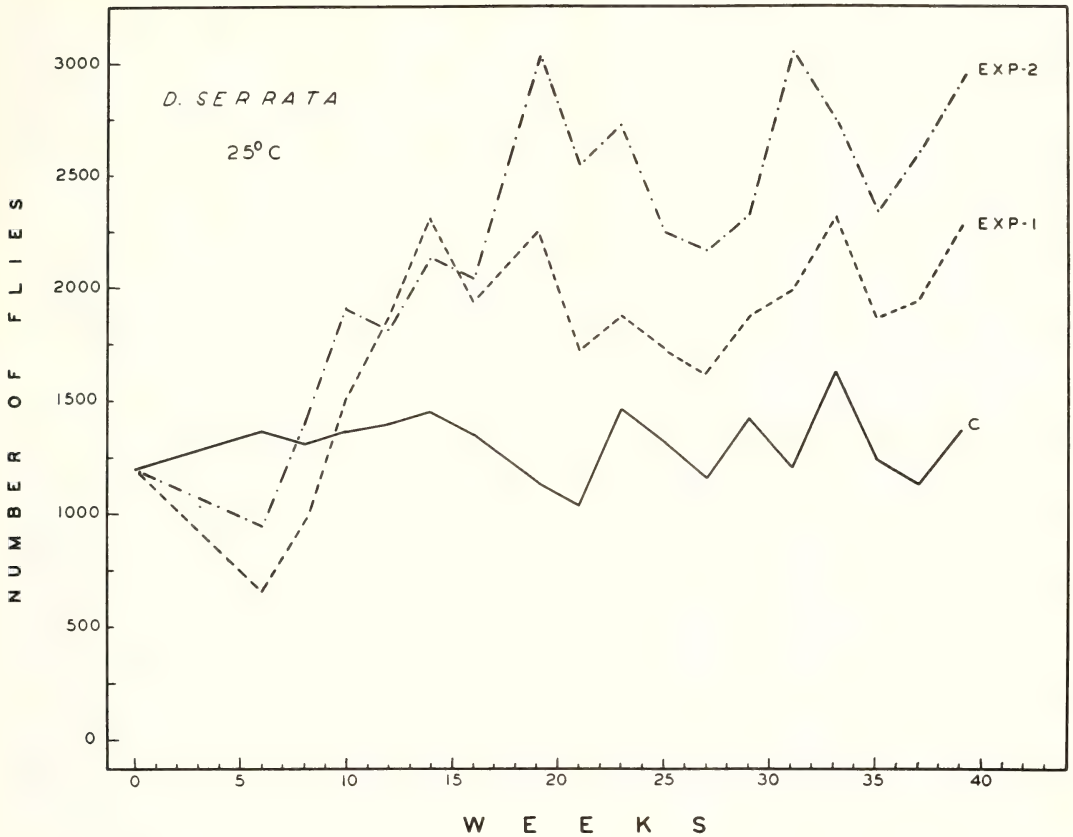

Fig. 3. Effects of radiation on inbred *Drosophila serrata* populations. An inbred population was split into three parts, two of which were irradiated. The resulting changes in population size are readily interpreted as indicating the induction of favorable mutations in the irradiated populations. Other experiments gave similar results. From Ayala (1966), reprinted by permission.

demonstration of favorable mutations has become both commonplace and economically valuable, through the induction of agriculturally-desirable mutations with radiation. Such induction is most easily done nowadays using large cultures of single cells from which whole plants can be grown, but has a long history. For animal populations, the study by Ayala (1966) is worth noting. Ayala inbred laboratory populations of the flies *Drosophila errata* and *D. birchii,* thus reducing both the level of variation and the average fitness of the population. Radiation led to the irradiated populations achieving larger sizes than non-irradiated controls (Figure 3), thus demonstrating that there are favorable mutations in amongst the harmful ones.

SINGLE LOCI DO MATTER

But is it worthwhile looking at the effects of single loci? It is a truism in genetics that the overall makeup of an organism results from a complex interaction between its genes and between them and the environment in which it develops. In fact, what evolves is not a collection of hair, skin, eyes (or cilia and flagella, or leaves and roots), but rather a developmental system that produces such characteristics under appropriate conditions. Given such complexity, will changes of one allele for another at one or just a few loci make any difference?

Yes. We have evidence from two sources that single gene changes are important in evolution.

Firstly, there are not as many functional genes as was once thought. The view of only two decades ago that organisms are determined by millions of genes appears quaint now, but it is easily understood. After all, there are about 2.7×10^9 (yes, only a little under three *billion)* nucleotide pairs in mammals

such as the house mouse (Sang (1984) gives a table of genome sizes in various animals). An average protein is about 400 amino acids long, so that it could be encoded by 1,200 nucleotides, meaning that there is enough DNA in mammals for 2,250,000 genes.

But we now know that most eukaryote genes are made up to a large extent of *introns,* sequences within them that are cut out of the primary RNA transcripts before they leave the nucleus and which therefore do not code for amino acids. For example, the protein ovalbumin from chickens *could* be coded for by just 1,879 nucleotides (the length of the final messenger RNA), but the gene itself is stuffed with introns and is about 7,600 nucleotides long (Sang, 1984)! Furthermore, much of the genome is made up of spacer sequences between genes and various other noncoding sequences, so that the dramatic differences in DNA content sometimes found between relatively closely related organisms (e.g., the pea *Pisum sativum* has more than nine times as much DNA as the mung bean *Vigna radiata:* Sang, 1984) reflect differences primarily in the non-coding and not in the coding sequences.

How many genes are there then coding for the complexity of organisms? And how can we find out? For the tightly-organised genomes of prokaryotes, lacking the introns and general profligacy of non-coding DNA of higher organisms, dividing the amount of DNA by the average size of a gene is enough (Watson, 1977). To estimate the relatively rare coding sections in the comparative vastness of a eukaryote genome requires indirect methods, such as estimating the number of loci at which lethal mutations can occur (Raff and Kaufman, 1983) or by estimating the number of different messenger RNA molecules produced (Raff and Kaufman, 1983), and using this figure as an estimate of the number of active genes. By such means we can arrive at the following highly approximate numbers of genes in three organisms whose disparities are only matched by the affection that geneticists feel for them:

ORGANISM	NUMBER OF GENES
Escherichia coli (bacterium)	3,000
Drosophila melanogaster (fly)	8,000
Homo sapiens (us)	30,000

It would be a mistake to think that these figures mean that humans are only ten times as complex as bacteria! The complexity of a developmental system depends on the *interactions* between genes, and is therefore dependent on some *power* of the number of loci, and is not a simple arithmetic function of the number. As an analogy, consider the complexity of a telephone exchange: the number of possible dyadic connections increases with the number of telephones, *n,* according to:

$$\Sigma^{n} (i - 1)$$

where i = 1 through n

so that, for three telephones there are three connections, but for six there are fifteen. The complexity of the system grows much more rapidly than the number of subscribers.

More direct evidence that single genes do matter comes from studies on interspecific differences. One rather spectacular example concerns the closely related flies *Drosophila heteroneura* and *D. silvestris,* whose males have dramatically different head shapes (Figure 4). These two species are extremely similar genetically, and can be induced to mate under laboratory conditions, when they yield viable and fertile hybrids (Val, 1977; Templeton, 1977). Analysis of the head shape in the hybrids and backcrosses shows that the differences between the two species in male head shape are determined by only about six segregating units (Lande, 1981). Because these segregating units may contain two or more tightly linked genes, the number of gene loci is probably around about 12.

The case of these Hawaiian Drosophila, and others such as that of the peppered moth, show that major evolutionary changes may involve substitutions of one allele for another at as few as half a dozen loci, with further evolution involving "fine tuning" at other loci to ameliorate side effects of the major changes. There is, for example, some evidence that the heterozygotes for the allele for black coloration in *Biston betularia* have come more nearly to resemble the homozygotes for this gene today than was the case last century (Wallace, 1981).

WHEELS WITHIN WHEELS: SELECTION AT MANY LEVELS

Selection is the only evolutionary force resulting in adaptation, because only selection discriminates between the mutations that constantly

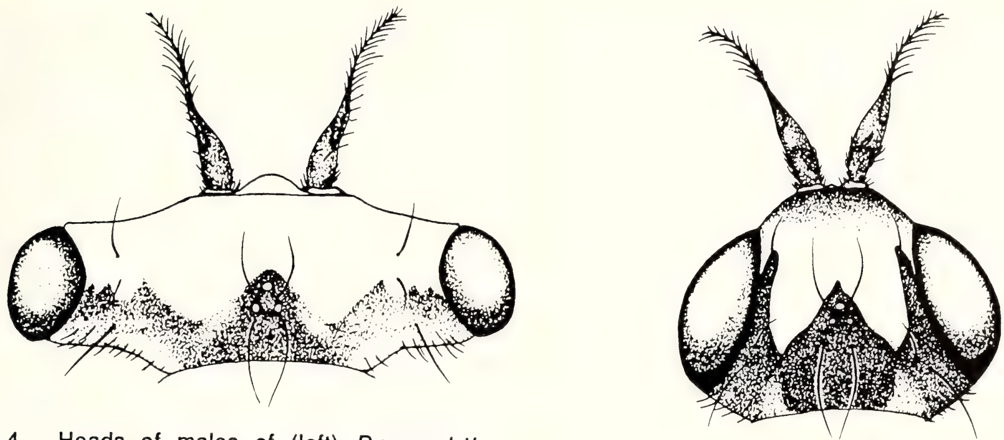

Fig. 4. Heads of males of (left) *Drosophila heteroneura* and (right) *D. silvestris*. From Kaneshiro and Val (1977), reprinted by permission.

rush like a gale through the genome. But selection can take place at many levels, and adaptation at one level can take place at the expense of adaptation at another.

While the levels at which selection can occur are many (Table 1), we can certainly fix our ideas by noting that the levels of the individual, above the individual, and within the individual satisfactorily include them all!

It is worthwhile pursuing the levels-of-selection approach a little further. Selection may take place at different levels in two ways: by acting at different levels of complexity in the physical hierarchy of life (as displayed in Table 1), or by acting at both the genetic and behavioral levels (of which more later). With regard to the hierarchical approach, we can turn to Hull (1980), who built on ideas enunciated by Dawkins (1976), and distinguish between *replicators* and *interactors*. To paraphrase Hull:

A *replicator* is an entity that passes on its structure directly in replication.

An *interactor* is an entity that interacts directly with its environment.

An interactor is made up of one or more replicators, usually very many. Selection acts by affecting the relative success of different interactors in passing on the replicators of which they are composed. The properties of interactors are of course formed by more than their replicator makeup, with environment and history playing their parts too. Selection is thus effective in causing evolution only to the extent that fitness differences between interactors mirror differences in their replicator makeup.

Levels of selection are thus determined by the level of interactor. But how can this level be recognised? It is not enough to look for average fitness differences between interactors because, for example, two populations may differ in average fitness only because one is made up of fitter individuals than the other.

I suggest that the best criterion for determining the level at which selection is acting in any one case is to discover which is the lowest level allowing a complete formal description of the situation. Thus, if population 1 is made up only of AA individuals, which are of higher fitness than the BB individuals of which population 2 is exclusively composed, then fitness differences between genotypes (level 2(b) in Table 1) is the appropriate level, and higher levels such as 3(a) or 3(b) are not usefully invoked.

Interactors above the gene level are usually not replicators as well, because their reproduction involves their dissolution into their constituent parts: when you reproduce, you do not pass on your genetic endowment intact but rather shuffled subsets of your genes packaged as gametes.

Higher taxonomic categories such as genera and families cannot therefore be units of selection, because they are not interactors. It is also problematic whether they "evolve" as some authors consider they do (e.g., Arnold and Fristrup, 1982), because they lack objective reality, being simply assemblages of species set up to facilitate cataloging. But it *would* be justifiable to consider a lineage evolving, because that can be objectively defined, in

TABLE 1
LEVELS OF SELECTION

Levels of selection during genetic evolution, determined on the basis of which entities are *interactors* (see text). With the possible exception of ecosystems, entities higher than the species are not interactors. Genera, for example, may evolve through selection within or between their constituent species, but are not themselves interactors. Other levels of selection may occur without affecting genetic evolution: for example, competition between cell types within a metazoan or metaphyte is unlikely to lead to an increase in the frequency of the winners' genes next generation.

1. Direct selection on DNA sequences.
   (a) Selection for the number of copies
      i Sequences capable of infective transmission (transposons etc.)
      ii Sequences incapable of infective transmission (highly repeated sequences e.g. *Alu*).
   (b) Selection between alleles (meiotic drive)

2. Selection on genotypes
   (a) Selection on haploid phases (bacteria, gametes of higher eukaryotes)
   (b) Selection on diploid phases
      i Cells within organisms (likely to be only rarely converted to evolutionary change)
      ii "Normal" complete higher organisms.
3. Supra-individual selection within species.
   (a) Intra-demic group selection
      i Groups of non-relatives
      ii Groups of relatives (= kin-selection)
   (b) Inter-demic group selection (this is the classical meaning of group selection)
4. Selection at the species level.
   (a) Non-random speciation (bias in characteristics of new species).
   (b) Non-random distribution of speciation rates (species with more optimal values of some characteristic speciate at a higher rate than others)
   (c) Non-random distribution of extinction probabilities (species with more optimal values are less likely to become extinct).
5. Selection at the ecosystem level (Wilson, 1976). Assemblages of species differ in their persistence, thus favoring "good mixers" among their constituent species

principle, and genera and families that are defined strictly on lineage lines could be said to evolve. Such lineage-defined taxa would then be roughly analogous to populations, which evolve even where selection is strictly at the level of the individuals within them.

There are exceptions to the rule that only genes are replicators. Individuals reproducing asexually and without meiosis do replicate their genetic makeup exactly, and lichens, which are simple communities made up of particular combinations of fungal and algal species, reproduce by dispersing fragments that form new colonies elsewhere.

SELECTION AND SOCIAL BEHAVIOR: THE EMERGENCE OF NON-GENETIC EVOLUTION

The realisation that selection can take place at many levels helps a great deal in understanding the evolution of cooperation. While much cooperation involves reciprocity, what has been called "social compensation" ("you scratch my back and I'll scratch yours"), there remains the large and important category of *altruistic* behavior. Altruistic behavior is defined biologically as behavior in which one individual reduces its reproductive capacity in favor of that of another (this differs from the everyday definition of altruism, in which *intention* and not *effect* is all that matters (Stent, 1978), but then we cannot really be sure of the intentions of other humans, let alone of non-humans). Social insects provide an extreme example of altruism: in many species, the workers are sterile and labor solely to aid their mother in producing some reproductive individuals to perpetuate the colony (E.O. Wilson, 1971).

The problem of altruism, as was seen by Darwin, is that it is hard to understand at first glance how reproductive restraint and, especially, sterility can be selected for! Darwin also glimpsed the process which was later explored quite fully by Bill Hamilton (e.g. 1963, 1964, 1972) and named *kin-selection* by John Maynard Smith (1964). Quite simply, kin-selection is group selection in which the groups are made up of relatives (Wade, 1980a). Reproductive self-sacrifice *can* be selected for if the result is to increase the overall frequency of the genes responsible for the behavior through their increased replication via relatives of the altruist. This approach provides a framework not only for understanding the evolution of groups such as the social insects (e.g. Crozier, 1982), but also for framing laboratory tests (Wade, 1980b).

No treatment of the evolution of social behavior would be complete without at least a brief mention of Maynard Smith's Evolutionary Stable Strategy

approach (Maynard Smith, 1982). Briefly, an ESS is a phenotype that, if present alone in a population, successfully excludes any other. The inclusion of the word "strategy" belies the generality of this approach, which of course is applicable to more than just behavioral differences. Furthermore, the ESS approach is a largely successful attempt to model evolution quantitatively without needing the precision in specifying parameters that bedevils efforts to apply population genetics to long-term evolutionary change.

As an example of the ESS approach we can take the explanation afforded by it to one of the puzzles of animal contests: owners of resources often fight fiercely for the resource whereas challengers are timid. This asymmetry can be shown in many cases to be uncorrelated with the fighting ability of the contestants. Maynard Smith pointed out that a strategy following the rule "If owner fight hard, if challenger retreat quickly" would replace either of the unconditional approaches of fighting hard every time ("hawk") or of always giving up easily ("dove"). This replacement is expected because the followers of this *bourgeois* strategy would never fight each other hard, whereas the "hawks" would fight each other hard each time they met, and the "doves" would easily lose whatever resources they had acquired.

Behavior leads us to consider the emergence of non-genetic evolution. Culture, defined biologically, is the transmission of learned behavior patterns. Evolution now becomes rather complex, because in organisms which are social, and these include most higher animals (Wilson, 1975; Wittenberger, 1981; Trivers, 1985), there is the passing on of *genes* (of course), and, potentially, of *environment* (a result of parental activities) and *learned behaviors* as well (Cavalli-Sforza and Feldman, 1981). Only genes show obligatory and exclusive vertical transmission; behavior can be passed horizontally (such as between non-relatives), and its transmission thus resembles the typical infective transmission of microorganisms (Cavalli-Sforza and Feldman, 1981; Lumsden and Wilson, 1981; Boyd and Richerson, 1985).

We expect our own species to be the champion at culture, but cultural transmission certainly occurs in other species too. Bonner (1980) considers examples, such as the occurrences of birds learning from others to pierce the caps of milk bottles, of chimpanzees passing on termite-hunting skills, and of the transfer between individual Japanese macaque monkeys of skills in processing new foods (such as paper-wrapped toffees supplied by scientists!).

Clearly, genetic and cultural evolution are both "biological", and we would expect them to interact, and the theory of this interaction is now a healthy field of study. Some authors, such as Lumsden and Wilson (1981) have stressed cases, such as tendencies to avoid incest, in which cultural biases reinforce genetic ones, but the two need not agree. Dawkins (1976) pointed out that cultural patterns may reduce the genetic fitness of carriers, and Boyd and Richerson (1985) provide a strong beginning to understanding this phenomenon quantitatively (celibacy is a possible example -- assuming that by adopting their calling priests and nuns are not somehow preferentially helping their non-celibate relatives!)

Wyles et al. (1983) point out that cultural evolution should tend to increase the rate of genetic evolution, because the acquisition of new behaviors will change the environment and hence the selective milieu of the creatures involved. They use the example of the distribution of the ability to digest lactose as adults: human groups which keep dairy cattle have this ability, and those which don't keep cattle lack it, suggesting that drinking milk as an adult (a cultural trait) selects for adult lactase secretion. In agreement with this suggestion is the quite close concordance observed between relative brain size and morphological evolutionary rate seen in Table 2, on the assumption that larger brains confer a greater ability to support culture. Of course, as stressed by Lumsden and Wilson (1981), it is possible that cultural evolution in our own species could become *so* rapid as to lead to a rate of environmental change that genetic change cannot keep up with!

TABLE 2
BRAIN SIZE AND EVOLUTIONARY RATE

Relative brain size (as a proportion of body weight) compared with anatomical evolutionary rate, as calculated by Wyles et al. (1983)

Taxonomic group	Relative brain size	Evolutionary rate
Homo (humans)	114	> 10
All hominoids	26	2.5
Songbirds	23	1.6
Other mammals	12	0.7
Other birds	4.3	0.7
Lizards	1.2	0.25
Frogs	0.9	0.23
Salamanders	0.8	0.26

Findings such as those of the occurrence of cultural transmission in species other than our own give a new twist to the Darwinian revolution. Darwin showed that humans have a heritage derived from that of other animals, and indeed rooted in the whole living world. This insight was greeted with antagonism by many fearing that it demonstrated that humans have an "animal nature". The recent studies on social behavior in other species complete Darwin's revolution by turning this popular understanding on its head. Rather than evolutionary biology saying that humans share unpleasant characteristics with other creatures, we can now see that other creatures are really quite like us in many of the things that we pride ourselves on!

ECOSYSTEM SELECTION: THE PINNACLE OF GROUP SELECTION?

Although cultural evolution is seen by some social scientists as necessarily involving the highest level of selection (e.g., Plotkin and Odling-Smee, 1981), this standpoint is arguable because more than one species displays cultural evolution and hence any level inclusive of more than one species would therefore seem (to me, anyway) to be "higher". This highest level is that of the community, as argued by D.S. Wilson (1976).

D.S. Wilson's (1976) argument is couched in terms of interaction coefficient matrices, but he also provides a simple thought-experiment that makes the point. Consider two plant species that are identical in how they profit from the rest of the members of their community, but differ in how they affect them. In particular, consider the effect of species A being harmful to earthworms whereas species B is beneficial to them. Species B could therefore be described as a "good mixer" and species A a poor one. Within communities, there are no systematic effects altering the relative abundances of the two species, but one emerges when the persistence times of communities with differing proportions of species A and B are considered: the more species A individuals there are, the worse off are the earthworms, and the shorter is the persistence time of the community. Overall, given enough time, species B should replace species A because communities with high proportions of species B should persist longer than communities with high proportions of species A (and hence be more important sources of species for new communities).

By the criterion I introduced above, of searching for the lowest possible level at which models adequately describe the biology of the situation, Wilson's model is truly one of selection at the ecosystem level, and not one of selection at the level of the species. If there were only one huge ecosystem, then the relative abundances of species A and B would depend only on stochastic effects. The occurrence of many communities with finite persistence times is needed before species B will necessarily replace species A; hence, the community is the level of selection. [Now we need ecologists to provide material, rather than thought, experiments on this process!]

Given the division of the living world into communities, Wilson's (1976) model indicates that species will evolve not only competitive properties but also cooperative ones. There is a place for "good mixers" in the wider world of life, after all.

FROM ECOSYSTEM TO CELL: THE CHAOS WITHIN

I find it somehow pleasingly paradoxical that consideration of the highest level of selection leads naturally to consideration of the lowest: that within the genome.

The idea of selection, and for that matter genetic drift as well (Van Valen, 1983), occurring within the genome is at first odd, schooled as we all have been in the impartial precision of Mendelian meiotic mechanics. But there are sections of the genome that do not obey Mendel's rules. These sections form a large part of the DNA, in some species a larger part than that that of the law-abiding genes we were taught about that obey the principles worked out by the seer from Brno.

George Miklos, in his companion article, will say more about our current picture of the chaos within the cell that is the genome, so that only the briefest outline is needed here to make my points. The non-Mendelian sequences in the genome fall into two types:

1. Transposable genetic elements ("jumping genes"), are of two types:

(a) Insertion sequences (ISs, which possess only the necessary flanking repeats to allow insertion, and the gene for the enzyme, transposase, necessary for copying themselves).

(b) Transposons. (Tns, which are composed of insertion sequences plus additional genes, such as for antibiotic resistance, that may affect the biology of the organism as a whole).

2. Highly-repeated DNA. Various "families" of relatively short sequences of apparently non-coding

DNA which are present thousands or hundreds of thousands of times.

Initially, the traditional view of selection as focussed at the level of the individual channelled thinking so that biologists racked their brains for some *function* for all that non-coding DNA. The first break came from Dawkins (1976), who wrote that "The simplest way to explain the surplus DNA is to suppose that it is a parasite, or at best a harmless but useless passenger..." carried along by the rest of the genome. Although attempts to explain the large amounts of non-coding DNA in terms of such functions as regulating coding DNA (Davidson and Britten, 1979) still appeared after Dawkins's book, it was not long before biologists concerned with genome structure and function seriously considered the view expressed by Dawkins, with particularly influential papers being those of Doolittle and Sapienza (1980), Sapienza and Doolittle (1980), and Orgel and Crick (1980). From these papers, and from those that followed them, emerged the term "selfish DNA", and an acceleration in efforts by population biologists to understand the dynamics of selection at the level of the genome.

The dynamics of intra-genomic selection is a field of study of intense interest, both for experiment and theory. Furthermore, as stressed by Miklos (1982), and by Doolittle (1982), the difference between effect and function is especially important when considering both transposons and highly-repetitious DNA. Transposons, in particular, have numerous effects on the biology of the organisms in which they occur, but it is uncertain as to whether or not these effects are the crucial agents in determining the levels of representation of these elements.

A detailed examination of the large "selfish DNA" literature is not feasible here, but a few remarks are worthwhile in the general context of levels of selection.

The first important fact is that even just an excess of DNA will be deleterious at the level of the individual through effects on such factors as cell cycle times and organ growth rates, although the strength of this factor will differ between organisms and between environments (Grime and Mowforth, 1982). Secondly, the evolutionary dynamics of transposable elements are markedly different from those of highly-repetitious DNA, and should be considered separately. Thirdly, because the dynamics of these two classes of non-Mendelian DNA interact, and highly-repetitious DNA occurs only in eukaryotes, there are likely to be significant differences in the dynamics of transposable elements between prokaryotes and eukaryotes. Fourthly, the diploidy of most eukaryotes will also give rise to different dynamics relative to those of prokaryotes and of those eukaryotes which are usually haploid, because of the impact of diploidy in allowing storage of genetic variation.

Transposable elements generally insert only at specific sites (Finnegan et al., 1982), but these are usually very short. Because they are so short, the number of possible insertion sites is effectively infinite (e.g., for those cases in which the insertion site sequence is three bases long, there would be about 45×10^6 such sites in the human genome). Transposable elements are also excised from the genome with reasonable frequency; it is likely (but not certain) that this removal is caused by the genome's recombination machinery (and is not under the direct control of the element itself).

Transposable elements cause mutations, because they disrupt the functioning of the sequence around their insertion site. Usually the mutation involves the inactivation of one or more genes, but activation of genes is also possible, as is major rearrangement of the genome (Finnegan et al., 1982; Syvanen, 1985).

The similarity of the genome to a community of species can now be made more explicit. Traditional Mendelizing genes are relatively law-abiding members of the community of genes (could we call this the "endobiome"?). Transposable elements form a class of genes which have moved outside the tidy legal framework established by Mendel. Whereas insertion sequences do little but replicate themselves and cause mutations, transposons code additionally for functions potentially beneficial to the whole community of genes. Transposons are thus the equivalent of "good mixer" species at the ecosystem level of organization, despite their usually deleterious mutational effects.

Debate continues as to whether insertion sequences are maintained primarily by selection at the level of the individual (the complete community of genes) or at their own level (as "selfish DNA"), a debate exacerbated by the apparent inability of some participants to see that both kinds of selection will occur. Two further findings are relevant. In at least some cases, the rate of transposition of a transposon can be shown to be inversely proportional to the number of copies already present (Doolittle et al., 1984). In general, a single transposon copy promotes its own transposition while inhibiting that of other copies (Syvanen, 1985). Syvanen (1985), a strong proponent of the importance of selection at the level of the individual or higher, argues that

selection at the species level has promoted mechanisms to hold transposition in check until a new environment favors a new genetic makeup, which releases the transposons to cause the mutations necessary for adaptation. Doolittle et al. (1984) place the level of selection at that of the community of genes: "self-restraint" by a transposon type will prevent the genome silting up with copies and hence promote its greater longevity (i.e., transposons with self-restraint are good mixers). But while both of these levels may be important, there is a third hypothesis, namely that this "self-restraint" is, instead, a competitive mechanism between transposons. This last hypothesis accords with the suspicion (Syvanen, 1985) that, in nature, there is considerable sequence divergence between copies of nominally the same transposable element.

There remains the distinction between effect and function. Chao et al. (1983) found that strains of the bacterium *Escherichia coli* "infected" with either of the transposons Tn5 and Tn10 tend in laboratory culture to outcompete strains lacking these elements. In the Tn10 case, the greater success of this strain occurred because of alteration to a specific site in the genome as a whole, and those cases in which the Tn10 strain lost in competition were also those in which this particular transposition event did not occur. Do these results necessarily indicate that transposable elements primarily are selected for at the level of the organism because of their production of mutations potentially increasing fitness at this level? No. While the competition experiments demonstrate that inter-bacterium competition is one component of the system, we still need to know the relative importance in nature of this selective pressure and those imposed by cross-infection and transposition.

In higher eukaryotes such as ourselves and *Drosophila,* diploidy leads to large stores of genetic variation impossible in prokaryotes, and hence to a much lesser reliance on mutation to provide an immediate response to selection in eukaryotes. Furthermore, many, and in some species most, transpositions will not result in mutations because they will occur within non-coding DNA. Furthermore, the much greater occurrence of genetic recombination through sexual processes in higher eukaryotes will probably, in combination with the two factors listed above, lead to a much greater significance of infection as against interstrain competition as a form of selection acting on transposable elements. This is not to deny that transposons *can* cause mutations in eukaryotes (Spradling and Rubin, 1981; Engels, 1983; Syvanen, 1985), or that such mutations do give rise to an increase in response to selection (Mackay, 1985), only that such effects may be less important than selection within the genome for transposition rate. The theoretical work already done (e.g., Charlesworth and Charlesworth, 1983; Kaplan et al., 1985; Ohta, 1985) needs to be extended, the phylogenetic distributions of transposable elements further elucidated (e.g., Brookfield et al., 1984; Hunt et al., 1984), and, especially, *much* more work done on the occurrence and effects of these elements in natural populations (e.g., Montgomery and Langley, 1983; Mackay, 1985)

Highly-repeated DNA sequences lack the sophistication of transposable elements, being simply the same (or very similar) sequences repeated thousands or hundreds of thousands of times. Insertion sequences may be derived from the breakup of transposons, and transposons from retroviruses (Flavell, 1984), but highly-repeated DNAs must have a different origin. It seems that, in some cases at least, they result from DNA fragments being made from RNA transcripts (a reversal of the normal cell functioning), with these fragments then being more or less randomly inserted into the chromosome again (Ullu and Tschud, 1984; Sharp, 1983; Brown, 1984; Rogers, 1986; -- the process of the reintegration of DNA copies made from RNA, while well-documented, remains even more mysterious than most molecular evolutionary events).

While there is sufficient difference between the various copies of highly-repeated DNA "families" elements to indicate the workings of quite old-fashioned mutation (Miklos, 1982, 1985), various people have been sufficiently impressed with the overall similarity of such DNA sequences within species as against between them to coin the term "concerted evolution" to describe this apparent correction of sequences. Dover in particular (e.g., Dover, 1982; Dover et al., 1982) has suggested that this process may be important in speciation (through reducing compatibility of chromosomes in the meiosis of hybrids), and, although many cases of speciation are known where this factor is absent (such as in several Hawaiian drosophilids), it remains an exciting possibility. More important for our theme is the suggestion (Hickey, 1982; Dover, 1984) that such sequences could have significant fitness effects at the level of the individual.

Much interest has centered on the mechanism of "concerted evolution". One factor is unequal crossing-over, resulting from an uneven alignment before recombination of DNA sequences which have several copies of a sequence. Unequal crossing-over leads to changes in the number of copies and through drift to strings of identical copies (Figure 5). While unequal crossing-over will thus have the effect sought, attention has shifted to gene conversion as

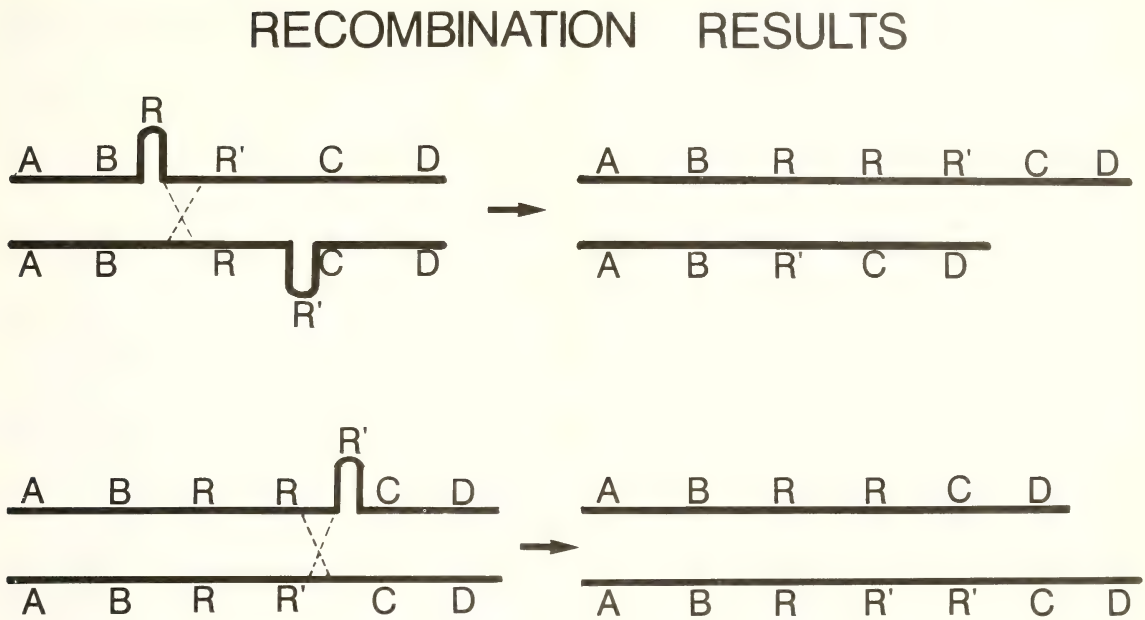

Fig. 5. Unequal crossing-over as a mechanism promoting change in copy number or "correction" of repeated sequences. R and R' are different members of the same highly-repeated DNA sequence family, and repeated rounds of unequal crossing-over, accompanied by genetic drift, lead to the loss of one or the other. Generation of chromosomes with increased numbers of copies may (a) or may not (b) result.

more likely to be a strong enough force to explain the observed levels of similarity (Dover, 1982; Nagylaki and Petes, 1982). Gene conversion is a consequence of normal meiotic processes in which repair mechanisms occasionally lead to a departure from 1:1 ratios in the gametes produced by a heterozygote. For example, a meiosis in a heterozygote A/A' may yield three A and one A' gamete. Selection in this context would occur by the A allele being copied at a higher rate than the A' allele, through, for example, having a stronger affinity for the DNA synthesizing enzyme.

Gene conversion could be a significant evolutionary force only if one allele is systematically favored over the other. A survey by Lamb (1984) shows that such biases in conversion occur often, which then validates the burgeoning literature on the evolutionary importance of the phenomenon (e.g., Ohta and Dover, 1984; Lamb, 1985; Walsh, 1985). Of particular interest is the case where fitness at the level of the individual is affected by the number of copies of A' it has, rather than of A. Ohta and Dover (1984) conclude that, under appropriate relative strengths of selection and conversion bias, selection could be ineffective in preventing the spread of A, even if it is deleterious to individuals, because all members of the population at any one time are very similar with respect to number of A' copies. By contrast, Walsh (1985) concludes that selection at the individual level would usually prevail, if present.

Clearly, the jury is still out in the matter of the evolutionary dynamics of highly-repeated DNA sequences. There are two further considerations. Firstly, efforts to find effects of highly-repeated DNA at the organism level are relatively scanty, but efforts to find these effects by varying the *amounts* of such DNA have failed to do so (Miklos, 1982, 1985). Secondly, gene conversion can only systematically change the total copy number of a family to the extent that non-homologous pairing can occur, and this seems likely to be small. However, because unequal crossing-over lacks any apparent capacity for bias, it would seem a poor candidate for a mechanism to produce very large numbers of copies of particular sequences.

The mechanism for producing many copies of a sequence therefore seems most likely to be reverse copying from RNA. There are two models for how this may occur. In one model, a great many of the repeats observed have the ability not only to be transcribed and then reverse-transcribed but also to

be then reintegrated into the genome. There is evidence that many repeated sequences contain recognition sites for the appropriate enzymes, leading Rogers (1985) to stress this model. The other possibility is that there is, for each family, a parent sequence that has a normal organismic function, which is then copied via processes not yet understood into many sites on the genome. Some genes are "amplified" by the production of extra-chromosomal DNA copies at times during development of high demand for their products (Sang, 1984), and these may be particularly susceptible to this process. Brown (1984) stresses this model for *Alu* sequences because of the discovery that they are stripped-down versions of a functional gene and Rogers (1986) believes that the LINE sequences ("long interspersed elements" several kilobases long) may be copies of an (unknown) parent sequence. It does not seem to have been generally noticed that most of the original basis for proposing "concerted evolution" is removed under the parent-sequence model, because the concert can then be orchestrated by changes in one or at least very few coding sequences (Brown, 1984). Given that DNA is also often excised by mechanisms at present poorly understood, the picture then is one of constant insertion of new sequences from RNA copies of certain sequences whose messages lend themselves to it, and their removal by the enzyme products of what might be called "sheriff" genes (Rothstein and Barash, 1983). As the coding genes evolve, so also changes the spate of reverse-transcribed copies, giving rise to apparent "concerted evolution". In the known cases of this phenomenon, the reverse-transcribed copies are sometimes themselves transcribed but, it appears, never translated. It seems reasonable to suggest that a copy of a coding gene which produced function-coding copies in abundance would thereby unbalance the cell's metabolism, screening out such cases, and perhaps even selecting for alleles of the coding locus which can only give rise to non-translatable reversed transcripts!

It seems worthwhile to note that phylogenetic analysis can be used to estimate the relative importance of the two factors proposed for producing highly-repeated sequences. Sequencing information can be used to produce phylogenetic trees of the sequences in any one genome. Under the parent-copy model, the resulting tree would consist of a single long stem with unbranched shoots, whereas under the autonomous-replication model the tree would be "shrubby" due to the repeated branching of the shoots. There are general statistical methods exist for testing the shape of trees (e.g., Penny and Hendy, 1985; Crozier et al., 1986), and these could then be used to test the relative difference of observed dendrograms from purely "shrub-like" or "tree-like" forms. A simple test of this nature indicates that *Alu* elements probably can transpose: Economou-Pachnis and Tsichlis (1985) found that an apparently new *Alu* copy has higher similarity to other *Alu* copies than to the functional gene believed to be the ultimate ancestor for this gene family.

LINKS: AND TOWARDS A MOLECULAR POPULATION GENETICS

Clearly, not only do we need links between the various levels at which evolution is studied, but this need is being filled through the emergence of a *molecular population genetics.* Or, more strictly, a molecular population genetics concerned with multi-level selection, because, as chronicled by Kimura (1983), a molecular population genetics dealing with the relative strengths of drift and selection has been around since the mid-1960s, concerned with evolution by substitution of one base for another in coding DNA. A prime task of the new molecular population genetics should be finding out the actual molecular changes which occur in response to selection, both in the short and in the long term.

Evolutionary biologists generally fall into one of two types. There are those who uncover what forms of selection are acting on organisms to bring adaptive evolution about. Then there are those who study the genetic machinery in the search for long-term effects of selection. While these two groups have not always taken much notice of each other, the two approaches are not only complementary, but should be combined. The realisation that DNA can be, and almost certainly often is, selfish should heighten the appreciation of the links between the two approaches.

Finally, I ask you to look again at the heads of those two remarkable animals, *Drosophila heteroneura* and *D. silvestris.* What *has* happened at the molecular level to bring about such a massive change? I hope that molecular population geneticists will see it as a major task to map the genetic changes involved in major alterations in development of this kind and then sequence them to see what has happened. When we know what happened to so diversify these two bizarrely-different yet closely related fly species, we will know a lot more about the process of adaptation itself.

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R. H. Crozier
School of Zoology
The University of New South Wales
P. O. Box 1
Kensington, NSW 2033
AUSTRALIA

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Molecular Facts and Evolutionary Theory

George L. Gabor Miklos

Evolutionary biology has had a fascinating recent history. It was realized more than a century ago that the way of approaching many evolutionary problems lay in studies of morphology. However, as pointed out by Bateson in 1922, "*discussions of evolution came to an end primarily because no progress was being made. Morphology having been explored in the minutest corners, we turned elsewhere. We became geneticists in the conviction that there at least must evolutionary wisdom be found.*" At the same time, while it was clear that morphology must have its bases in embryology, it was instead the mathematically oriented theory of neo-Darwinism that rose to prominence over the next half century. This theory is essentially an amalgam of Mendelian genetics and classical Darwinian selection, firmly based on changes in gene frequencies at particular loci. In the late 1960s, it began to be evaluated at a crude molecular level using gel electrophoresis techniques that allowed the examination of polymorphisms at many enzyme coding loci.

In the mid 1970s the technological advances of genetic engineering ushered in an entirely new era of molecular biology. The molecular biologist became the successor to the pure geneticist, and the focus switched back to the molecular analysis of development. The molecular biology of recombinant DNA revolutionized the previous concepts of genome organization and function and led to a reappraisal of the importance of neo-Darwinism. Present day studies of molecular evolution or molecular population genetics are largely the application of recombinant DNA technologies to traditional evolutionary problems, namely the origin, type and extent of genetic variation in populations, or the determination of phylogenies. However, the application of this technology to eukaryotic genomes has spawned so many surprises, that traditional notions of how evolution works have had to be substantially re-evaluated.

The era of molecular evolution. and what will undoubtedly be its *golden era,* began with the cloning technologies of the mid-1970s, when DNA molecules (reviewed by Felsenfeld, 1985), RNA molecules (reviewed by Darnell, 1985) and protein molecules (reviewed by Doolittle, 1985) could be purified and examined by rapid and ultra-sophisticated new techniques. While evolutionary phenomena can be examined at many levels, it was the genome itself which became the most accessible.

In this essay, I shall examine the new findings that have emerged about eukaryotic genomes using the modern technologies, and how these data alter our perceptions of evolution.

THE STRUCTURE OF EUKARYOTIC GENOMES

DNA Amounts

Eukaryotic genomes can have thousands of millions of base pairs of DNA. The variation in genomic DNA between organisms having the same grade of morphological organization can be quite large (Table 1). Thus in annelid worms, there can be a five-fold difference between two species belonging to the same family. In molluscs, there can be at least a ten-fold difference in genome size. In mammals, the variation is less extreme, but is still at least two-fold (Table 2). It is known for example that two closely related, near identical species of barking deer can differ by at least 500 million base pairs of DNA and have their genomes organized radically differently into three and 23 chromosomes respectively.

Since eukaryotic genomic DNA exists as long molecules, millions and even hundreds of millions of base pairs in length, it was a primary requirement of any technology to be able to examine small defined sections of a genome in pure form. The discovery of enzymes which specifically cut DNA, the *restriction endonucleases,* and the availability of bacterial *cloning vehicles,* meant that specific pieces of DNA from any organism could be purified, fused to cloning vectors and clonally amplified (reviewed by Weinberg, 1985). The end product is millions of copies of a pure sequence that is now in sufficient mass to be analyzed by sophisticated molecular biological techniques. This *tour-de-force* of gene cloning is conceptually similar to locating and cloning the proverbial needle in the haystack. So it is with a genome; once a particular sequence of DNA has been cloned, all the tests that are necessary to characterize it can be performed fairly routinely. Thus any part of any genome can be subjected to sequence analysis and the order of the bases determined.

The sequencing technologies have now led us to what many consider as the ultimate goal - the sequencing of the entire human genome (Robertson, 1986). To put this into perspective, it should be remembered that human beings have approximately 3,300,000,000 base-pairs of DNA in their haploid genome, and if sequencing continues world wide at the current rate of 1,000,000 base-pairs per year, this project would take 3,300 years. However, most eukaryotic genomes contain a fair proportion of noncoding DNA, which accounts for more than half of the genome. Thus, if only those portions of the human genome which code for genes are cloned and sequenced, the task is somewhat simplified. Fifty thousand genes, each with a coding length of say 2,000 bases, yield roughly a century of work based

on the present technology. Obviously, it will not be possible to compare the total genomic sequences of all mammals in the foreseeable future, nor is this a sensible avenue along which to proceed. In order to obtain some idea of how present day genomes have altered under the rigors of past evolutionary events, it is sufficient to examine small segments of genomes in order to make comparisons between organisms. The critical decision in an evolutionary context is which parts of a genome are to be compared if, for example, the object is to determine a phylogeny. Some parts of eukaryotic genomes consist of junk DNA and these can change extremely rapidly. Other parts, such as conservative gene families, change relatively slowly. It is prudent therefore to know how eukaryotic genomes are partitioned in terms of sense and nonsense DNA before we rush off and spend time sequencing and characterizing every piece of putatively interesting DNA.

Highly Repetitive Sequences (Junk DNA)

A major finding concerning most eukaryotic genomes is that a significant proportion of the DNA consists of monotonously repetitive sequences (reviewed in Singer, 1982; Miklos, 1985). In certain crabs, for example, more than 30 percent of the genomic DNA consists of long stretches of DNA made up of alternating AT, yielding a landscape of ATATATATATATATATATAT...which extends for millions of base pairs. Such findings on localized repetitive sequences turn out to be general, although the basic repeating unit itself is variable in length and sequence. The fly, *Drosophila melanogaster* for example, has a haploid genome of 165 million base pairs or five percent of the human genome. When the fly genome is partitioned, it is found that approximately 50 million base pairs exists as families of repetitive sequences based on variants of repeats such as AAGAG, AATAT as well as one long repeat. Since these highly repetitive sequences are not transcribed into RNA, and hence do not code for protein products, nearly a third of the fly genome is silent and is unlikely to have significant evolutionary value (Miklos, 1985). Figure 1 not only illustrates the distribution of localized junk DNA sequences on the chromosomes of the fly (where they are concentrated around the centromeric regions), but puts into perspective the differing genome sizes of three organisms. The *entire* fly genome can be accommodated in a single human X chromosome, and the *entire* human genome of 3300 million base pairs can fit into a single chromosome arm of the Congo eel *Amphiuma*.

Data such as these are not restricted to invertebrates. In certain American rats such as *Dipodomys ordii, more than half* of the genome consists of variants of the sequences AAG, TTAGGG and ACACAGCGGG (Fry and Salser, 1977). These three sequence families, known as *MS,* α and β,respectively make up 1200 million, 1100 million and 600 million base pairs of DNA, so that there are at least 2900 million base pairs of junk DNA over and above the main 2500 million base pairs that already exist in the basic rat genome (Figure 2).

Nomadic DNA

Genomes of higher organisms are also in a state of flux, containing DNA sequences which are mobile and can move around the genome. These are the so-called 'jumping genes' or nomadic elements which can excise from a particular location in the genome and reinsert elsewhere. The physical consequences of such events may, of course, be profound. If a mobile element inserts into a gene, or into its controlling sequences, a mutation can occur so that the gene is no longer capable of normal expression. Such wanderings of mobile elements are now considered by some scientists to be prime movers in the generation of evolutionary novelty, since not only can mobile elements turn genes 'on' and 'off' and modulate their activities, but they can act as 'removalists' and relocate other pieces of the genome to new sites, in some cases placing them under the control of different regulatory circuits. When mobile elements insert into other mobile elements, it is relatively easy to see how clustered scrambled arrays of DNA sequences can arise in a genome.

Mobile elements or their defective relatives can make up a substantial proportion of a genome. In the case of *Drosophila melanogaster,* 20 million base pairs of its 165 million base pairs of genomic DNA consists of 30 to 50 different families of mobile elements which move around the genome (reviewed by Rubin, 1983). By contrast, its sibling species *D. simulans* has only about three million base pairs of its genome invested in such sequences (Dowsett and Young, 1982). Since these two species are almost identical morphologically, it is unlikely that these different mobile element populations are, or have been, major players in morphological diversification in these two species.

In general, mobile elements consist of a stretch of DNA a few kilobases (kb) in length, flanked by repetitive sequences of a few hundred base pairs in length. In *Drosophila,* the mobile element families have names such as *copia, 297, hobo, gypsy, H.M.S. Beagle,* and *roo.* Their nomadic nature is affectionately recalled in the flamboyance of their designations. There are furthermore many similarities between some of the nomadic element families of *Dresophila,* and the integrated proviruses of avian and murine RNA tumor viruses (Varmus, 1983).

Split Genes and Multigene Families

A third major finding about eukaryotic genomes is that the coding regions of the genes themselves are not single continuous stretches of DNA but are generally fragmented into peptide coding and noncoding regions, or *exons* and *introns.* Thus in the case of the *bithorax* gene in *Drosophila,* one particular part of the genic landscape that is copied into RNA is over 70,000 nucleotides in length, but after this RNA has been cut and respliced by the cellular machinery, the resultant messenger RNAs are less than 5,000 nucleotides in length. Most of the original RNA transcript has been discarded (Hogness *et al.,* 1985).

Multigene Families

Eukaryotic genes, however, are not only internally fragmented, but turn out not to be the solitary and pristine entities that are used in neo-Darwinian theory. "Real" genes are often members of multigene families which can make the same or very similar products (Hunkapiller *et al.,* 1983). Examples of such multigene or multisequence families, with family memberships ranging from two to hundreds of thousands of relatives, are the

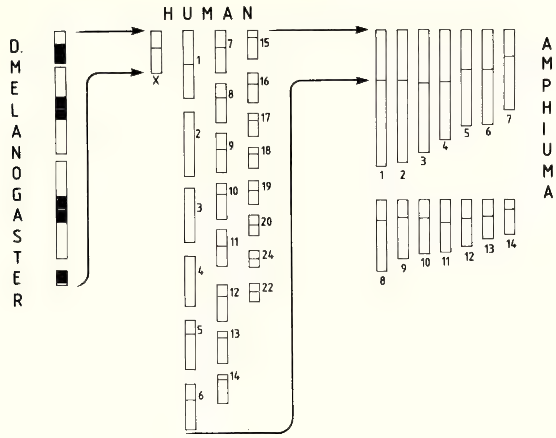


Fig. 1

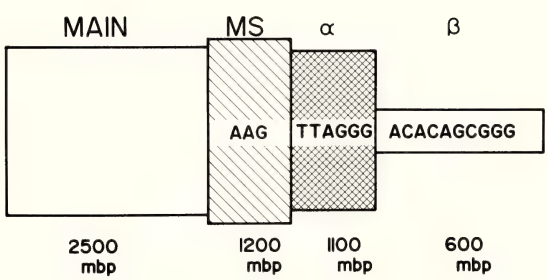


Fig. 2

haemoglobin genes, the antibody genes, the actin genes, the tubulin genes, the collagen genes, the histone genes, the chorion genes, the transfer RNA, the 5S, 18S and 28S ribosomal genes, the highly repetitive sequences and the dispersed repetitive sequences. What of course is immediately obvious is that selection acting on a phenotype, which depends on a multisequence family, can only assess the output of that family as a whole, and not that of its individual members. This in fact is very different to selection acting on the end product of a single locus. The situation has been appreciated by Ohta (1983); *"in view of the widespread occurrence of multigene families in genomes of higher organisms, the evolutionary theory based mainly on change of gene frequency at each locus would appear to need considerable revision."*

Pseudogenes

However, our thinking needs even further revision in the light of molecular discoveries. Having found that eukaryotic genomes are littered with sundry localized highly repetitive sequences, dispersed repetitive sequences ~~and~~ nomadic elements, it was then revealed that the cellular machinery is capable of taking the RNA transcripts of genes and back copying them to DNA molecules, which are then reinserted into the genome (reviewed by Baltimore, 1985). These are the non-functional pseudogenes which now adorn the landscape. An excellent example is the glyceraldehyde-3-phosphate dehydrogenase (GAPDH) gene. In both human beings and mice there is only one functional GAPDH gene in the genome, but there are 10 to 30 pseudogenes in human beings and over 200 in the mouse genome! (Piechaczyk *et al.*, 1984).

"Conversion" Phenomena

DNA molecules can also "convert" each other, so that given a family of sequences, a particular one can physically replace parts of another. This has been beautifully demonstrated in the case of three serine tRNA genes on different chromosomes of the yeast *Schizosaccharomyces pombe* (Amstutz *et al.,* 1985). The evolutionary implications of such cellular events for multigene and multisequence families are again profound. For instance, the homogeneity seen at the sequence level in a multigene family may have nothing whatsoever to do with its functional attributes, but may be indicative of homogenizing mechanisms (Dover and Tautz, 1986).

The Genomic Overview

Our glimpse into the genome reveals an arena where change is the rule, not the exception. Although many biologists have been slow to accept this view, the molecular data are now overwhelming. Looking at the genomic macromolecules from a physical-chemical viewpoint, genomic flux ought to be an inevitable by-product of enzymic action. DNA is not a sacrosanct molecule, it is a substrate in the cellular jungle. It can be replicated, cleaved, repaired, nibbled, rejoined, supertwisted, converted, moved around, modified and generally tinkered with, all of which may have serious consequences for the cell and the organism. Just as we find fossils on this earth, so in retrospect it is natural that the genome is littered with fossil DNA which represents nature's evolutionary experiments. The existence of enzymes which act on DNA make a whole series of events irrevocable.

Ohno (1970) was the first to appreciate the situation when he pointed out not only that there was so much junk DNA in our genome, but that *"genes in the euchromatic region on mammalian chromosomes can be compared to oases in a barren stretch of desert"*...and that...*"For every redundant copy of the pre-existent gene that emerged triumphant as a new gene, hundreds of other copies must have degenerated to join the rank of junk DNA."* (Ohno, 1982).

In terms of evolutionary perspectives, this cursory survey of eukaryotic genomes has left at least one clear message. We would be foolish to go looking for the action in every piece of DNA. It is firstly necessary to sort the non-functional from the functional material. Many generations of biologists were brought up on the notion that everything ought to be functional since it was assumed that natural selection *should* have pruned out all aspects of an organism that did not serve a specific function. What had not been readily perceived is that natural selection cannot act on changes which it does not see. It is an editor, not a composer as has been pointed out by King and Jukes (1969). Consequently we have suffered from the malaise of seeking functional explanations first. The recombinant DNA revolution has helped to partially reverse this thinking as has the concept of selfish DNA propounded by Orgel and Crick (1980) and Doolittle and Sapienza (1980). DNA is an information transmission system and it suffers from biochemical noise at many levels (Tautz *et al.,* 1986). Some of the noise causes meaningful changes, some just causes chaos. How then do we sift the wheat from the chaff and ask about the mechanistic changes which brought about significant evolutionary changes?

THE EUKARYOTIC GENOME IN DEVELOPMENT

Mammals such as the whale, the mouse and the bat are morphologically very dissimilar and are adapted to radically different ways of life. However all three have about the same genome size, and many of their genes and gene families will undoubtedly be very similar. What factors then are responsible for the enormous morphological differences between these organisms? As yet we do not know, but as a result of the molecular data base, we have a very good idea where *not* to search. The highly repetitive sequences, the mobile elements, the dispersed simple repeats and the pseudogenes can undoubtedly be ignored from a developmental viewpoint. This still leaves nearly 50,000 genes in a vertebrate only some of which will be the important decision-making ones. As Jackson (1986) has so clearly warned us, *"The hip young gunslingers of modern developmental biology shoot hard, fast and often inaccurately. Any likely gene which raises its head is quickly cloned and analyzed."* However, what we have learnt from our initial forays into the genome is that it is a cruelly unequal maelstrom. Initially it would be foolish to clone genes at random. What is required is to sort the most significant and interesting ones from their less important colleagues.

Executive Genes

How do we distinguish the executive genes from those which dutifully and unerringly carry out the more mundane cellular tasks? This is not at all an easy assignment, but it has been approached most readily in lower organisms such as the worm, *Caenorhabditis* (Horvitz *et al.,*1983) and *Drosophila,* where genes which affect early embryogenesis for example are very rapidly being cloned and sequenced (reviewed by Gehring, 1985). Why do the lower organisms show such promise for unravelling gene circuits involved in development and differentiation and hence in understanding how such genes and circuits produce morphological novelties? The answers are relatively straightforward when compared to mammals. The crucial gene circuits of mammalian embryology largely carry out their functions when the embryo is experimentally inaccessible; in human beings, most major formative events are well and truly over in the first 12 weeks post fertilization and these embryos are thus unsuitable for direct study. Secondly, the developmental genetics of human beings is in a rudimentary state and cannot easily be interfaced with molecular biology as yet. On the other hand, organisms such as *Drosophila* have a short life cycle, manipulative genetics and a burgeoning recombinant DNA data base. The ability to reintroduce genes into the *Drosophila* genome rapidly and precisely mean that significant inroads are now being made into the molecular biology of early development.

The apprehension of many biologists is that what is true for the fly will be irrelevant to human beings. As far as the basic developmental *principles* are concerned, however, these fears are largely being allayed. The finding that nearly

half of the gene products which occur in the brain of the fly cross-hybridize to the human brain was significant in this regard (Miller and Benzer, 1983). Clearly many components of the neuronal circuitry are likely to be very similar in vertebrates and invertebrates.

Owing to the enormous input into the genetics of this fly over the last half century, its genome has been saturation mutagenized so that most genes which affect a particular phenotype can be identified and then subsequently mapped and cloned. The 'importance' of the gene can also be evaluated, since in many cases it is possible to construct a homozygous deficiency just for the locus in question and determine the *null* phenotype, a procedure not routinely available in mammals.

Many genes which effect the anterior-posterior and dorsal-ventral gradients in the *Drosophila* egg have been discovered (Nusslein-Volhard, 1979; Anderson and Nusslein-Volhard, 1984). The *principles* which underlie these critical developmental gradients are likely to be variations on a theme, so it is sensible to start wherever a foothold presents itself. In *Drosophila,* the first steps in this area have already been taken. Genes affecting the segmentation of the body have been tracked down (reviewed in Nusslein-Volhard and Wieschaus, 1980), and many have been cloned. In the case of the gene, *Kruppel,* its predicted protein product turns out to be homologous to an important transcription factor in the 5S gene system of *Xenopus*. The significance of this finding is as yet unclear.

Similarly, in the *Drosophila* nervous system, the power of the genetic analysis has revealed genes which affect differentiation of parts of the ectoderm into neuroblasts. These seven genes, *Notch, almondex, big brain, mastermind, neuralized, Delta* and *Enhancer of split,* all cause specific hypertrophy of the nervous system at the expense of epidermal structures (Lehmann *et al.*, 1981). They all have affects on a switching process in which cell fates are altered from one state to another. The important point to note here is that by specifically targeting the genome for certain phenotypes, many of the genes affecting particular processes can be uncovered, a situation not yet feasible in mammals.

The message from *Drosophila* is that executive developmental decision-making genes are relatively few in number (Raff and Kaufman, 1983), maybe of the order of but a few hundred. Since this organism has about 10,000 transcription units, it is apparent that the bulk of the genes provide the metabolic backup for the decisions of the executive genes. Thus it is crucial, in the case of a mammal, where there are maybe 50,000 genes or so, to try and sift those few of developmental importance from the remainder. Deciding which of these may have been instrumental in the generation of evolutionary novelty is then the crux of the problem.

Genomic Raiding

Certain key decision-making genes in *Drosophila* contain a small sequence termed the homeo-box whose exact function is yet to be elucidated. When cloned probes of this sequence are used to challenge other organisms to see if they have sequences homologous to this probe, it is found that they do. Equivalent sequences are found in man, mouse, chicken and the amphibian *Xenopus* (Shepherd *et al.*, 1984). In *Xenopus* the sequence is expressed during early development beginning at gastrulation, just as in *Drosophila* (Carrasco *et al.*, 1984). Whilst the significance of such homologies is not yet totally clear, it is obvious that much of relevance to higher eukaryotes can be gleaned from the crossraiding of vertebrate and invertebrate genomes. For example, the calmodulin and pyrimidine biosynthesis genes of the rat have been used to isolate the corresponding genes from *Drosophila*. The oncogenes of certain vertebrate retroviruses have also been found in the fly. A heat shock gene of *Drosophila,* hsp70, has nearly 80 percent homology with its counterpart in the chicken. Thus there is clearly a component of eukaryotic genomes which through sheer conservation over evolutionary time, will be sufficiently similar between diverse organisms to allow genes to be isolated through cross homologies. What roles such highly conserved genes will play in the generation of evolutionary novelty is unclear, since the more conserved they are, the lower is their probability of contributing to innovation. However, one of the significant questions as far as evolutionary emphases are concerned is whether genes or pieces of genes which have such obvious homologies are performing the same suite of functions in the different organisms, or whether in fact their functional attributes have largely diverged. This is as yet a largely unknown area.

The invertebrate molecular data base has provided the first glimpses into how early developmental events are controlled at the level of the DNA. However, in spite of the breathtaking insights that possession of some of the cloned genes has given us, the real hurdles are yet to come. For example, when we consider a series of events as apparently simple as the dichotomy between feather or skin formation in vertebrates, we need to approach the problem at the level of *cell* interactions in the developing epidermis (Oster and Alberch, 1982). In this system, if the epidermal layer *evaginates,* scales or feathers are formed, whereas if the layer *invaginates,* skin and hair are the consequence. The molecular events at the *cellular* level which control the folding of the epidermal layer and the epidermal-dermal interactions are clearly the places on which to focus experimentally. This, however, involves an understanding of the underlying basis of cell *shape* changes and reminds us how little progress we have made in bridging the gap between DNA molecules and phenotype. In lower organisms, we are just at the beginning of determining which developmental gene circuits affect morphology. In mammals, our molecular data base is in a rudimentary state.

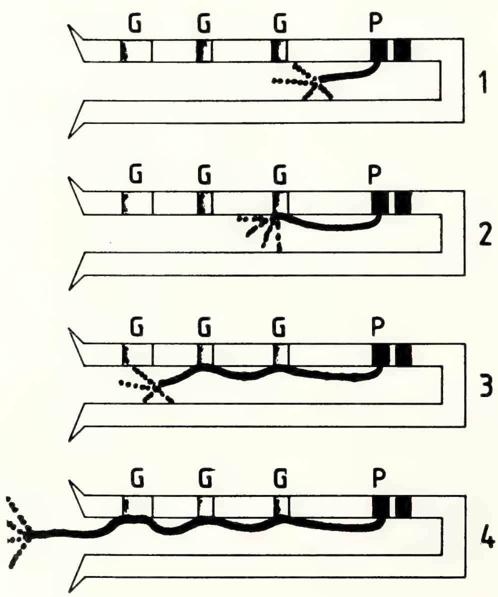
Nervous Systems

There is one particular facet of higher organisms, namely the nervous system, whose molecular development has yet to be explored in an evolutionary context. Prime attention has always focussed on external morphologies and with good reason. Moreover, since the limelight has fallen on the contributions of various morphological novelties such as the development of jaws, or the appearance of feathers, to evolutionary 'progress', the contribution of the nervous system has been less appreciated.

However, when the vertebrate brain is examined molecularly, a few shocks are already apparent. The total informational content of different messenger RNAs which are transcribed in tissues such as the liver and kidney, averages 30 million nucleotides. The informational content of the brain, however, is in excess of 110 million nucleotides (reviewed in Davidson and Britten, 1979). The cloning data of Milner and Sutcliffe (1983) are impressive (Table 3). Of 191 clones selected at random from a cDNA library of the rat brain, more than half are absolutely brain specific, with the remainder being differentially expressed in the brain, but also occurring in liver and kidney. In mammals, most of the genes may well be concerned with neural functioning! If this turns out to be so, then some of the cornerstones of evolution may not have been in those morphological areas that have received the most attention, but may well have been in producing the sophisticated computers that power our behavioural repetoires. This is indeed food for thought.

It is easy to be overawed by the molecular complexity of the functioning mature brain. However, this pales into insignificance when one considers how the brain is set up in the first place. The problems of how neurons make correct synaptic connections is an old one, but only now are some molecular approaches being seriously explored. Once again it is the invertebrates, with their less complex systems, that are providing the first inroads. A simple example reveals the complexity of the problem. In the developing grasshopper limb, certain cells, destined to be *pioneer* neurons, trace a pathway from the periphery of the developing limb to the developing central neurons system (Figure 3). They accomplish this by a process of filopodial exploration. When they come in contact with a guidepost cell, which presumably has characteristic cell surface properties relative to its neighbours, the correct direction of further exploratory movements is assured (Goodman and Bastiani, 1984; Goodman *et al.*,1984; Thomas *et al.*, 1984). Following such directional pathfinding,

PIONEER AXONS, GUIDEPOST CELLS



P = peripheral pioneer neuron
G is located by filopodial exploration

Fig. 3

the pioneer neuron must still have the information to generate a correct synaptic connection, at its final destination. Imagine the molecular information just to wire up the 'simple' 100,000 neuron computer of *Drosophila,* and compare it to the wiring task of the human brain, which has approximately 100,000,000,000,000 synaptic connections. When one considers the evolution of such biological computers, the problems of the generation of novel evolutionary wiring pathways seem somehow just as important as the evolution of morphological novelties, maybe in some cases even more so.

THE GENOME IN EVOLUTION

Facts and Theory

We return yet again to asking how morphological or neuronal novelty is generated and spread throughout a population. Has there been a significant evolution of *new* genes, or have the old genes, or duplicate variants of them been assembled into new circuits, which have themselves now led to new morphologies? We still do not know. The problem is even harder than this since, as we saw earlier, eukaryotic genomes are not simple collections of single genes, but consist also of multigene families. Neo-Darwinian theory has held that the spread and fixation of variants through a population is largely the consequence of natural selection of single gene systems with most evolutionary changes being thought to arise as adaptive responses to the environment.

The theory is experiencing great difficulty with multigene families even in their simplest form. Clearly when a variant family member arises, selection has to survey the family as a whole. Furthermore the *theory* misses the essential point that is so aptly summarized in the following, 'Selection may account for the survival of the fittest, but fails to account for the arrival of the fittest'. It is in the latter category that modern molecular biology is having its greatest impact.

However, a far more interesting and novel mode of change, the *process* known as molecular drive has been proposed and analyzed by Dover (1982, 1986) and it is especially applicable to multigene families. This mode of change is predicated on the observed molecular data which stem from the flux that goes on within a genome. DNA sequences promote their own amplification, dispersion, conversion and so forth and can obviously cause recurrent changes within a genome. This genomic turnover in multigene and multisequence families may well be *the* prime mover in evolution, in that DNA turnover mechanisms cohesively altering the genomes of a population could result in evolutionary novelties (Dover and Flavell, 1984; Dover and Tautz, 1986). Given all of this, however, we still have been unable to determine the underlying causes of morphological differentiation between the species we began with, namely whales, mice and bats. We will be unlikely to do this until we move our efforts away from the abstractions of neo-Darwinian *theory,* and return to molecular embryology. Once again, as pointed out long ago by Bateson in 1922, it is "*in embryology* (that the) *quintessence of morphological truth* (is) *most palpably presented.*"

The evolutionary problem comes home with even more force when we consider fossil sequences and try to speculate on the underlying bases of change. While some groups such as the Proboscidea, show a progressive and gradual series of morphological changes, others appear abruptly in the fossil record. Thus turtles appear fully formed as do pterodactyls and early amphibians (Lull, 1940). We will ultimately need to decide whether the underlying genomic changes were of a major kind or not. The root of our current problems is our inability to determine whether any morphological change is initially the result of a change in a single gene, a multigene or multisequence family, or whether it is a novel gene.

CONCLUSIONS

Let me summarize then some of what has been learnt about the structure and function of eukaryotic genomes and how this information has influenced our perspectives on the underlying bases of phenotypic change:

1. The study of genomic DNAs has revealed the existence of junk DNA, mobile elements, pseudogenes, split genes, multigene families, and sundry DNA turnover processes. We are still evaluating which of these are more or less important for phenotype.

2. The ubiquity of multigene families, when considered together with the cellular enzymic machinery, means that there is enormous potential for the generation of evolutionary novelty from within. There is now a distinct possibility that evolutionary changes stemmed more from within the genome than from external forces.

3. The demonstration of key executive genes in *Drosophila,* together with their homologous sequences in higher eukaryotes, has yielded a clearer view of the circuits involved in particular morphological and neuronal programmes. It is now necessary to determine how *easy* or how *difficult* it is to perturb such circuits. This will yield some idea of what it *costs* to evolve in a genetic sense. It is further necessary to determine in which directions a circuit can be modified in order to be able to guess how a given structure can alter.

The mechanistic side of evolution is now much more exciting than it has ever been, with the very real prospect that we shall soon be able to understand what makes the genome tick in a structural sense. Then we can go on and think about the harder problems, protein-protein interactions, and ultimately cell-cell interactions through time. Then and only then will we be able to guage how difficult a problem we have set ourselves, in enquiring about the molecular bases of morphological novelty and the molecular costs of neuronal novelty.

It ought to be obvious that the molecular analysis of development offers some hope of unravelling the origins of phenotypic structural change. The conventional approaches of 'explaining' the origins of morphological novelties, via the neo-Darwinian selectionist-neutralist debate is a sterile one, because it is not addressed to the *genesis* of the changes themselves. Levin (1984) has pointed out that "*the neutralist-selectionist controversy is more a product of the sociology of science (the two camp advocacy approach) than its*

substance." Whilst this controversy has dominated molecular population genetics since the early 1970s, its contribution, when measured against the burgeoning molecularly precise data bases, has not only been oblique to central evolutionary issues, but in the light of multigene families and molecular drive, may well turn out to have been an irrelevant side issue.

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TABLE 1
VARIATION IN NUCLEAR DNA AMOUNTS IN VARIOUS INVERTEBRATES
(from Conner *et al.*, 1972; Hinegardner, 1974)

ANNELID WORMS			Genome size in million base pairs
Family	*Nephtyidae*	*Nephtys incisa*	6600
		Neptys sp.	2200
	Cirratulidae	*C. luscuriosa*	3000
		C. grandis	600

MOLLUSCS		
Limpets	*Lottia gigantea*	400
	Acmaea mitra	900
Venus Clams	*Tivela stultorum*	900
	Mercenaria compechiensis	2100
Nut Clams	*Nucula proxima*	2900
	Acila castrensis	5000

TABLE 2
VARIATION IN NUCLEAR DNA AMOUNTS AND CHROMOSOME NUMBERS IN VARIOUS VERTEBRATES (from John and Miklos, 1987)

	Genome size in million base pairs	Chromosome number
Muntiacus vaginalis (barking deer)	2400	3
M. reevesi (barking deer)	2900	23
Homo sapiens (humans)	3300	23
Orcyteropus afer (aardvarks)	5500	10

TABLE 3
PARTITIONING AND EXPRESSION OF POLYADENYLATED MESSENGER RNAs IN THE RAT BRAIN (from Milner and Sutcliffe, 1983)

	Per Cent	
CLASS 1	18	Present equally in Brain, Liver, Kidney
CLASS 2	26	Differentially expressed in Brain, Liver, Kidney
CLASS 3	30	In brain only
CLASS 4	26	In brain only; very rare brain mRNAs

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Department of Molecular Biology,
Research School of Biological Sciences,
The Australian National University,
CANBERRA, A.C.T. 2601

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Inaugural Poggendorff Memorial Lecture: Walter Poggendorff — Pioneer Plant Breeder

D. G. McDonald

This Inaugural Poggendorff Memorial Lecture was delivered on November 6th, 1986, at the Hawkesbury Agricultural College, Richmond, in memory of Walter Poggendorff who died on February 7th, 1981. Dr. D.J. McDonald, MSc.Agr., Ph.D., Regional Director of Agriculture, Orange, N.S.W., was chosen as the speaker on account of his long-standing professional association with Walter Poggendorff.

Dr. McDonald's main research work has been in the field of rice. He was a rice-breeder and research agronomist at Yanco and later the Regional Director of Research there, before becoming Regional Director of Agriculture for the Central West, South-East and Illawarra regions, based at Orange. After initiating the current rice-breeding program in N.S.W., he bred Australia's first long-grain rice variety, called Kulu. He also pioneered the use of aerial seeding techniques for rice in N.S.W. His wide experience was used in Bangladesh, Burma and China to help their agricultural programs. It is no surprise that Dr. McDonald was awarded the Farrer Memorial Medal for Distinguished Service to Australian Agriculture.

INTRODUCTION

Walter Hans Poggendorff was Chief, Division of Plant Industry in the New South Wales Department of Agriculture when I first met him in 1957. He was visiting Leeton Experiment Farm on one of his regular country visits to see "whatever might be of interest".

Having been appointed as a Research Agronomist (Pastures) only a few weeks before, I found his personal interest in my initial experiment very encouraging. His suggestion that I consider taking up the job of Rice Breeder/Agronomist at Yanco, shortly to be vacated when Tom Lawler resigned to go north, took me completely by surprise! However, in April of that year I went to Yanco Experiment Farm to assist with the harvest of rice plots. So began my career in rice breeding and research which was to bring me into close contact with Walter Poggendorff for the next 10 years.

"Pogg", as most of his friends affectionately knew him, took great interest in the rice research program until he retired in 1968. It was his conviction about the need for adapted long grain varieties of rice that stimulated me to try my hand at breeding them. His knowledge of the crop, and perspectives born of his own encounters with it, are lasting impressions for me. He never failed to spend time with me on his country visits, especially during those early years, and rarely arrived without some snippet of new information or a fresh suggestion about the work.

Though he concentrated on rice, Walter Poggendorff was no stranger to an awesome range of other crops that came within his breeding portfolio. He told me that the advice given to him in Head Office as he was leaving to take up his position of Assistant Plant Breeder at Yanco was to "go and cross up everything you can lay your hands on", advice which he took to heart. Few plant breeders could lay claim to having contributed in such widely diverse species as rice, peaches, apricots, grapes and rock melons.

Plant breeding as a science was still in its infancy when he began his work in 1928. Looking back from our present position, less than 60 years down the track, the implications of his statement that "Only one variety of rice, of the many thousands in existence, is known to have been deliberately bred by man...." (Poggendorff, 1937) is difficult to comprehend. Walter Poggendorff literally was a pioneer in Australian plant breeding.

RICE BREEDING AND RESEARCH

The rice industry to which he came as Assistant Plant Breeder in 1928 was a turbulent youngster. In 1925, during the first year of commercial cropping, the Murrumbidgee Irrigation Area Ricegrowers' Society was formed. Within a year this Society had persuaded government to impose a protective duty on imported rice. They fought bitterly to have the Irrigation Commission liens over crops removed and in 1928 successfully petitioned for establishment of a Rice Marketing Board. At the same time, co-operation on the technical front was widened with grower representatives being added to the Rice Investigations Committee (I.R.E.C., 1955).

Poggendorff's assignment was "to undertake the task of providing pure seed stocks of the varieties in demand, and effecting any improvement possible in the yield, quality or type of rice" (Anon., 1937).

Seed Certification

Only a few varieties had been tested by

growers and seed of those cultivated commercially in 1928 had become badly mixed (Wenholz, 1928).

After his first season as Assistant Rice Breeder, Poggendorff was able to report that he had started pure-lining the two varieties of most interest, Caloro and Colusa. He had already noted (report to the Director of Plant Breeding, June 1928) the range of promising variants in Caloro including "..a few very good yielders...". A third variety, Wataribune, from which Caloro had been selected in California, does not seem to have been included in the program for long. Ultimately, it was discarded by growers because of its very late maturity and heavy awn (Anon., 1937).

He also reported that a very significant part of the pure seed problem was due to inability of growers to distinguish between seed of different varieties which they sometimes mixed. His first published paper in the Agricultural Gazette was a detailed description of Caloro and Colusa aimed at rectifying this problem (Poggendorff, 1928).

Twelve tons of the first pure strain of Caloro, named Caloro II, were distributed in 1932. A year later two tons of another pure strain, named Late Caloro, were released on trial. Despite its late maturity the latter gained instant popularity because of its high yield and better grain quality. By 1936/37 these two strains were estimated to occupy 30% of the total area of rice and the demand for seed supplies could not be met (Poggendorff, 1937).

Purifying the earlier maturing variety, Colusa, proved a much more difficult task. Red rice contamination was eliminated quite quickly but, on three successive occasions, bulk seed plots were rejected because of the reappearance of offtypes. A relatively high degree of natural crossing over much greater distances than anticipated was found to be the reason. Pure seed of the variety was finally released in 1935 as Colusa 180 together with another very early Colusa Selection 146-1, though neither gained much popularity.

By 1936 the problem of seed purity had been resolved and Poggendorff was able to report -

> "Thus far, a complete series of varieties, late, midseason, early and very early, all of the same short-grain type, has been produced for commercial culture....With the entry of the last into the continuous pure seed production scheme evolved, the first objective of the improvement programme commenced in 1928 may be considered achieved" (Anon, 1937).

Experiment on Natural Crossing

In the course of his selection work, Poggendorff realised that, if natural crossing occurred between varieties some distance apart, there were serious consequences for the design of pure seed layouts. Using a unique "circle" design which he devised for the purpose, he established that natural crossing was reduced to 0.06% when varieties were separated by 10 feet (Wenholz, 1936).

This experiment, though similar in concept to earlier ones conducted in the U.S.A., was a mark of Poggendorff's ability to improvise. The results were apparently not published in detail but they were certainly taken into account in the pure seed program where "The results obtained led to a complete reorganisation of stud plot methods" (Anon., 1937).

Dwarf Strains of Caloro

Buried amongst his comments on improved strains of Caloro and Colusa is an intriguing observation -

> "Among the interesting curiosities which have been discovered during the course of this selection work, are two "mutations" in Caloro, both dwarfs, but completely fertile." (Poggendorff, 1937).

One wonders what the outcome might have been had Poggendorff taken these "curiosities" seriously enough to persevere with them as potential varieties. Perhaps he did but I can find no further specific reference to them. Little did he realise that just such dwarf phenotypes would one day revolutionise concepts of high yielding plant type. It is likely of course that, without chemical weed control, accurate land levelling, and high rates of fertiliser, the dwarfs would have performed comparatively poorly.

Variety Improvement

While the first priority of the time was to lay the foundation for pure seed production of Caloro and Colusa, Poggendorff very quickly became interested in other areas of variety improvement. In particular he saw the need to do something about straw strength and quality (milling and cooking quality) of the varieties grown.

Introduction of Foreign Varieties

From the outset, local millers and processors were asking for higher quality grains than the Californian types like Caloro and Colusa. Varieties were introduced from all sectors of the rice-growing world in an attempt to find the desired types. Unfortunately, those originating in tropical countries generally were far too late maturing to be of any direct interest. Others from China, Spain, Italy and Japan were very much like the standard varieties already grown. However, the American varieties included some medium and long-grained rices which, though all later maturing than Caloro, appeared to have possibilities (Wenholz, 1931). A few impressed Poggendorff with their quality and/or straw strength, notably Lady Wright and Texas Patna. Others such as Blue Rose and Edith followed in later years and were "acclimatised".

A most interesting feature of Poggendorff's reports on these introduced varieties was his quite frequent reference to their "progressive adaptation". His observations led him to assert that "The time taken to reach maturity often diminishes, comparatively rapidly at first, then more slowly, and in some cases a certain amount of minor "breaking up" occurs in apparently pure varieties, probably due to actual "non-fixity of the varieties' reactions to new light and temperature conditions" (Poggendorff, 1937). The "acclimatisation" of some U.S. varieties, such as Ediths and Blue Rose, which had special attractions from a quality point of view, is specifically mentioned.

In later years we discussed this phenomenon many times and I think it is fair to say that he was unwilling to rule out the possibility of genetic adaptation.

Intervarietal Crossing

With the pure seed program well established, Poggendorff set about trying to make crosses between the best of the high quality introductions with the adapted *japonica* varieties. His belief was that -

"In artificial crossbreeding undoubtedly lies the greatest hope of further rice improvement, and one which has as yet been barely touched" (Poggendorff, 1937).

Making crosses proved extremely difficult under the hot dry conditions and only succeeded in doing so after a detailed study of flowering and pollination (Poggendorff, 1932). He devised his own method which relied upon choosing dull, cloudy days when flowers would open before anthers dehisced. Fresh pollen was obtained by floating heads of the donor parent on the water where higher temperature and high humidity caused flowers to mature and open (Poggendorff, 1933).

Wenholz reported excitedly in 1932 that -

"A few grains have been developed from crosses of Caloro with Blue Rose and Lady Wright, and it is expected that this modest beginning will be an epoch-making stage in the improvement of Australian rice".

The American varieties were said to be so superior in grain quality, and to command such an advantage in price on the export market, that the combination of such quality with early maturity by crossbreeding was "clearly indicated as an urgent need in the improvement of Australian varieties".

As good fortune would have it, one of these crosses, Lady Wright x Caloro did produce progeny with good potential. In his report to the Director of Plant Breeding in 1935, Poggendorff described one of these, Lady Wright Crossbred 13-3-1, as follows:

"Large medium type grain like that of Blue Rose, but longer and broader; matures slightly after Caloro II and before Late Caloro and Blue Rose. Decidedly one of the most promising selections of its type yet tried".

Writing to H.S. Gardiner, Managing Director, Waters Trading Co. Sydney, on 6/5/39 he described Lady Wright Crossbred selections as follows:

"13-3-1 and 13-3-3 are examples of practically complete success in attaining the desired objective - both are excellent yielders, far surpassing any introduced "medium-long" grain variety in this respect, though not quite equal to the best of the short-grains; both have outstanding straw strength, and I think that their appearance and cooking quality are beyond reproach.

13-3-1 could be grown commercially at practically the same price as Caloro; it is unfortunate that there is no better local market for this type than you indicate, even if the export market is closed by competition; every one of many persons who have been given the opportunity of trying a small sample of 13-3-1 cooked has been anxious to obtain more; for instance, the local hotel company would be glad to purchase two tons annually".

Despite his optimism and unfailing belief that the new varieties had a place in the local industry, they were never to be taken up commercially though testing continued until the mid 1950s. The reason for this is difficult to ascertain. The correspondence makes it clear that rice traders in Australia and Britain generally commented favourably though they seemingly were reluctant to move away from their proven sources of high quality rice, notably Burma.

The selections tested in the 1950s were of rather bold grain appearance. I examined these carefully with Poggendorff who asserted that the more slender character of his original lines had been lost. It proved impossible to recover those original lines from foreign collections though I tried for several years to do so.

Numerous crosses were made in the 1930s in an attempt to combine early maturity, grain quality, straw strength and yield. In his report to the Director of Plant Breeding dated 10/8/39, Poggendorff made the following observations -

"Special attention has been directed to the improvement of straw strength and cooking quality of the standard short-grain varieties. It is a rather peculiar fact that the short type of rice grain is almost invariably associated with rather weak straw; the medium and long-grain types vary widely in straw strength, but a few, such as Lady Wright and Edith, possess outstanding strength, again invariably associated with poor yields under local conditions. The problem of breeding a strong-strawed, heavy-yielding short-grain of good quality is further complicated by the strong dominance of the long-grain character in the hybrid progeny. As a result of repeated attempts and long selection, several strong-strawed short-grain types of good quality and promising yield have been isolated; these are of the breeding Lady Wright x Colusa and Lady Wright x Caloro".

This intention to "improve the quality" of short-grain varieties by making them firmer cooking would later be provéd to be a mistaken concept since the soft cooking character of these varieties is preferred by those who normally eat them. The association of short-grain type and high yield with weak-straw and medium/long-grain type with low yield and often strong straw, together with the partial sterility of many progeny of crosses between the different types which he noted (Anon., 1939) would continue to frustrate breeders across the world for many years to come.

The fact that Poggendorff was not successful in breeding a variety of high quality which was taken up by industry, should not reflect badly on him. A letter from Dr. Jenkin W. Jones (Senior Agronomist in Charge, Rice Production and Improvement, U.S. Department of Agriculture) to the Director of Plant Breeding on 1/10/31 makes it clear that he was trying to

do the same thing for California. He wrote in part -

"I have been working on development of suitable long and medium-grain rices, for California, during the past five years. We have at the Biggs Station some promising selections from crosses between long- and short-grain varieties, short- and medium-grain varieties, and long- and medium-grain varieties, but none of the selections are fixed well enough to warrant distribution, though a few which are being tested in the nursery look very promising. Within the next five years we expect to have medium- and long-grain varieties that are adapted to conditions in California".

Sufficient to say that, with all the resources available to him, he was no more successful than Walter Poggendorff. In fact, in the long run, it was my privilege to breed the first commercial long-grained rice variety, Kulu, for southern New South Wales which was released in 1967, more than 30 years later and well in front of California.

The Special Problem of Milling Quality

In his annual report to the Director of Plant Breeding for 1933/34 Poggendorff refers to a serious problem of grain cracking in commercial crops the previous season and gives account of a "varietal study of the trouble" -

"It appears that:

1. There is a marked difference in the amount of cracking in different varieties and strains.

2. Long-grained varieties as a class are consistently more subject to cracking than short-grained.

3. In a very general way, lateness of maturity means less cracking - there are some notable exceptions.

The matter appears somewhat complicated, and is difficult to generalise upon, but the evidence is sufficient to indicate the probable importance of taking this factor into consideration in selection".

The prospect of selecting for resistance to cracking must have been daunting for he comments -

"Some form of shelling device that would hull without breaking sound grains would greatly facilitate this work, and quality analyses; hulling each individual grain with the thumb nail or teeth becomes extremely tedious".

Susceptibility to grain cracking, which resulted in poor milling quality, was to become a key issue in variety development in the future and his sense that it should be taken into account in selection was eminently justified. So serious did the problem become that sample milling equipment was purchased by the Rice Marketing Board to facilitate research (Anon., 1940).

BREEDING FRUIT TREE, VINE AND CUCURBIT SPECIES

As Assistant Plant Breeder at Yanco, Walter Poggendorff was responsible for working not only with rice but also with peaches, apricots, pears, almonds, grapes and rockmelons. It would be impossible within the confines of this paper to fully document his work with these species and, in any case, many of the original reports appear to have been lost or discarded. I have simply attempted to draw out the highlights of his achievements in these areas from 1928 to 1939 and to identify any fruits of his labours that were not matured until years later.

Peaches

The primary objectives of Poggendorff's work with peaches as given in his own report to the Director of Plant Breeding entitled "Peach Improvement Committee - Canning Peach Breeding" dated 29/7/39 were:

"1. The development of earlier and better canning varieties than any available at the time (Leader, Levis, etc.) to help close the existing gap of about a fortnight between the latest maturing canning apricot (Trevatt) and the earliest peaches.

2. The replacement of red-centred maincrop varieties, particularly Pullar's Cling, with clear-centred varieties of equal or better quality and yielding power".

Work started in 1928 and encompassed crossing, raising seedlings of standard varieties and introduction of new varieties. Nearly 30 crosses were made using varieties readily available with the particular purpose of eliminating the red centre from varieties like Tuscan and Pullar and transmitting the earliness of Leader to some progeny. The idiotype to which he worked was -

"one that combined the quality of Golden Queen, the yield and size of Pullar's, and the fruiting habit of Goodman's, had a smooth symmetrical shape, clear-centre and a small pit".

Hundreds of seedlings of all available canning varieties were raised "both in the hope of obtaining useful new varieties, and to discover by the general behaviour of the seedlings, the probable inherent value of the varieties as parents."

New varieties were introduced mainly from South Africa, China and the U.S.A. He was particularly impressed by a set of unnamed crossbreds which he obtained from Dr. Wight in California as a result of his visit to the U.S.A. in 1935.

Six of Poggendorff's crossbred selections, as well as a Transvaal Yellow seedling selection, had been passed by Leeton Cannery authorities at least twice by 1939. In his final report to the Director of Plant Breeding dated 11/7/40 an obviously disappointed plant breeder commented that two of his early crossbred selections of which much had been expected, were likely to be outclassed by some of the Californian material. He went on to say -

"Though these findings are naturally somewhat disappointing, it must be remembered that the Tuscan X Leader hybrids are the first-fruits of the canning peach breeding programme in N.S.W., endeavouring to compete against selected material which is the product of a very much older and very extensive American breeding programme; indeed, considering the circumstances, it is encouraging to note how close to the desired mark the local hybrids have attained."

The Transvaal Yellow seedling selection was released under the name "Wight" following rigorous field testing and on the recommendation of a Canning Peach Conference convened by the Department of Agriculture in 1946. "Wight" was described as:

"the name given to the Transvaal Yellow seedling raised at Yanco. The variety is yellow-fleshed clingstone, with a clear centre, and has exhibited outstanding field and processing qualities. Wight matures late in the season at approximately the same time as the standard variety, Pullars Cling. It is recommended for general planting on the Murrumbidgee Irrigation Area because of excellent quality for canning" (Anon., 1950).

Two crossbred selections from the Californian material obtained from Dr. Wight were also named and released at the same time.

Despite this recommendation, Wight has not been widely planted and the industry has settled almost exclusively on the variety Golden Queen or its derivatives.

Apricots

Breeding work with apricots was aimed at the production of later maturing varieties than the standard, Trevatt. Such varieties, it was hoped would bridge the gap between the last of the Trevatts and the earliest canning peaches.

Poggendorff made many crosses with this end in mind using both Trevatt and Moorepark as the standard parent. However, his efforts were to end in disappointment since -

"practically all the seedlings which have shown any promise in quantity, size and yield are earlier, or no later, than Trevatt. The progeny of several late seedlings which were selected for crossing for this character, have failed to satisfy in other respects" (Anon., 1939).

In the hope of achieving a range of useful characters, he introduced new varieties from the geographical centres of origin of the apricot but early results were disappointing. Numerous progeny of seed from Morocco, Palestine and Southern Asiatic Russian States, though showing considerable variation, in general lacked quality and size, and were usually very early (Anon., 1939).

Plums and Prunes

Although he is reported to have made a number of crosses between prune varieties, over a period of at least three years, it seems that nothing ever came of them.

His main contribution in plums was to demonstrate over several years that the varieties d'Argen and Robe de Sergeant were practically self-sterile. He further proved that President and Angelina were the best pollinators for d'Argen and that the Giant prune was rather unsatisfactory (Wenholz, 1932).

Pome Fruits

The only perceived need for breeding work to be done with pears was in the Murrumbidgee Irrigation Area were an improved canning variety was required. Poggendorff made crosses with this objective in view but no reports of useful selections from his program could be found (Wenholz, 1934).

As with plums, he conducted extensive pollination studies and identified the best pollinator varieties to be used (Wenholz, 1932).

As an interesting aside to the pollination studies, Poggendorff undertook an investigation of the effect of cross-pollination on colour in pome fruits (Poggendorff, 1930). The need for such an investigation was amply demonstrated by -

"the persistence of a belief among growers that, especially in apples, varieties which are naturally poorly coloured can be improved to some extent in this respect through pollination by well-coloured varieties, and *vice versa*".

To resolve the issue, he made reciprocal crosses between Granny Smith, a green apple, and seven highly coloured varieties including Delicious and Yates. The results were clearcut -

"not one of these crossbred fruits showed any variation in colour from that of the normally fertilised or self-fertilised female parent, nor was any colour difference observed in the reciprocal crosses, or in crosses between the coloured varieties themselves.

Similar observations have been made with pears".

Almonds

Almond growers in the Murrumbidgee Irrigation Areas, and elsewhere, complained that some varieties did not crop satisfactorily despite being in good condition. In 1932 Poggendorff set about trying to elucidate the causes of the trouble and identify appropriate solutions. He quickly established that fruit set of all varieties could be increased greatly by pollination with other suitable varieties. Some particular varieties, such as I.X.L., were almost completely self-sterile. (Wenholz, 1936).

He continued pollination studies on almonds for about four years with the ultimate objective of identifying "varieties of greatest commercial value which will interpollinate satisfactorily to ensure the best possible crops". By 1936 he put forward a tentative list of recommended varieties and pollinators (Anon., 1937).

These studies required very large numbers of cross-pollinations and detailed observations of

fruit set. A year after transfer of the program to Wagga Experiment Farm it was reported that "some 22,000 hand pollinations have been made". The majority were obviously made by Walter Poggendorff.

Grapes

The 1931 annual report of the Director of Plant Breeding advised that "it has been deemed advisable to do some cross-breeding work with grapes with the object of improving on our present varieties" (Wenholz, 1931). Because of the importance of grapes in the Murrumbidgee Irrigation Area, Walter Poggendorff was the obvious person to take on this work.

Primary objectives of the program were to incorporate disease resistance into wine-grape varieties and to improve the standard table variety Ohanez, which had excellent carrying quality but lacked flavour and disease resistance and was an undesirable colour (white). The additional task "of developing a seedless raisin or a larger sultana of better quality" was added shortly afterwards (Wenholz, 1932).

Despite his efforts to incorporate disease resistance from American varieties into local varieties of European origin, Poggendorff met with little success. He considered that the number of seedlings raised from each cross was insufficient to provide a reasonable chance of selecting the complex set of characters required (Anon., 1937). The difficulty lay in emasculating large numbers of grape flowers. Characteristically he found a way around this using "a new technique of emasculation by the use of hot water prior to anthers....". However, little came of the wine-grape program. Progress with the sultana/raisin project was also blocked by incompatability of Gordo Blanco with some other varieties.

Work with table grapes proved more promising. Poggendorff crossed Ohanez with several black skinned varieties. Two selections from an Ohanez x Purple Cornichan cross proved promising and one was eventually released under the name "Nyora". The description of Nyora given by Hedberg (1985) reads -

> "It is an attractive, early late season grape.
>
> Nyora berries are dark purple, large, long oval and seeded. The berry skin is tough, the pulp is soft and fleshy and the berries have a pleasant vinous flavour. The bunches are large, shouldered conical and well filled to compact. Vines are vigorous and the large, hairless, dark green leaves have shallow lateral sinuses and a narrow petiolar sinus.
>
> Nyora vines are usually cordon trained and spur pruned. Barren shoots are a problem when vines are either spur or cane pruned, causing poor yields in some years. The bunches are very susceptible to bunch dieback and mealy bugs. Nyora grapes look attractive (aided by a heavy bloom), travel well and store for up to 14 weeks".

Rockmelons

In December, 1929, Poggendorff published a detailed description of rockmelons and casabas which could be grown in the Murrumbidgee Irrigation Area as a guide to growers and seedsmen. (Poggendorff, 1929). He classified them not only on maturity but also on their commercial attractiveness and tolerance of transport on the long haul to capital city markets. This initiative was designed to deal with problems of "wrong nomenclature of varieties on the part of some local seedsmen" and to counteract the practice of "cataloguing delicate greenhouse varieties of English origin which are a complete failure under our field conditions". (Wenholz, 1932).

Subsequently he undertook a considerable program of breeding and improvement work with both rockmelons and casabas. Disease was a major problem and successive outbreaks of powdery mildew and downy mildew decimated susceptible varieties. A severe epidemic of powdery mildew in 1934 revealed only two resistant lines but the importance of this disease was reduced by the identification of suitable resistant varieties from Dr. Jagger in the U.S.A. (Anon., 1939). Downy mildew continued to be a major problem.

While no reference can be found to the release of any variety arising from his crossing work, it is clear that Poggendorff made a major contribution to the development of rockmelons and casabas by classifying and purifying varieties, and introducing and testing new, good quality varieties resistant to disease (particularly powdery mildew) which allowed the industry to grow.

CONCLUSIONS

Walter Poggendorff was undoubedly one of Australia's outstanding plant breeders. That he was able to work successfully with such a wide range of crops, and that his work was invariably of practical benefit to industry, speaks volumes for his intellectual ability, his powers of observation, his great practical skills and, above all, his dedication and energy.

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N.S.W. Department of Agriculture,
Orange Regional Office,
181 Anson St.,
Orange, N.S.W., 2800, Australia.

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Earth History of the Southeast Indian Ocean and the Conjugate Margins of Australia and Antarctica

J. J. Veevers

Clarke Memorial Lecture, delivered to the Royal Society of New South Wales, 5th August, 1987, at Macquarie University.

ABSTRACT. Australia and Antarctica occupied the eastern part of Gondwanaland until their breakup by the growth of the Southeast Indian Ocean in the mid-Cretaceous (96 Ma). The growth of the ocean was preceded by a stage of separation of the land areas of the continents that started in the mid-Jurassic (160 Ma). This separation was effected by 360 km of extension (6 mm/year) by normal and listric faulting in a 600 km wide zone represented today by the submerged continental margins. In the first phase of seafloor spreading, from the mid-Cretaceous to the Eocene (45 Ma), the Southeast Indian Ocean grew to a width of 500 km at the slow rate of 10 mm/year, and in the second phase grew another 2600 km at a fast rate of 60 mm/year. Even so, Australia and Antarctica remained side by side along the N-trending transform faults in the east until the Oligocene (35 Ma). With their subsequent definitive separation, the Circum-Antarctic Current was able to flow unimpeded through the oceanic gap with the result that Antarctica became isolated from the surface water of the rest of the world ocean and this contributed to its intense glaciation.

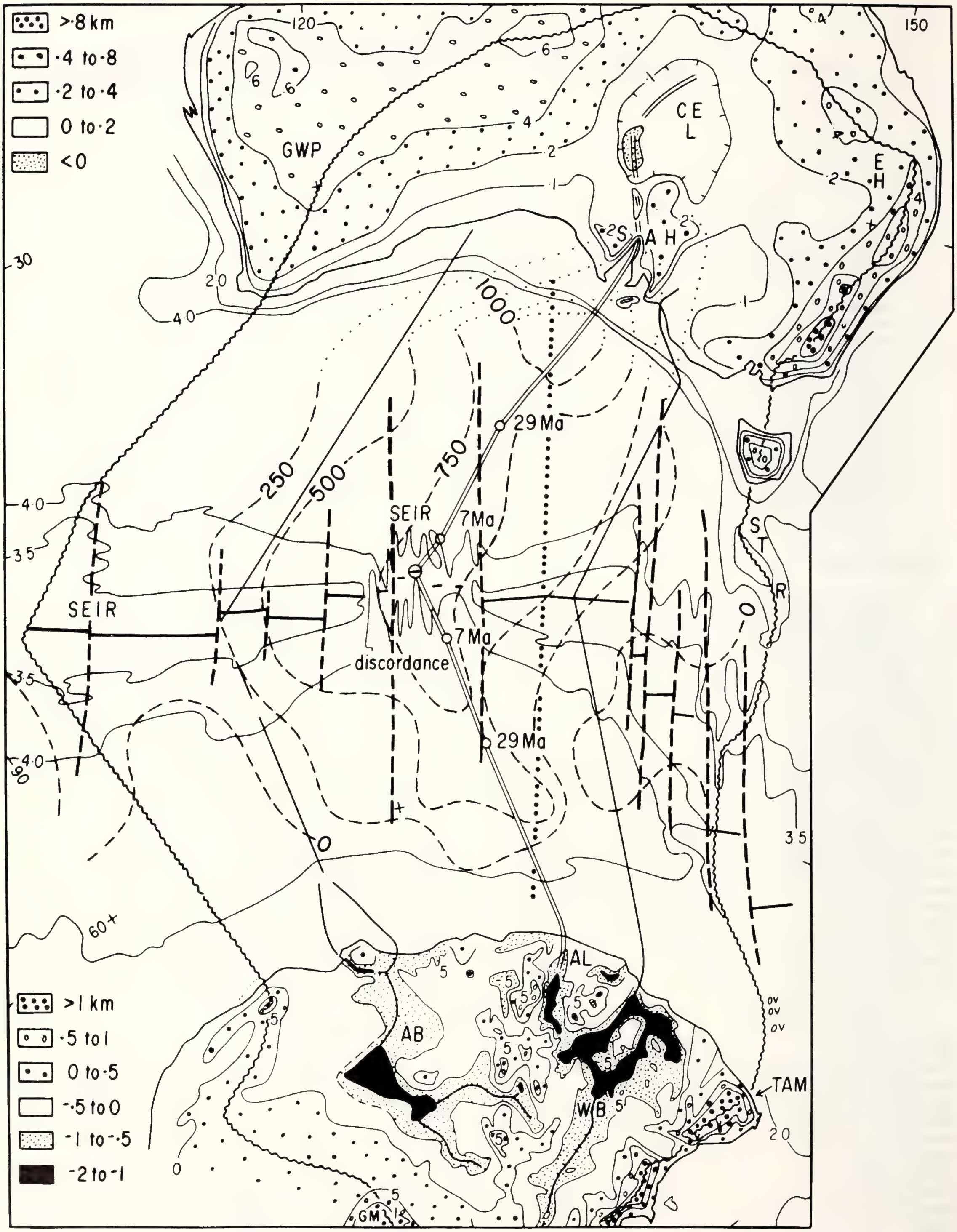
INTRODUCTION

The topic of this lecture may seem at first sight to have little connection with Clarke's area of interest. Clarke did not visit Antarctica but in 1856 and 1860 he did the next best thing in visiting Tasmania. For, as I shall try to show, Tasmania is closer in kind to Antarctica than to mainland Australia, and only a late change of allegiance led Tasmania to stay with the mainland. Another connection is that, in company with all those who voyaged by ship to Australia, Clarke would have had an all too intimate acquaintance with the highly corrugated surface water of the Southeast Indian Ocean in the Great Australian Bight. He got to know the seafloor off Sydney better by spending a few days aboard the HMS *Challenger* in 1874 and the USS *Tuscarora* in 1875, and these cruises 'convinced him that the ocean was just as much the province of the geologist as was the land' (Grainger, 1982, p. 250).

The Southeast Indian Ocean is an interloper between the continents. Its growth can be traced back to 96 Ma, and its embryonic growth by intracontinental extension to 160 Ma. Eliminating these two phases of separation brings Australia and Antarctica together along the original suture. Wegener (1915) made the first sketch of this reconstruction but it was not until the floor of the Southeast Indian Ocean became surveyed during the research cruises of the R/V *Eltanin* of Lamont-Doherty Geological Observatory in the 1960s and 1970s (Weissel & Hayes, 1972; Konig, 1987) that a precise basis for reconstruction was made possible by the recognition of the pattern of seafloor-spreading magnetic anomalies. Subsequently Cande & Mutter (1982) interpreted the magnetic anomalies as indicating two stages of seafloor spreading: fast back to 45 Ma, slow from 45 to *ca* 100 Ma [narrowed by Veevers (1986) to 96 Ma]. The phase of continental extension along the southern margin of Australia was outlined by Boeuf & Doust (1975) and Talwani *et al.* (1979), and brought into sharp focus by a study of multichannel seismic profiles in Bass Strait by Etheridge *et al.* (1984). Deep seismic profiles of comparable quality were made from the R/V *S.P. Lee* by the U.S. Geological Survey in 1984 (Eittreim & Smith, 1987), and with data from the conjugate Australian margin provide a coast-to-coast cross-section through the crust of the conjugate Antarctic and Australian margins (Veevers, 1987).

PRESENT MORPHOTECTONICS

The region is bisected by the Southeast Indian Ocean Ridge (SEIR), the crest of which marks the divergent boundary between the Indo-Australian and Antarctic Plates (Fig. 1). The ridge trends from W to E to 139°E and is offset along transform faults to the SSE, in sympathy with the trend of Tasmania/South Tasman Rise (STR) and the trend of conjugate Antarctica E of 150°E. The profile of the seafloor at 132°E (Fig. 2), in a direction parallel to spreading, is dominated by the deep depression of the



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-·5 to 0
-1 to -·5
-2 to -1
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AL
WB
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GM

Southeast Indian Ocean: from 3 km deep at the crest to 5 km at the foot of the continental margins, modelled as due to thermal contraction of the oceanic lithosphere (depth proportional to age$^{1/2}$), and all of it is anomalously deep. This ocean is 3000 km wide, with the ridge flank on the N slightly wider and appreciably deeper than that on the S. The sediment cover is very thin (<100 m) except beneath the abyssal plains: up to 1000 m in the N and 3500 m in the S. The spacing of the magnetic anomalies, reflecting the spreading rates, has a similar asymmetry for anomalies older than anomaly 6 (A6). The time scale shows that 12% of the seafloor spread during the first, slow half (96-45 Ma) and 88% during the second, fast half (45 Ma to present). The azimuth of spreading differed also; fast spreading, as now, was to the N, and slow spreading to the NNE. Another asymmetry in the magnetic profile is that the oldest seafloor at 132°E in the north is 80 Ma, and in the south 96 Ma, attributed to a NNW jump of the spreading ridge at 80 Ma with an attendant transfer of the older seafloor to the Australian Plate (Veevers, 1987).

Profiles of the seafloor and adjacent continents in a direction normal to spreading (Fig. 3) are dominated by a broad depression, kidney-shaped in plan, called the Australian-Antarctic Depression (Veevers, 1982). The axis is simple and has the shape of a valley-in-valley on the continents. To afford a direct comparison, the ice cover of Antarctica (ruled lines in Fig. 3, XI) is isostatically unloaded from the bedrock to the position of the broken line. The axis of the depression at the SEIR corresponds to the Australian-Antarctic discordance (Weissel & Hayes, 1974). It is a region of anomalously rough and deep seafloor with large asymmetry of spreading rate and magnetic anomaly amplitude as detailed by Vogt *et al.* (1983). The anomalous depth of the seafloor at the SEIR is >750 m (Fig. 1), and it corresponds to a negative satellite free-air gravity anomaly (FAA). The depth anomalies and the FAA extend N and S of the ridge to the rest of the Australian-Antarctic Depression, and are most intense S of the Great Australian Bight (Fig. 4). A favoured interpretation is that the Australian-Antarctic discordance and depression are maintained dynamically by an anomalously cool asthenosphere that rises slower than elsewhere (Forsyth *et al.*, 1987). As related later, the depression can be traced back to the Mesozoic. Also shown on Fig. 4 are the crustal-source magnetic anomalies measured by the MAGSAT satellite at a mean elevation of 400 km. They reflect structure at shallower depth (to the Curie isotherm) than the satellite FAA. The negative magnetic anomalies (interpreted as due to a combination of relatively low- intensity magnetised, hot, and thin crust) overlap the negative FAA near the continental margins, but the positive magnetic anomaly (relatively high intensity magnetisation, cold, and thick crust) cuts across the FAA. The trend of the magnetic anomalies on the continents reflects deep crustal structure, a topic to be discussed later.

All previous stages of the ocean can be constructed from the age-distribution of the present seafloor as modelled in Figure 5.

RECONSTRUCTION AT A13 = 35 Ma (early Oligocene)

This reconstruction (Fig. 6) is chosen to illustrate the ultimate geographical separation of Australia and Antarctica by the detachment of the South Tasman Rise from the conjugate part of Antarctica. Earlier spreading of the Southeast Indian Ocean on a N azimuth took place here equally as in the W, but the lithosphere of both continents remained juxtaposed along the many transform faults

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Fig. 1. Morphology of the Australian-Antarctic Depression. Australia shown by contours (km) of 1° x 1° area mean rock altitude (Wellman, 1979) to afford direct comparison with the ice-smoothed topography of Antarctica; Southeast Indian Ocean by the 3.5 and 4.0 km isobaths and the crest of the Southeast Indian Ocean Ridge (SEIR) and fracture zones (Circum-Pacific Map Project, 1981); and Wilkes Land by sub-ice bedrock contours (km) (Drewry, 1983). Residual depth anomalies (m) of the Southeast Indian Ocean (broken lines) from Watts & Daly (1981), interpolated by dotted lines to the isostatic continental values across southern Australia (Wellman, 1982). Axis of depression shown by double lines, subsidiary axes by single lines, and flank crests by wavy lines. Circles along axis indicate datum points along isochrons. Lambert equal-area projection. Modified from Veevers (1984). Dotted line locates profiles of Fig. 2. AB Aurora sub-ice basin, AL Adelie Land, CEL Central-eastern Lowlands, EH Eastern Highlands, GM Gamburtsev Mountains, GWP Great Western Plateau, SAH South Australian Highlands, STR South Tasman Rise, TAM Transantarctic Mountains, WB Wilkes sub-ice basin.

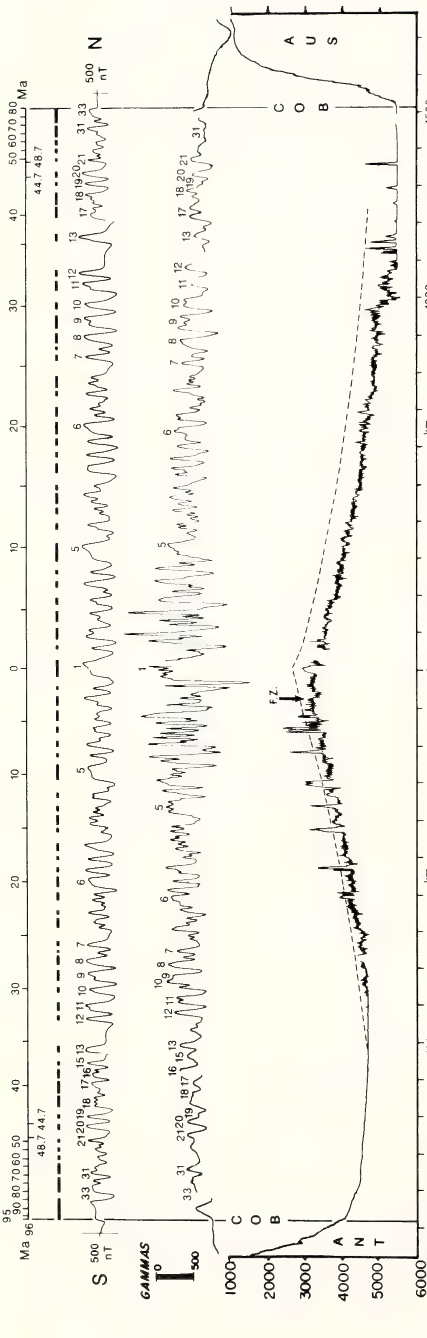

Fig. 2. *Below*: seafloor and magnetic anomalies at 132°E (Weissel & Hayes 1972), reproduced with the permission of *Nature* and Drs D.E. Hayes and J.K. Weissel. Location on Fig. 1. Broken line is empirical depth proportional to age$^{\frac{1}{2}}$ (Sclater *et al*. 1971), and the difference between it and the observed depth = depth anomaly. *Above*: synthetic anomalies from block model (App.). COB = continent-ocean boundary MT = magnetic trough.

until A18 (42 Ma). By 35 Ma the Southeast Indian Ocean was no more than 100 km wide at its narrowest part. By 30 Ma (late Oligocene), this narrow strait (and coincidentally the strait between South America and Antarctica) had opened wide enough to let the Circum-Antarctic Current flow through, so that Antarctica was insulated from the surface water of the world ocean resulting in refrigeration (Kennett *et al.*, 1975).

RECONSTRUCTION AT THE COB = 96 Ma (mid-Cretaceous)

A restored cross-section (Fig. 7) shows that the zone of extension from the Great Australian Bight to Wilkes Land was 600 km wide between the present coastlines. Two valleys lay within the zone of extension:

1. A 330 km wide valley lay between the magnetic troughs (MT).

2. A 45 km wide rift valley within an arch lay in the middle.

The suture, the point of separation by seafloor spreading, was the fault that bounded the N side of the rift valley. The longevity of the cross structure of the Australian-Antarctic Depression is shown by the exceptionally thick Cretaceous sediment that was deposited in the Ceduna depocentre of the Great Australian Bight and derived mainly from the initial uplift of the Eastern Highlands (Veevers, 1982). Closure of the Southeast Indian Ocean (Fig. 8) by overlaying the Antarctic COB on that of Australia was first done by matching the COB magnetic anomaly (Konig, 1981, 1987) and confirmed and extended (Veevers, 1987) by using seismic data (Eittreim & Smith, 1987). This provides a reconstruction of the eastern part of Gondwanaland at the point of breakup in the mid-Cretaceous within a broad suture zone of intracontinental extension that had developed since the mid-Jurassic. In the Great Australian Bight, the narrow Polda Trough branched eastward off the zone as did the much broader Bass Strait south of Cape Otway (C). The decisive change in the direction of the suture (S') from which the Southeast Indian Ocean grew - W instead of N of Tasmania - meant that Tasmania, the northern outpost of the Jurassic Gondwanaland dolerite, was detached at the last moment from the closely related Antarctica.

This stage has been reached by eliminating the seafloor spreading between the continents. To reconstruct the stage that preceded continental separation in the mid-Jurassic, the intracontinental extension must be eliminated. From the geometry of rotational normal faults in the top 10 km of the Bass Basin, Etheridge *et al.* (1984) calculated a NNE extension of 60-80% (beta = 1.6-1.8). The

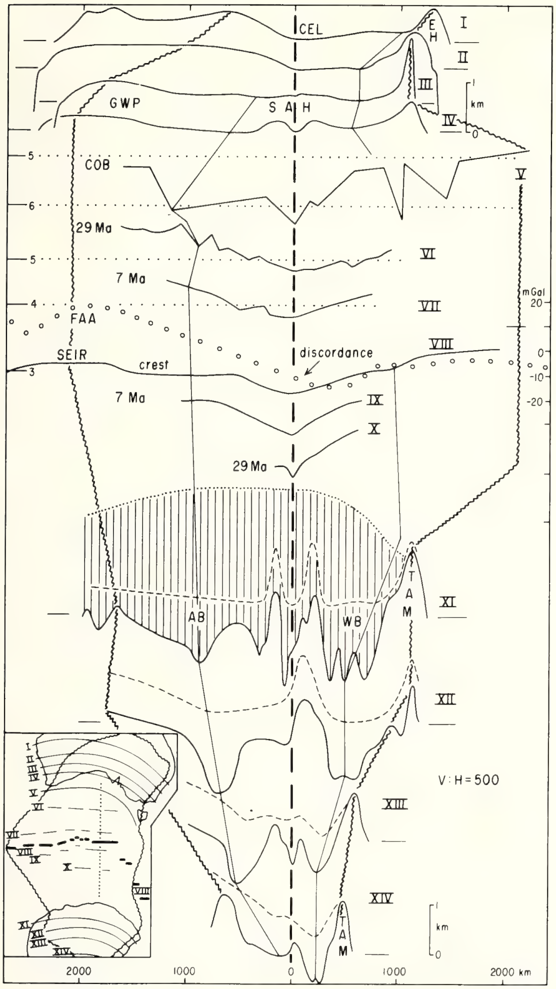

Fig. 3. Profiles of the seafloor and adjacent continents in a direction normal to spreading and aligned on the axis of the Australian-Antarctic Depression, located in inset. Abbreviations as in Fig. 1. Antarctic profile XI shows ice extending between the surface (dotted line) and bedrock (full line), and the isostatically unloaded bedrock (broken line) at its pre-ice elevation. Modified from Veevers (1984).

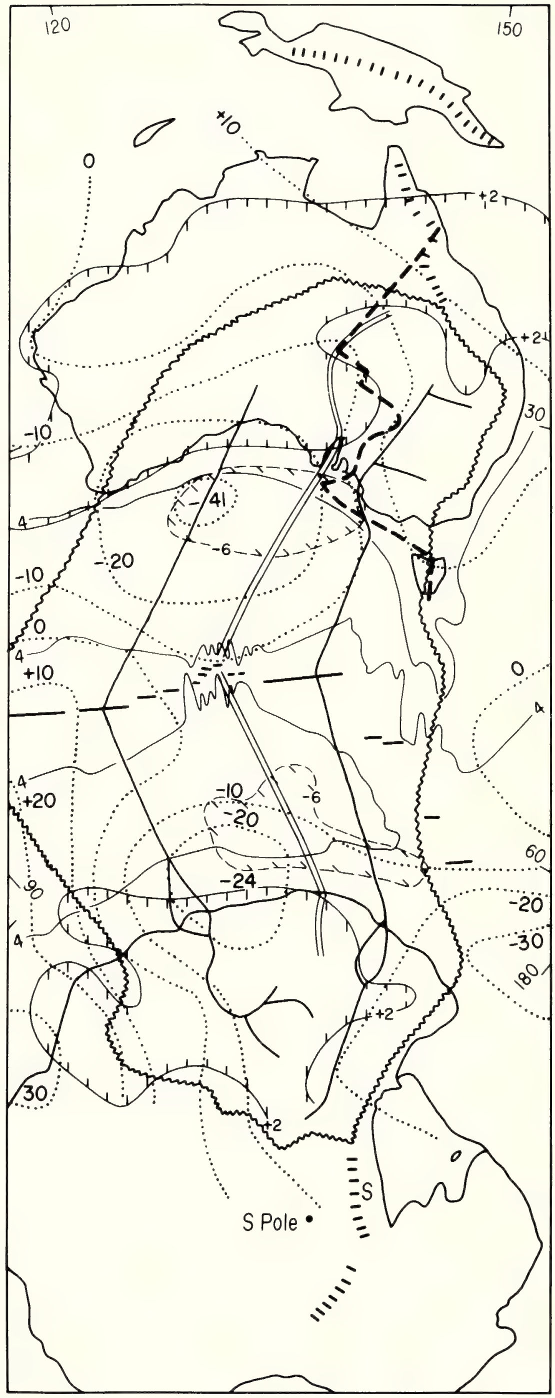

Fig. 4. Correspondence of the negative satellite free-air gravity anomalies (FAA) (dotted lines) with the Australian-Antarctic Depression. Gravity anomalies, in mGal, from Anderson *et al.* (1973) to 70°S, extended southward from Gaposhkin & Lambeck (1971). Modified from Veevers (1982, 1984) by the addition of selected crustal-source magnetic anomalies (+2 and -6 nT) from the MAGSAT Project, in Australia from Johnson & Mayhew (1985), in Antarctica from Ritzwoller & Bentley (1983). Heavy broken line is the Tasman Line (Veevers, 1984).

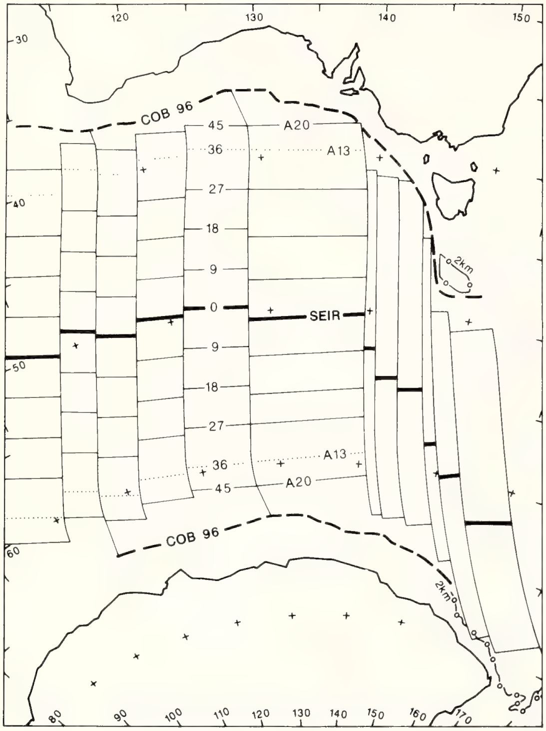

Fig. 5. Age distribution of the seafloor of the Southeast Indian Ocean, updated from Veevers (1984) by rotating the SEIR from a finite pole at 13.0°N, 31.5°E (Konig 1987) through 20.8°/2 north and south to A13 (36 Ma), and through increments of 20.8°/8 to model the intermediate isochrons. The wide spreading segment about 135°E was modelled by hand so that A20 and A13 have no offset on the west. Note presumed asymmetrical position of the SEIR east of 140°E.

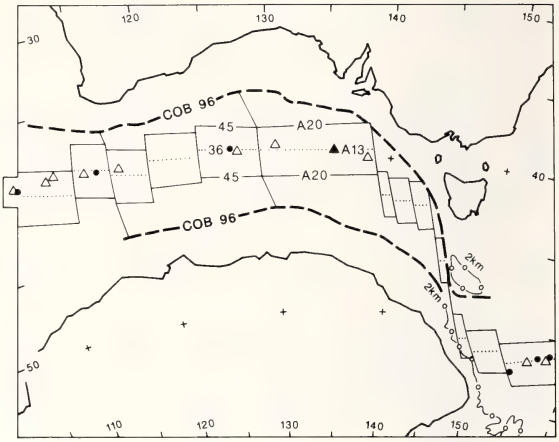

Fig. 6. Reconstruction at A13 = 36 Ma (early Oligocene) by superimposing the A13 anomalies (triangles on Australian Plate, filled circles on Antarctic Plate) by a rotation of Antarctica to Australia from a pole at 12.0°N 34.0°E (Konig 1987) by 20.8°, modified from Konig (1987).

continental crust beneath Bass Strait to the E, along 147°30′E, ranges from 15 to 22 km thick, as shown by a coast-to-coast seismic refraction survey (Johnson, 1973), so that its extension from a 35 km thick crust, as is found onshore, is beta = 1.5. This value is similar to that of the top 10 km of the Bass Basin, and suggests that crustal thickness provides a reliable estimate of the amount of extension. Estimates of the crustal thickness across the entire zone of extension are available between the Great Australian Bight and Wilkes Land (Fig. 7), and yield a beta = 1.8 to the S and beta = 1.9 to the N, equivalent to 360 km of extension on an azimuth of 035°, at right-angles to the main set of normal faults of the southern margin, and parallel to the transfer faults interpreted by Etheridge *et al.* (1984) in the Bass Basin.

RECONSTRUCTION OF GONDWANALAND = 160Ma (mid-Jurassic) and older

Fitting the zone of extension into a rectangular section of standard crustal thickness of 32 km along the estimated azimuth, as from A′ to B in Figure 7, yields the stage before the start of extension in the mid-Jurassic (160 Ma) (Fig. 9). This is the pristine configuration that had lasted from the origin of Gondwanaland at least 1000 Ma ago. The geological connections in the Gondwanaland stage, from younger to older, are as follows:

1. Early to mid-Jurassic (180-160 Ma) dolerite extends across the Transantarctic Mountains through northern Victoria Land to Tasmania, and peters out to the N in minor isolated bodies, exemplified by the Prospect Dolerite in western Sydney, New South Wales.

2. Permian/Triassic nonmarine quartzose sediments of northern Victoria Land (Collinson & Kemp, 1983) resemble those of mainland Australia (Victoria) (Douglas, 1976), all 'inboard' of the mixed marine and nonmarine sequence of Tasmania that lay closer to the Palaeo-Pacific margin.

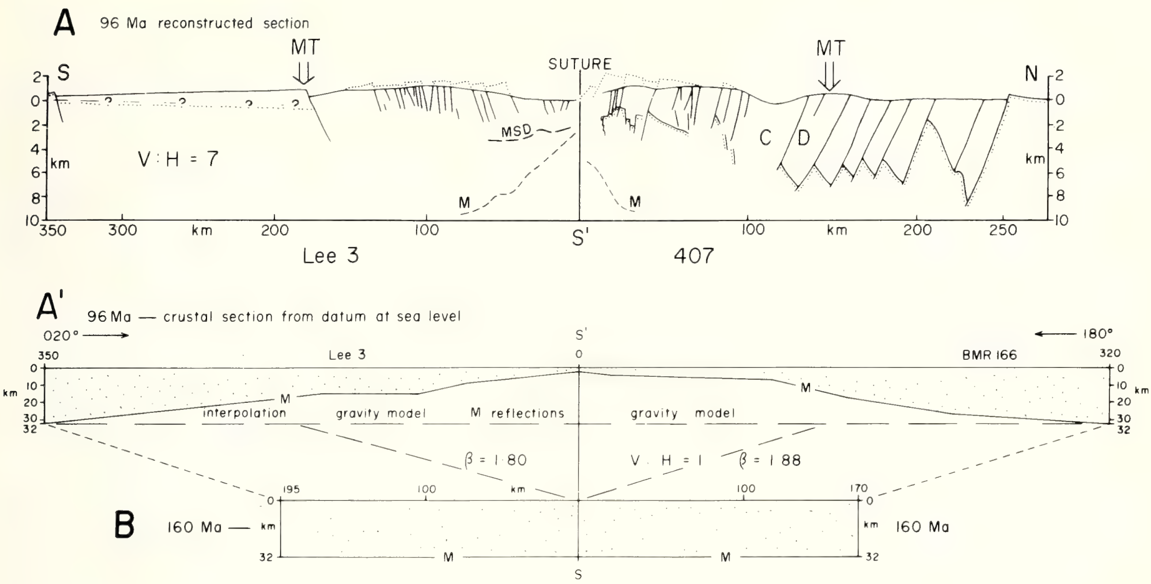

Fig. 7. A. Section of the restored suture zone at breakup (96 Ma) of the conjugate sections 407, projected on an azimuth of 180°, and *Lee* 3, both located in Fig. 8, after Veevers (1987), with the permission of the Circum-Pacific Council for Energy and Mineral Resources.
A'. Crustal layer sections after extension across the zone of extension (BMR 166, Great Australian Bight, and *Lee* 3, Wilkes Land margin, located in Fig. 8), drawn from a datum at sea level to facilitate measurement.
B, restored section of the crustal layer before extension, at 160 Ma, assuming a constant volume of crust.

3. End-Devonian (360 ± 5 Ma) granitoids in northern Victoria Land extend through Tasmania to central Victoria.

4. Ordovician (500-450 Ma) granitoids extend across the Transantarctic Mountains through northern Victoria Land to Tasmania (Dove River Granite) and reappear to the NW in western Victoria and southeastern South Australia.

5. The Middle Cambrian-Early Ordovician (Shergold *et al.*, 1985) Bowers Trough of northern Victoria Land is aligned with the Dundas Trough of Tasmania. The popularity of reconstructions based on the alignment of these structures has waxed and waned (Jago, 1981). Here their alignment is derived from an independent reconstruction.

6. The easternmost known occurrences of Archean rocks in Antarctica and Australia (Oliver *et al.*, 1983) lie on a N-trending sinusoidal line.

DISCUSSION

I mentioned the similar shape in Australia and Antarctica of the MAGSAT positive anomaly (Figs. 4, 9). In Australia, the +2 and +4 nT contours coincide with the Archean/Proterozoic boundary of the Yilgarn/Albany Province, and past the Great Australian Bight curve northward E of Spencer Gulf between the Gawler Block and the Precambrian and Phanerozoic boundary of the Tasman Line. In Antarctica, the +2 and +4 nT contours likewise curve southward past 135°E and, ignoring the eastward lobe of the +2 nT contour, are the mirror-image of the equivalent contours in Australia. Regardless of the precise geological boundary traced by the contours - it could parallel the Archean/Proterozoic boundary - the eastern trace of this boundary diverges from the reconstruction suture at about 140°E. The parallelism of the boundaries to the W signifies that the suture is situated within a Proterozoic fold belt, represented by exposures of the Albany Province. These exposures trend W to E between Archean cratonic nuclei - the Yilgarn and Gawler Blocks in Australia,

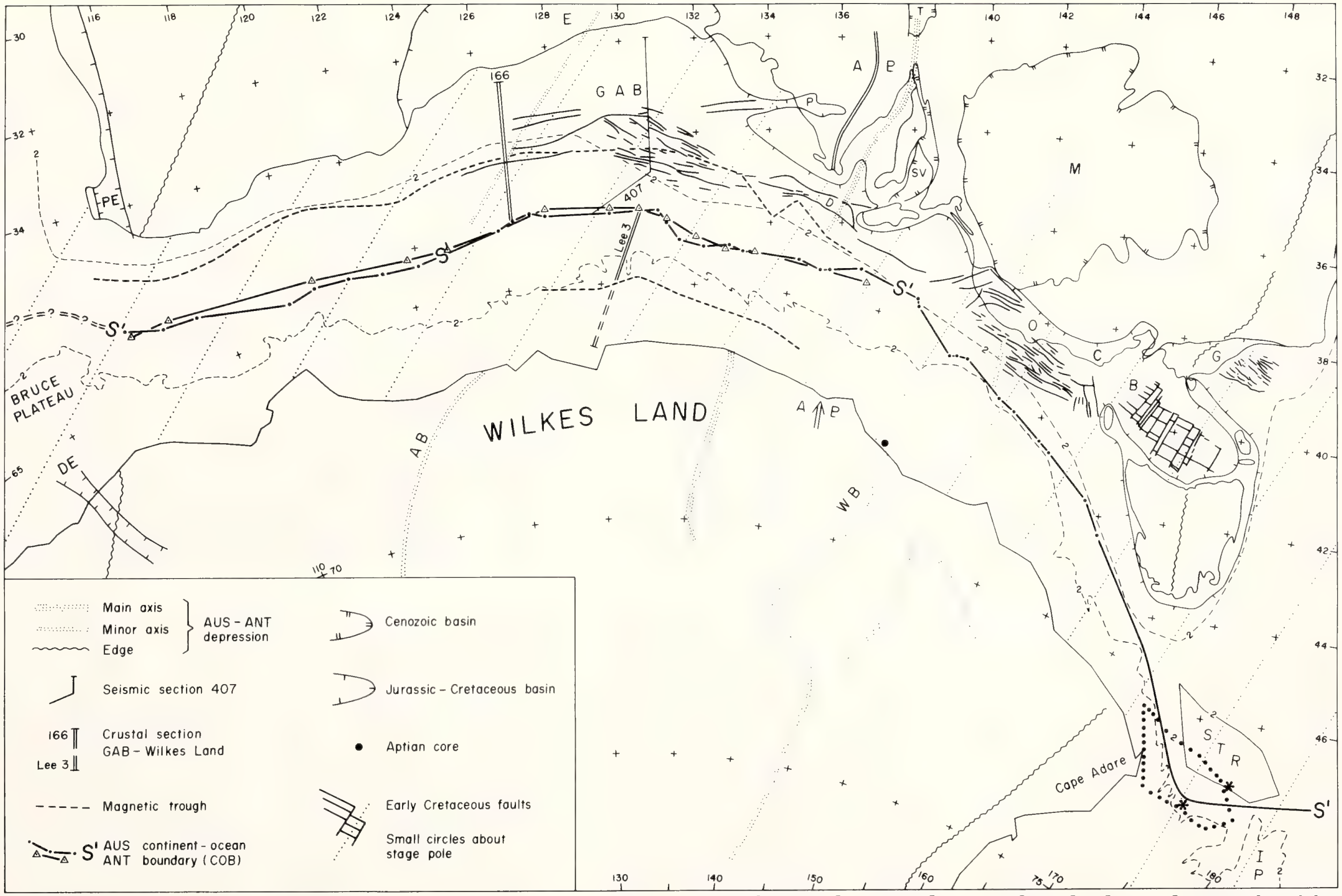

Fig. 8. Closure of the Southeast Indian Ocean at 96 Ma (by 27.85° about 1.5°N, 37.0°E). Australia with 2° x 2° grid, Antarctica with 5° x 5° grid, present coastlines for reference only. Modified and extended from Veevers (1987) (with the permission of the Council for Energy and Mineral Resources) and Veevers & Eittreim (in prep.). The suture, S', is indicated by the Australian continent-ocean boundary (COB), matched between ANT 127° and 144°E by the COB of the Wilkes Land margin determined from seismic and magnetic profiles. Early Cretaceous normal faults in Great Australian Bight Basin (GAB), Polda Trough (P), Duntroon Basin (D), Otway Basin (O), and Gippsland Basin (G) from references in Veevers (1987); and in Bass Basin (B), with in addition the orthogonal set of transfer faults, from Etheridge *et al.* (1984). Small circles about extension pole (42.03°N, 40.58°E) drawn from coast to coast across zone of extension. South Tasman Rise (STR) rotated by hand to remove the overlap shown by the dotted line (star indicates Deep Sea Drilling Project (DSDP) site 281). Archean/Proterozoic boundary from Oliver *et al.*(1983), Aptian core, Domack *et al.* (1980), and Australian-Antarctic Depression, Veevers (1982). AB - Aurora sub-ice basin, DE - Denman Glacier rift zone (Masolov *et al.*, 1981), E - Eucla Basin, IP - Iselin Plateau, M - Murray Basin, PE - Perth Basin, SV - St Vincent Basin, T - Torrens Basin, WB - Wilkes sub-ice basin.

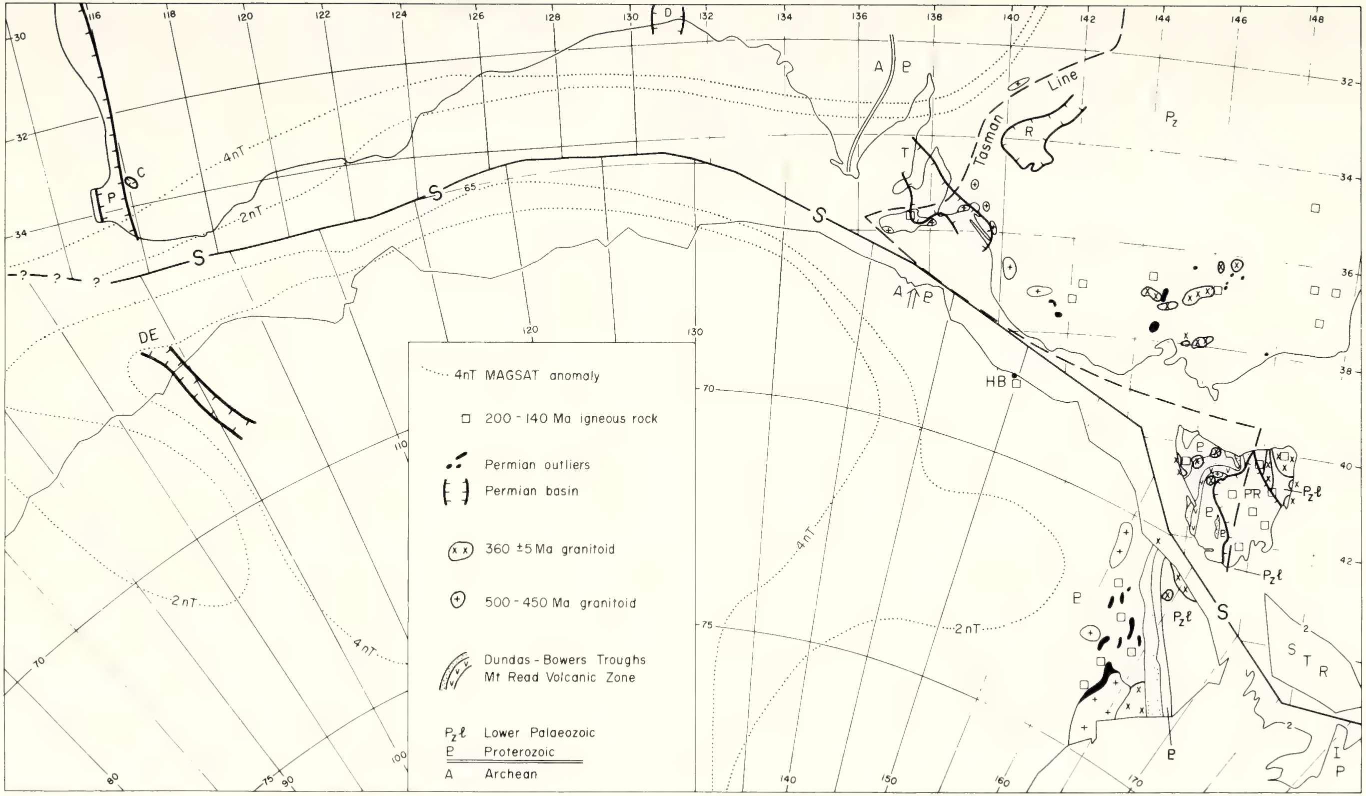

Fig. 9. Closure of the rift system at 160 Ma, modified and extended from Veevers (1987), with the permission of the Circum-Pacific Council for Energy and Mineral Resources. The suture, S, is approximated west of AUS 136°E by the present position of the Australian and Antarctic magnetic troughs, east of AUS 136°E to Tasmania by a line between the present 2-km isobaths and the coast, and south of Tasmania by the line between the reconstructed 2 km isobath of the South Tasman Rise (STR) and the Iselin Plateau (IP). Tasmania constituted a third plate between 160 and 96 Ma, and rotated 133 km from coast to coast by crustal extension beneath Bass Strait on an azimuth of 027°. The South Tasman Rise constituted a fourth plate and has been rotated by hand by 166 km toward Tasmania on an azimuth of 027° so that it lies alongside the Iselin Plateau. Dundas Trough and Mt Read Volcanic Zone from Corbett *et al.*(1974), Bowers Trough from Laird & Bradshaw (1983), inferred rift zone at Denman (DE) Glacier from Masolov *et al.* (1981), 360±5 Ma granitoids from Richards & Singleton (1981), Williams (1976), and Grindley & Oliver (1983). Selected MAGSAT anomalies from Johnson & Mayhew (1985) and Ritzwoller & Bentley (1983). Modified and extended from Veevers & Eittreim (in prep.). Australian Permian basins: C - Collie, D - Denman, P - Perth, PR - Tasmania, R - Renmark, T - Troubridge.

and the southernmost Prince Charles Mountains in Antarctica (Yoshida & Kizaki, 1983) - and change trend at about 140°E. The location of the breakup suture within the W-E Proterozoic fold belt can be attributed to tensile failure along the tectonic grain of an anisotropic fold belt wrapped around isotropic nuclei, as observed today in the East African Rift System (McConnell, 1974) and along the Early Cretaceous break-up suture of the South Atlantic (Kennedy, 1965). An interesting question is : what caused the suture zone to branch to the E? The branch into the Polda Trough is readily explained as an unsuccessful attempt at following the W-E trend into the Archean nucleus of the Gawler Block (cf. the Lake Tanganyika rift valley that ends blindly after penetrating 200 km into the N edge of the Zambia Block, and instead continues to the E within the Ubendian fold belt). The branch through Bass Strait parallels the Gambier-Beaconsfield Fracture Zone (Crawford & Campbell, 1973; Harrington *et al.*, 1973), and, in my view, represents a transform fault along which an eastern part of Gondwanaland diverged from Precambrian Australia in the Early Cambrian (Veevers & Powell in Veevers, 1984, fig. 182B). I note, in passing, that the Late Ordovician to Middle Devonian sinistral motions along the Gambier-Beaconsfield Fracture Zone postulated by Harrington *et al.* (1973) and adopted by Veevers (1976) seem now to be untenable in view of the fixing of the Archean and Proterozoic boundary at similar longitudes in Australia and Antarctica, and the interpretation of the Gambier-Beaconsfield Fracture Zone as an Early Cambrian structure. This part of the Tasman Line could have acted as a guide for tensile stress. Unlike the SE branch, it failed to develop into an ocean probably because the Late Proterozoic to Jurassic northerly trend of Tasmania and northern Victoria Land deflected the suture southward to pass between the South Tasman Rise/New Zealand Plateau and Western Antarctica.

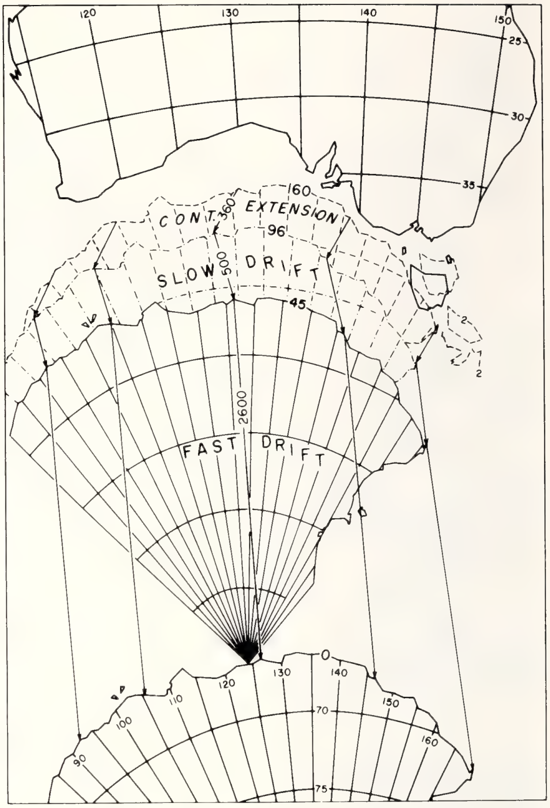

Fig. 10. 3-stage separation of Antarctica from mainland Australia, from the initial position at 160 Ma, including that of Tasmania, the South Tasman Rise, and the Ross Sea margin (2-km isobath), all shown by the broken line, given by a rotation of 30.64° about 5.68°N, 38.36°E, through the stage of continental extension (3.564° about 42.03°N, 40.58°E) to 96 Ma (dot-and-dashed line), with Tasmania (full line) in present position, through the stage of slow drift (6.82° about 35.74°S, 73.85°E) to 45 Ma, and finally through the stage of fast drift (24.10° about 13.0°N, 31.5°E)(Konig, 1980) to the present. Arrows indicate the azimuth of separation, and the arrow about AUS 130°E is marked with numerals that indicate the amount (km) of separation. Lambert equal-area projection, with 5° x 5° grid drawn to coastlines. Modified from Veevers & Eittreim (in prep.).

SUMMARY

During the long history of Eastern Gondwanaland, the parts which later became mainland Australia (Fig. 10, full line), Tasmania, the South Tasman Rise, and Antarctica (broken line, 160 Ma) constituted an area of continental lithosphere as shown in Figure 10. The tight reconstruction of Sproll & Dietz (1969) achieved by fitting the 1000-fathom isobaths and that of Smith & Hallam (1970) (1000 m isobaths) approximate the pre-extension reconstruction but have Antarctica 200 km too far to the W of Australia; while the loose reconstruction of Weissel *et al.* (1977) places Antarctica midway between the pre-extension and breakup positions. From the mid-Jurassic (160 Ma) to mid-Cretaceous (96 Ma), mainly in the Early Cretaceous, Australia

separated from Antarctica by 360 km of extension in a zone that branched eastward into the narrow Polda Trough and the wide Bass Strait. In the mid-Cretaceous (96 Ma), continental extension was replaced by seafloor spreading of the Southeast Indian Ocean from a suture that passed eastward along the middle of the zone of extension to the longitude of Adelaide and then southeastward south of Tasmania/South Tasman Rise. As a result Bass Strait became a failed arm. The drift of Antarctica away from Australia was so slow that only 500 km of seafloor had been generated by the middle Eocene (45 Ma), and the continents remained juxtaposed along the transform faults W of Tasmania. A phase of fast drift followed so that five times as much seafloor was generated in the last 45 Ma as had been generated in the first 50 Ma. In the process, Australia and Antarctica finally separated at about 35 Ma and allowed the Circum-Antarctic Current to start circulating. For convenience, I have shown Antarctica moving away from Australia. Continental palaeomagnetism (Embleton, 1981), however, suggests that relative to the spin axis of the earth Australia did most of the moving northward while Antarctica remained in high latitudes. In the process Australia has cut short its brief existence as an independent continent by colliding with the Southeast Asian island arcs and will soon amalgamate with the continental lithosphere of Asia, as India, its previous neighbour, did 50 Ma ago (Powell & Johnson, 1980).

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APPENDIX

PARAMETERS OF MAGNETIC BLOCK MODEL OF FIGURE 2

Magnetic reversal time scale and block model (black is normal polarity, clear is reversed polarity) from Berggren *et al.* (1985) and Kent & Gradstein (1985).

Source depth	5.5 to 6.0 km
Trend	090°
Remanent magnetisation	0.007 emu/cm^3
I_0	-70°
D_0	10°
I_r	-74°
D_r	0°

[continued overleaf]

Spreading rate (cm/year)

0 - 36 Ma	2.9	ANT
	3.25	AUS
36 - 44.7 Ma	2.2	ANT
	1.7	AUS

(this lower rate is due to the absence of A15-A16, 37.0-39.5 Ma, presumably at an undetected fracture zone)

44.7 - 48.7 Ma	1.0	ANT
	1.0	AUS
48.7 - 96 Ma	0.45	ANT
	0.45	AUS

J.J. Veevers
School of Earth Sciences
Macquarie University
N.S.W. 2109 Australia

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Potassium-Argon Ages, Petrology and Geochemistry of some Mesozoic Igneous Rocks in Northeastern New South Wales

J. A. DULHUNTY, E. A. K. MIDDLEMOST AND R. W. BECK

ABSTRACT. Twelve specimens of volcanic and subvolcanic rocks were collected from outliers in the Wellington-Muswellbrook-Narrabri region of northeastern N.S.W. They were originally assumed to be of Tertiary age as they occurred in close field association with volcanic rocks of established Tertiary age. However, anomalies were perceived as to their possible misconceived age relative to ages of erosion surfaces onto which they were extruded, and sediments into which they were intruded. K-Ar dating proved all specimens collected to be of Mesozoic age, and the establishment of high-level Mesozoic erosion surfaces, previously regarded as Tertiary, has simplified some aspects of landscape development in northeastern N.S.W.

The 12 specimens are all silica undersaturated alkaline rocks that have many geochemical, mineralogical and petrographic similarities. The more primative basanites, alkali basalts, hawaiites and tephrites all plot as a coherent group in the "within-plate" field on the Ti-Y-Zr discriminant diagram. Spidergrams illustrate the broad chemical similarities among the least differentiated specimens. This study augments the concept that there was widespread alkaline volcanism in northeastern N.S.W. over an extensive period during the Mesozoic.

INTRODUCTION

Erosional remnants of volcanic flows and shallow intrusive rocks occur extensively in eastern N.S.W. Prior to the availability of radiometric dating, many geologists assumed they were of Tertiary age based on geomorphological misinterpretation and their close resemblence to volcanic rocks of established Tertiary age. K-Ar dating has since confirmed Tertiary ages for many occurrences (Dulhunty, 1971, 1972, 1973; Wellman & McDougall, 1974), however it has also revealed that many occurrences previously assumed to be Tertiary, are of Mesozoic age (Embleton et al, 1985; McDougall & Wellman, 1976; Dulhunty & McDougall, 1966; Dulhunty, 1967, 1972, 1986).

During the Mesozoic the Gondwana supercontinent broke up and the Australian continent assumed its present form. In eastern Australia this was an era of widespread volcanic and subvolcanic activity. In order to increase our knowledge of the abundance, distribution and character of volcanic activity during the Mesozoic, 12 specimens suitable for K-Ar dating were collected at grid reference points given in the following section.

Field investigations and collection of specimens were carried out by Dulhunty. Petrological investigations and electron microprobe studies were undertaken by Middlemost, and chemical analyses were carried out by Beck. Whole-rock trace element analyses were executed by the Analytical Group of the N.S.W. Department of Mineral Resources. K-Ar dating was done at Geochron Laboratories, Cambridge, U.S.A., between 1971 and 1978 before the present decay and abundance constants came into general use (Steiger and Jager, 1977). Table 1 shows Geochron analytical results, and ages converted to new I.U.G.S. constants. The dating was carried out more than a decade ago and reproducability of analytical results may in some cases appear rather poor; however, the data served adequately to distinguish between Mesozoic and Tertiary volcanic and subvolcanic rocks, which was the purpose for which the dating was done. The authors regard the measured K-Ar ages as providing good estimates of the time since crystalliztion and cooling of the rocks.

The 12 specimens were selected for K-Ar dating on the basis of crystallinity and lack of alteration due to either deuteric effects or atmospheric weathering, or both. They were collected from occurrences of doubtful age, that is they were either Mesozoic or Tertiary. All were fresh, showing only traces of feldspar alteration, and contained less than about 7% of glass or poorly crystallised material as revealed by microscopic examination of thin-sections.

FIELD OCCURRENCES

The 12 specimens selected for investigation were as follows:

SPECIMEN K 18, Mt Bodangora, phonotephrite, age 148 $\pm$6 Ma, Fig. 1 map site 1, Dubbo Sheet SI 55-4, Grid 285989, Lat. 32.447^{O}S Long. 149. 149.003^{O}E, collected at Bodangora Trig - the highest point on the upper flow.

SPECIMEN K19, Mt. Dangar, tephrite, age 194 $\pm$7 Ma, Fig. 1 map site 2, Singleton Sheet SI 56-1, grid 347002, Lat. 32.337^{O}S Long. 150.487^{O}E, collected at Dangar Trig - the highest point on the upper tephrite mass.

SPECIMEN K 20, Mt. Bodangora, phonotephrite, age 182 $\pm$7 Ma, Fig. 1 map site 1, Dubbo Sheet SI 55-4, grid 285989, Lat. 32.447^{O}S Long. 149.003^{O}E collected 5 m above base of lower flow on north face of outlier.

SPECIMEN K 21, Mt Dangar, phonotephrite, age 206 $\pm$8 Ma, Fig. 1 map site 2, Singleton Sheet SI 56 - 1, grid 347002, Lat. 32.337^{O}S Long. 150.487^{O}E collected 1.2 m above base of lower phonotephrite mass within contact-cooled margin.

SPECIMEN K 35, Mt Binalong, basanite, age 148 $\pm$6 Ma, Fig. 1 map site 3, Manilla Sheet SH - 9, grid 297188, Lat. 30.795^{O}S Long. 150.013^{O}E, collected at south end of upper flow 20 m below level of Binalong Trig.

SPECIMEN K 38, Mt. Binalong, hawaiite, age 119 $\pm$8 Ma, Fig. 1 map site 3, Manilla Sheet SH 56 - 9, grid 297188, Lat. 30.795^{O}S Long. 150.013^{O}E, collected from middle of sill at south face of Mt Binalong.

SPECIMEN K 36, Nerada, alkai basalt, age 145 $\pm$9 Ma, Fig. 1 map site 4, Narrabri Sheet SH 55 - 12, grid 291187, Lat. 32.804^{O}S Long. 149.953^{O}E, collected on Travelling Stock Reserve 300 m north from Nerada boundary fence and 700 m west of Coxs Creek.

SPECIMEN K 34, Boonoomarah Ridge, tephrite, age 198 $\pm$8 Ma, Fig 1 map site 5, Narrabri Sheet SH 55 - 12, grid 284175, Lat. 30.901^{O}S Long. 149.882^{O}E, collected 400 m east of road on western side of ridge at its southern end.

SPECIMEN K 32, Mt. Talbareeya, trachyte, age 170 $\pm$6 Ma, Fig. 1 map site 6, Gilgandra Sheet SH 55 - 16 grid 275137, Lat. 31.629^{O}S Long. 149.804^{O} E, a rock core drilled from surface outcrop 152 m above road-level on NNW face of Mt. Talbareeya.

SPECIMEN K 37, Emerald Hill, hawaiite, age 183 $\pm$8 Ma, Fig. 1 map site 7, Manilla Sheet SH 56 - 9, grid 304179, Lat. 30.869^{O}S Long. 150.066^{O}E, collected 0.3 km east of Monbrook Homestead.

SPECIMEN K 33, Gunnedah West, alkali basalt, age 189 $\pm$19 Ma, Fig., 1 map site 8, Tamworth Sheet SH 56 - 13, grid 304162, Lat. 31.018^{O}S Long. 150.047^{O}E, collected on north side of Coonabarabran road at 18 km from Gunnedah.

SPECIMEN K 39, Namoi River, hawaiite, age 217 $\pm$9 Ma, Fig. 1 map site 9, Narrabri Sheet SH 55 - 12, grid 287226, Lat. 30.487^{O}S Long. 149.947^{O}E, collected on northeast side of roadside outcrop 500 m west of Namoi River and 3 km east of Belah Park Homestead.

RADIOMETRIC DATING AND FIELD INTERPRETATIONS

Results of K-Ar dating of the 12 specimens investigated, are set out in Table 1 which shows that all are of Mesozoic age. Numerical Time Scale by Van Eysinga, F.W.B. (1975). Geological Time Scale, Elservir Sci. Publ. Co., Amsterdam, The Netherlands. Triassic 230-195 Ma, Jurassic 195-141 Ma and Cretaceous 141-65 Ma.
Specimens K33, K34, K36, K37 and K39 were collected from separate low-profile surface outcrops at map sites 8,5,4,7 and 9 respectively in Fig. 1. They were partly concealed by Quaternary weathering products which obscured their relations with preexisting country rocks. Specimens were collected from the 5 outcrops as they were regarded as being representatives of numerous similar occurrences in the Gunnedah-Narrabri district, which may also be of Mesozoic age.

Rock specimen K 32 was drilled on Mt. Talbareeya which consists wholly of trachyte. It was collected by Dr Judy Bean, New England University, Armidale, and Phillipe Schmidt, Australian National University, Canberra, for petrogrpahic studies (Bean 1975) and donated by them for the present investigation.

Two rock specimens were collected from each of three prominent topographical features at map sites 1, 2, and 3 (Fig 1). They were Mt. Bodangora (specs. K 18 & K 20), Mt. Dangar (specs. K 19 & K 21) and Mt. Binalong (specs. K 35 & K 38). Field studies were made at each locality and their modes of occurence are illustrated by sketch sections in Fig. 1.

Mt. Bodangora on the northwestern side of the Wellington-Gulgong road, 24 km from Wellington, rises to 750 m above sea level, and 150 m above surrounding country (Fig. 1) It is capped by 35 m of Mesozoic phonotephrite in the form of 2 almost horizontal flows. The lower is 15 m thick with an age of 182 $\pm$7 Ma, and the upper is 20 m thick with an age of 148 $\pm$6 Ma. This age difference could represent an interval of 34 Ma between the extrusion of the 2 flows.

The lower flow lies on 20 m of

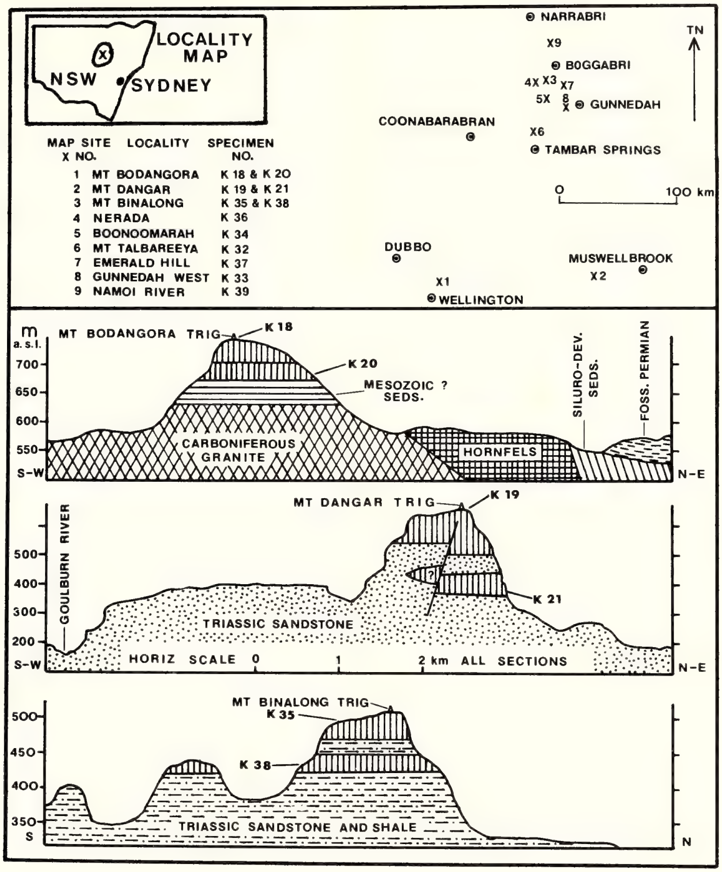


Figure 1. Map sites of selected igneous rock outcrops, and sketch sections showing occurrences at Mts. Bodangora, Dangar and Binalong. Latitude and Longitude for each sample site are given under Field Occurrence.

sediment with unidentifiable fragmentary plant remains but no determinative fossils. This in turn rests upon an almost horizontal erosion surface cut into Carboniferous granite. The sediments were previously believed by the author (J.A.D.) to be of Permian age due mainly to the occurrence, 4 km to the northeast, of fossiliferous Permian sediments resting unconformably on a basement of metamorphic rocks (Fig. 1). From studies of the lithology and indeterminative fossil content of the Mt. Bodangora sediments, it has now been concluded that they are probably equivalent to Triassic sediments underlying Mesozoic volcanics in the Binnaway-Gunnedah-Narrabri region to the north and northeast.

The ages of the lower phonotephrite flow and the underlying sediments, are significant in geomorphological deductions regarding the ages of erosion surfaces with which each is associated. Previously, when the flow was regarded as Tertiary, and the underlying sediments as Permian, the erosion surfaces beneath each appeared to be early Tertiary and mid Permian, leaving only the first half of the Permian time for the surface exposure of the Carboniferous granite. It now appears that the volcanic flow was extruded onto an erosion surface formed during early Triassic time, and that the underlying sediments were deposited on an erosion surface probably formed during late Triassic time. This would leave the whole of Permian and most of Triassic time for exposure of the Carboniferous granite.

Mt. Dangar, 7.5 km W of Sandy Hollow village and 2.5 km north of the Goulburn River, is a high prominent topographical feature capped by tephrite and phonotephrite. K-Ar dating has now established Mesozoic ages varying from

194 $\pm$7 Ma at the top to 206 $\pm$8 Ma at the lowest point. It is a steepsided outlier of igneous rock and Triassic sandstone standing some 300 m above the surrounding sandstone plateau surface. The steep sides of the outlier are largely covered by slumped rock masses and scree slopes of sandstone and igneous rock which obscure the exact relations between the two.

There would appear to be two masses of volcanic or subvolcanic rock, one above (tephrite) and one below (phonotephrite), separated by about 100 m of sandstone (Fig. 1). There is evidence of faulting and what appears to be a cooled finer-grained margin extending about 1.5 m up from the underlying sandstone at the base of the lower mass of igneous rock, suggesting that it may be a sill. No outcrop of the top of the lower mass, or the base of the upper mass could be found. Although the form of the occurrence was uncertian, specimens K 19 and K 21 were collected from the highest and lowest places where suitable igneous rock cropped out (Fig 1). The K-Ar dates for the two specimens were so close that they could be of the same Jurassic age. If the upper mass of tephrite is a flow, it must have been extruded onto a surface of late Triassic sands early in Jurassic time, or if intrusive it must have been intruded into late Triassic sands. The K-Ar result that the age of Mt. Dangar igneous rock is early Jurassic, and not Tertiary, is of considerable significance in relation to geomorphological history of the area (see Fig. 1).

Mt. Binalong, another high prominent topographical feature, 10 km S 15^{O} W of Boggabri, stands some 225 m above the general level of surrounding country. It is capped by an erosional remnant of basanite (K 35) lying on Triassic sandstone and shale in which a sill of hawaiite (K 38) occurs some 25 m below the base of the basanite. An occurrence of sandy wash beneath the northern end of the basanite indicates that it is a flow extruded over an upper Jurassic erosion surface on Triassic sediments. Until recently both igneous rocks were regarded as of Tertiary age by N.S.W. geologists, but K-Ar dates of 148 $\pm$6 Ma for the flow, and 119 $\pm$8 Ma for the sill have now established them as being Mesozoic in age. If the basanite flow was regarded as Tertiary, the height of the erosion surface at its base, about 180 m above the surrounding country, suggested a Tertiary profile relief of at least 180 m. However the Mesozoic age for the flow means that the erosion surface over which it flowed was of late Jurassic age and of no significance in relation to Tertiary topography.

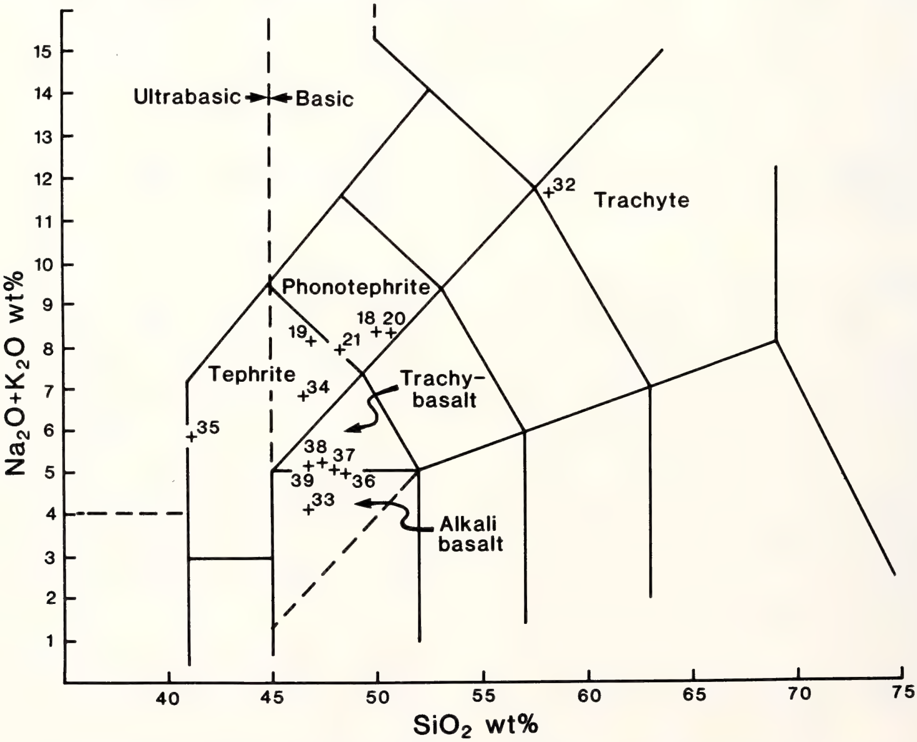


Figure 2. A total alkali-silica diagram used to classify the rocks studied (after Le Bas et al. 1986: 747). The datapoints are numbered in the same manner as Table 2.

PETROLOGY

As there is little detailed quantitative geochemical and petrographic information on the Mesozoic volcanic rocks of northeastern N.S.W. this seciton attempts to rectify the deficiency. According to the TAS system of volcanic rock classification (Le Bas et al. 1986: 747; & Fig. 2) the specimens consist of 1 basanite, 2 alkali basalts, 3 hawaiites, 2 tephrites, 3 phonotephrites and 1 trachyte.

The basanite (Table 2, No. 35) not only contains 19% normative nepheline, but it also contains 5.0% normative leucite and 21.3% normative olivine (See Figure 3). There is no simple relationship between the composition of these rocks and their age, except that the two tephrites are of similar age. Although they are of disparate age the alkali basalts and hawaiites form a geochmically coherent group in both their major and trace element abundances (See Table 2 and Figures 2 and 4). Figure 5 shows that the data-points for these essentially unaltered alkali basalts, hawaiites, basanite and tephrites all plot as a coherent group in the "within-plate" field on the Ti/100 - Zr - 3Y diagram (Pearce & Cann 1973:295). The spidergrams (Figures 6 and 7) illustrate the broad geochemical similarities among the least differentiated specimens, and they also demonstrate the geochemical affinities, between these Mesozoic rocks and selected Tertiary lavas from Eastern Australia. The present study has shown that there is no simple geochemical test that is able to separate the Mesozoic and Tertiary basanites, tephrites and alkali basalts of N.S.W.

BASANITE

The specimen with the lowest Thornton and Tuttle (1960) differentiation index (D.I = 27.8) is the basanite from Mt. Binalong (Table 2, No 35). It is a porphyritic rock with large phenocrysts of olivine set in a fine-grained groundmass essentially composed of clinopyroxene, nepheline, olivine, Fe-Ti oxides and apatite. Most of the olivine ranges in composition between Fo_{69} and Fo_{74}, with a mean of Fo_{72}, and it contains $\bar{x}$ MnO = 0.59% and $\bar{x}$ CaO = 0.77% (See Table 4 No. 3). The pale brownish pink clinopyroxenes are all high-Ca titansalites (See Figure 8.) Their mean composition is $En_{36.5}$ $Fs_{12.1}$ $Wo_{51.4}$, and they carry $\bar{x}$ TiO_2 = 2.80%, $\bar{x}$ Al_2O_3 = 6.11% and $\bar{x}$ Na_2O = 0.61% (see Table 3, Nos. 6 and 7). The nepheline is abundant (30%) and usually occurs as an irregular shaped phase in the groundmass. It contains $\bar{x}$ Na_2O = 15.12%, $\bar{x}$ K_2O=5.23%, $\bar{x}$ CaO = 1.33%, and $\bar{x}$ Fe_2O_3 = 0.85%. All of these nephelines ideally contain more silicon and less aluminium that is represented by the formula $NaAlSiO_4$ (i.e. $\bar{x}$ Si = 8.479, $\bar{x}$ Al = 7.430). The Fe-Ti oxides are mainly titanomagnetities that contain $\bar{x}$ TiO_2 = 22.43%, $\bar{x}$ MgO = 2.68%, $\bar{x}$ Al_2O_3 = 1.93%, and $\bar{x}$ Cr_2O_3 = 0.36%.

ALKALI BASALT. The next most primitive specimen is the alkali basalt (D.I. = 30.6) from Gunnedah West (Table 2, No. 33). This specimen is a medium-grained basalt with an ophitic texture. Labradorite (x $Or_{1.9}$ $Ab_{40.4}$ $An_{57.7}$) is the dominant phase, but some smaller anhedral crystals of sanidine (x $Or_{50.5}$ $Ab_{44.5}$ An_{50}) were also analysed (see Figure 2.8). Olivine makes up about 20% of the volume of the rock and most of these olivines have compositions of between Fo_{69} and Fo_{73} and are thus close to their mean value of Fo_{71} (See Table 4, No 2). They also carry $\bar{x}$ MnO = 0.36%, $\bar{x}$ CaO = 0.36%, and $\bar{x}$ Cr_2O_3 = 0.03%. Many of the large olivines are partly altered to fine grained aggregates of chlorite, goethite and hematite. The large pale brownish pink clinopyroxenes consist of approximately equal proportions of titansalites and titaniferous salites (See Table 3 Nos 2 and 4 and Figure 8). Their mean composition is $En_{38.1}$ $Fs_{14.5}$ $Wo_{47.4}$, and they carry $\bar{x}$ TiO_2 = 2.30%, $\bar{x}$ Al_2O_3 = 3.72% and $\bar{x}$ Na_2O = 0.48%. The rock also contains approximately 1.5% biotite which usually occurs in small elongate crystals in close association with the Fe-Ti oxides. It is a titaniferous biotite that carries $\bar{x}$ TiO_2 = 6.36%, together with $\bar{x}$ Na_2O = 0.74% and $\bar{x}$ BaO = 0.10%. The rock contains approximately 3% analcime, and it was found to be relatively pure as it carries only $\bar{x}$ CaO = 0.17% and $\bar{x}$ K_2O = 0.15%. A variety of different Fe-Ti oxides occur. Ilmenites ($\bar{x}$ TiO_2 = 51.3%) and titanomagnetites ($\bar{x}$ TiO_2 = 14.1%) coexist in the groundmass, whereas titanomagnetites that contain significantly more Cr_2O_3 ($\bar{x}$ 10.7%), Al_2O_3 ($\bar{x}$ 7.4%) and MgO ($\bar{x}$ 4.5%) occur included within the olivines. A little interstitial zeolite, mainly with the chemical composition of scolecite, was observed.

The other alkali basalt is from Nerada (Table 2, No 36). It is porphyritic with large olivine phenocrysts set in a fine-grained groundmass of feldspar, clinopyroxene, olivine, Fe-Ti oxides, analcime and apatite. It has a differentiation index of 35.7 and carries 3.5% normative nepheline. The olivine is variable in composition (Fo_{49} to Fo_{65}) with a mean of Fo_{61}. This mineral carries $\bar{x}$ MnO = 0.62% and $\bar{x}$ CaO = 0.43% (See Table 4, No 4). Feldspar makes up over half of the rock (approx. 54 vol. %). It consists mainly of labradorite ($\bar{x}$ $Or_{1.6}$ $Ab_{35.1}$ $An_{63.3}$), but there is some 5 vol. % of smaller anhedral crystals of sanidine. The clinopyroxenes range in composition from pale brownish pink titaniferous salites to titansalite. Their mean

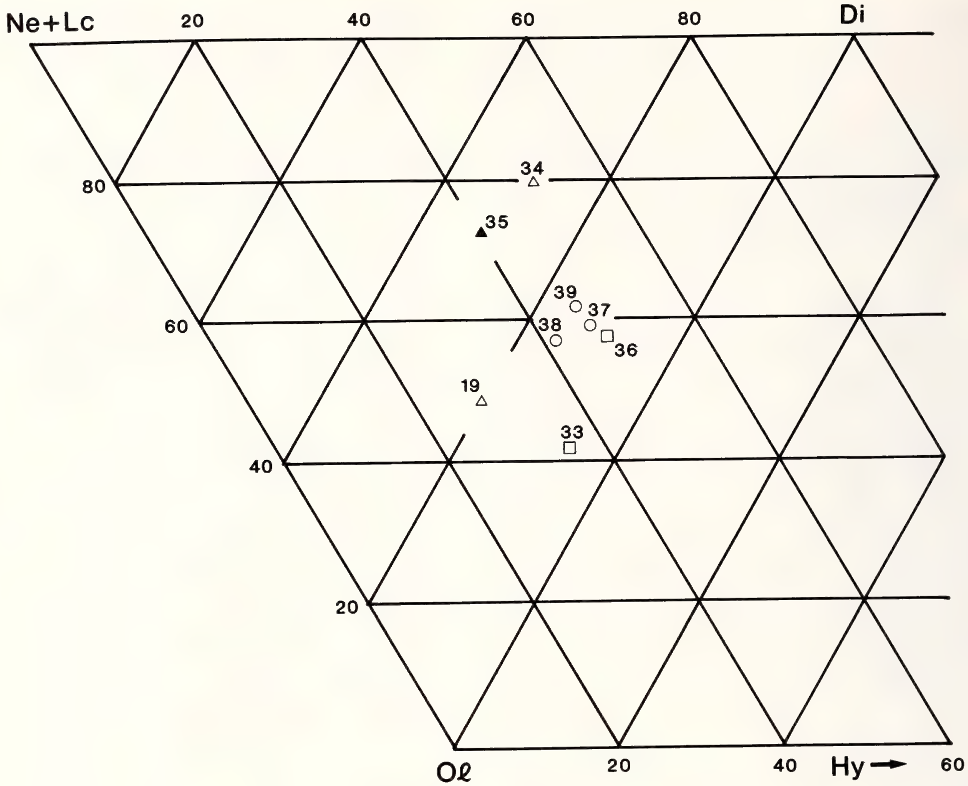


Figure 3. The normative compositions of the basanite (▲), tephrites (△), alkali basalts (□) and hawaiites (○) projected in the basalt tetrahedron and showing that all these rocks fall within the "alkali basalt" grouping (after Yoder and Tilley 1962: 352).

composition of $En_{39.2}$ $Fs_{14.9}$ $Wo_{45.9}$. These pyroxenes also carry $\bar{x}$ TiO_2 = 1.92%, $\bar{x}$ Al_2O_3 = 3.93% and $\bar{x}$ Na_2O = 0.41% (See Table 3, No 3, and Figure 8). The Fe-Ti oxides are mainly titanomagnetites that carry $\bar{x}$ TiO_2 = 26.6%, $\bar{x}$ Al_2O_3 = 2.04%, and $\bar{x}$ MgO = 1.41%. The analcime occurs as small anhedral interstitial grains.

HAWAIITE. The hawaiite from Belah Park west of the Namoi River (Table 2, No 39) is a porphyritic rock that has a differentiation index of 34.2 and carries 6.8% normative nepheline. Large fresh olivine crystals occur as phenocrysts, and they are set in a groundmass of feldspar, clinopyroxene, olivine, Fe-Ti oxides, analcime and apatite. The olivine is highly variable in composition (Fo_{57} to Fo_{79}) with a mean of Fo_{71}). This mineral carries $\bar{x}$ MnO = 0.36% and $\bar{x}$ CaO = 0.39% (See Table 4, No 1). These results are regarded as biased as most of the specimens analyses were phenocrysts and they are more forsteritic than the groundmass olivines. Feldspar makes up approximately half of the rock. It consists mainly of labradorite ($\bar{x}$ $Or_{1.7}$ $Ab_{41.3}$ $An_{57.0}$: See Table 5, No 5), but some andesine is present, whereas the groundmass contains sanidine. The clinopyroxenes range in composition from pale brownish pink titansalites to titaniferous salites with a mean composition of $En_{37.6}$ $Fs_{14.2}$ $Wo_{48.2}$. These pyroxenes also carry $\bar{x}$ TiO_2 = 2.05%, x Al_2O_3 = 3.40% and $\bar{x}$ Na_2O = 0.67%. The Fe-Ti oxides are mainly titanomagnetites that carry $\bar{x}$ TiO_2 = 28.30%, $\bar{x}$ Al_2O_3 = 1.31%, and $\bar{x}$ MgO = 1.32%. Analcime occurs as small anhedral grains within the groundmass.

The hawaiite from Emerald Hill (Table 2, No 37) has a differentiation index of 35.9, and it carries 4.7% normative nepheline. It has an ophitic texture. The essential minerals are plagioclase, titansalite, olivine and sanidine, with accessory amounts of titanomagnetite, analcime and apatite.

Plagioclase, mainly labradorite, is the most abundant mineral (48 vol. %). The pale brownish pink titansalites have a mean composition of $En_{35.3}$ $Fs_{17.4}$ $Wo_{47.3}$ (See Table 3 No 8). These pyroxenes also contain $\bar{x}$ TiO_2 = 3.30%, $\bar{x}$ Al_2O_3 = 4.69% and $\bar{x}$ Na_2O = 0.60%. The olivine crystals are generally smaller than the titansalite and they have compositions in the range of Fo_{53} to Fo_{65} with a mean of Fo_{62}, and they carry $\bar{x}$ MnO = 0.49% and $\bar{x}$ CaO = 0.39%.

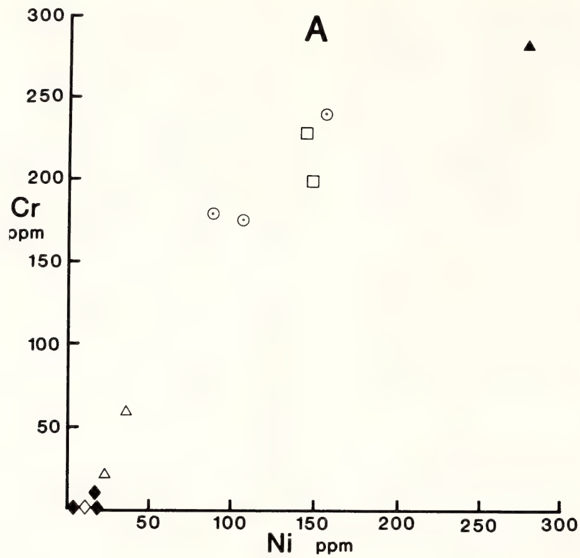
The hawaiite from Mt Binalong (Table 2, No 38) has a differentiation index of 36.1, and it carries 6.2% normative nepheline. It has an ophitic texture. The essential minerals are plagioclase, titaniferous salite and olivine, with accessory amounts of titanomagnetite, analcime, biotite and sanidine. Labradorite ($\bar{x}$ $Or_{1.5}$ $Ab_{39.0}$ $An_{59.50}$) is the dominant phase (See Table 5, Nos 4 and 6), and it often occurs enclosed within large pale pink titaniferous salites. The latter mineral has a mean composition of $En_{40.5}$ $Fs_{13.8}$ $Wo_{45.7}$ and carries $\bar{x}$ TiO_2 = 1.43%, x Al_2O_3 = 2.52% and $\bar{x}$ Na_2O = 0.33%. Many of the olivines are partly altered. They range in composition from Fo_{50} to Fo_{58} and their mean composition if Fo_{55}. They carry $\bar{x}$ MnO = 0.74% and $\bar{x}$ CaO = 0.45%. The Fe-Ti oxides are mainly titanomagnetites that carry $\bar{x}$ TiO_2 = 25.2%, $\bar{x}$ Al_2O_3 1.60%, and $\bar{x}$ MgO = 1.85%. Small elongate crystals of titaniferous biotite sometimes occur in association with these Fe-Ti oxides. The rock contains approximately 4% biotite that contains $\bar{x}$ TiO_2 = 5.83% and $\bar{x}$ Na_2O = 0.66%. Analcime and sanidine occur as small anhedral grains.

TEPHRITE. The tephrite from Boonoomarah (Table 2, No 34) has a differentiation index of 40.8 and it carries 12.5% normative nepheline. It is a porphyritic rock that contains large phenocrysts of pale brownish pink titansalite set in a groundmass of sanidine, labradorite, titansalite, olivine, analcime, titanomagnetite, apatite and calcite. The rock contains approximately equal amounts of sanidine and labradorite (See Table 5, No. 3). The sanidine has a mean composition of $Or_{38.7}$ $Ab_{55.0}$ $An_{6.3}$; with $\bar{x}$ Fe_2O_3 = 0.63%, whereas the labradorite is more variable in composition (i.e. An_{50} - An_{65}) and it has a mean composition of $Or_{2.6}$ $Ab_{40.4}$ $An_{57.0}$. Clinopyroxene makes up some 25% of the rock and it is mainly titansalite with a little titaniferous salite. The mean composition is $En_{35.7}$ $Fs_{16.2}$ $Wo_{48.1}$ with $\bar{x}$ TiO_2 = 2.75%, $\bar{x}$ Al_2O_3 = 4.07% and x Na_2O = 0.48% (See Table 3 No 5). Olivine makes up only 9 vol. % of the rock and it ranges from Fo_{55} to Fo_{62} with a mean of Fo_{59}. This mineral carries $\bar{x}$ MnO = 0.62% and $\bar{x}$ CaO = 0.56%. The rock also contains about 10% analcime. The main Fe-Ti oxide is titanomagnetite and it carries $\bar{x}$ TiO_2 = 32.7%, $\bar{x}$ Al_2O_3 = 1.31% and $\bar{x}$ MgO = 0.75%.

The basic tephrite from Mt Dangar (Table 2, No 19) has a differentiation index of 51.5 and it carries 6.0% normative nepheline. It is a porphyritic rock that contains occasional large olivine phenocrysts set in a trachytic groundmass composed of sanidine, plagioclase, titansalite, olivine, analcite, titanomagnetite, and apatite. The alkali feldspar is dominantly sanidine with a mean composition of $Or_{45.1}$ $Ab_{49.0}$ $An_{5.9}$ and it carries $\bar{x}$ Fe_2O_3 = 0.49% (See Table 5, No 9); whereas the plagioclases range from andesine (An_{44}) to labradorite (An_{53}) with a mean of $Or_{4.1}$ $Ab_{49.6}$ $An_{46.3}$. Most of the clinopyroxenes are pale brownish pink titansalites. They have a mean composition of $En_{32.3}$ $Fs_{19.7}$ $Wo_{48.0}$ and carry $\bar{x}$ TiO_2 = 2.21%, $\bar{x}$ Al_2O_3 = 3.75% and $\bar{x}$ Na_2O = 0.73%. Olivine makes up approximately 15 vol. % of the rock and it ranges in composition from Fo_{54} to Fo_{62} with a mean of Fo_{60}. It contains $\bar{x}$ MnO = 0.65% and $\bar{x}$ CaO = 0.49 (See Table 4, No 5). The Fe-Ti oxides are mainly titanomagnetites with a mean TiO_2 content of 26.4% and $\bar{x}$ Al_2O_3 = 2.38% and $\bar{x}$ MgO = 2.14%. The analcime occurs as small anhedral interstitial grains.

PHONOTEPHRITE. The phonotephrite from Mt Dangar (Table 2, No 21) has a differentiation index of 52.2 and carries 9.0% normative nepheline. It is a porphyritic rock in which olivine phenocrysts are set in a trachytic groundmass that mainly consists of elongate laths of andesine and alkali feldspar, together with salitic pyroxenes, olivine, Fe - Ti oxides, magnesio-hornblende, nepheline, zeolites and apatite. The feldspars make up approximately 55 vol. % of the rock. They consist of andesines ($\bar{x}$ $Or_{3.6}$ $Ab_{50.1}$ $An_{46.3}$), and alkali feldspars that range in composition from anorthoclase to sanidine. The pale pinkish brown pyroxenes range from titaniferous salites to titansalites with a mean composition of $En_{32.7}$ $Fs_{19.1}$ $Wo_{48.2}$. They also contain $\bar{x}$ TiO_2 = 2.0%, $\bar{x}$ Al_2O_3 = 3.75% and $\bar{x}$ Na_2O = 0.89%. The olivine ranges in composition between Fo_{50} to Fo_{58} with a mean of Fo_{56}. This mineral also carries x MnO = 0.77% and $\bar{x}$ CaO = 0.44%. The Fe - Ti oxides are mainly titanomagnetites. Small magnesio-hornblende crystals occur within the groundmass. They are pleochroic from light to dark brown, and with respect to the standard amphibole formula (cf. Leake 1978:512) they contain $(Ca + Na)_B$ = 2.00, Na_B = 0.52, $(Na + K)_A$ = 0.21, $Mg/(Mg + Fe^{2})$ = 0.64, and Si = 7.194. The nepheline and the zeolites (mainly natrolite) occur together in the groundmass.

The first of two phonotephrites from Mt Bodangora has a differentiation index of 58.0 and carries 6.5% normative nepheline (Table 2, No 18). It is a



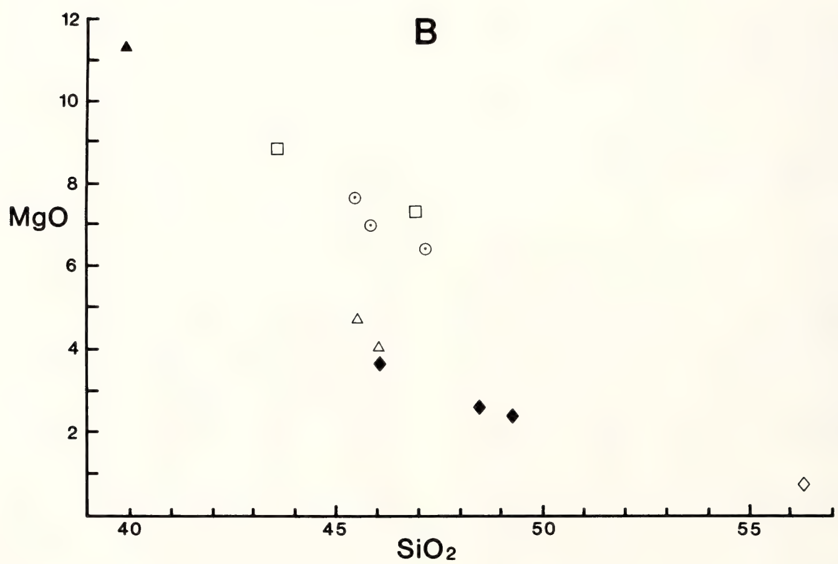


Figure 4. An illustration of the chemical variation in the Mesozoic rocks, where (▲) = basanite, (□) = alkali basalt, (⊙) = hawaiite, (△) = tephrite, (◆) = phonotephrite, and (◇) = trachyte. Both diagrams readily separate the basic alkalic rocks from the basic hyper-alkalic rocks.

porphyritic rock that contains occasional olivine phenocrysts set in a pilotaxitic groundmass that is mainly composed of a felted mass of lath-shaped feldspar crystals. Other phases observed in the groundmass include titaniferous salite, olivine, Fe - Ti oxides, zeolites and apatite. The dominant phase is sanidine with a mean composition of $Or_{41.8}$ $Ab_{52.4}$ $An_{5.8}$. Andesine is also found. The olivine (15 vol. %) ranges in composition between Fo_{35} and Fo_{46} with a mean of Fo_{40} (See Table 4, No 6). This mineral carries $\bar{x}$ MnO = 1.9%, and $\bar{x}$ CaO = 0.57%). The pale pinkish purple titaniferous salite has a mean composition of $En_{28.1}$ $Fs_{24.9}$ $Wo_{47.0}$, and it carries $\bar{x}$ TiO_2 = 1.60%, $\bar{x}$ Al_2O_3 = 2.67% and $\bar{x}$ Na_2O = 0.71% (See Table 3, No 10). Most of the Fe-Ti oxides are titanomagnetites and contain $\bar{x}$ TiO_2 = 23.8%, $\bar{x}$ Al_2O_3 = 1.36% and $\bar{x}$ MgO = 0.74%. Small patches of natrolite occur within the groundmass and they are regarded as being alteration products of nepheline.

The second phonotephrite from Mt Bodangora (Table 2, No 20) has a differentiation index of 58.4 and carries 4.0% normative nepheline. It has an ophitic to pilotaxitic texture. The dominant mineral is alkali feldspar (approx. 40 vol. %) and it ranges in composition from sanidine ($\bar{x}$ $Or_{44.3}$ $Ab_{50.7}$ $An_{5.0}$) to anorthoclase. Approximately 25 vol. % of the rock is composed of andesine ($\bar{x}$ $Or_{5.3}$ $Ab_{61.0}$ $An_{33.7}$). The pale pinkish purple titaniferous salite has a mean composition of $En_{29.0}$ $Fs_{23.7}$ $Wo_{47.3}$, and it carries $\bar{x}$ TiO_2 1.21%, $\bar{x}$ Al_2O_3 2.52% and x Na_2O = 0.74% (See Table 3, No 9). The olivine ranges in composition between Fo_{30} and Fo_{38} with a mean of Fo_{34}. This mineral carries $\bar{x}$ MnO = 1.27%, and $\bar{x}$ CaO = 0.80%. Most of the Fe - Ti oxides are titanomagnetites and contain $\bar{x}$ TiO_2 = 23.7%, $\bar{x}$ Al_2O_3 = 1.22% and $\bar{x}$ MgO = 0.72%. Small patches of natrolite occur within the groundmass and are interpreted as being alteration products of nepheline.

TRACHYTE. The trachyte from Mt Talbareeya (Table 2, No 32) was the most evolved rock studied. It has a differentiation index of 78.8 and it carries 7.2% normative nepheline. The rock is porphyritic with light yellowish brown olivines set in a trachytic groundmass that is completely dominated by elongate laths of alkali feldspar. Most of this feldspar is sanidine ($\bar{x}$ $Or_{43.1}$ $Ab_{54.7}$ $An_{2.2}$); however it grades into anorthoclase (See Figure 9). The Or content of the alkali feldspars analysed varies between 31 and 49 (See Table 5, Nos 7 and 8). Clinopyroxene constitutes approximately 8 vol. % of the rock and most of it is ferrosalite. This phase has a mean composition of $En_{19.8}$ $Fs_{32.5}$ $Wo_{47.7}$, and carries $\bar{x}$ TiO_2 = 0.46%, $\bar{x}$ Al_2O_3 = 1.56% and $\bar{x}$ Na_2O = 0.79% (See Table 3, Nos 11 and 12). The pyroxenes do however range from salites (En_{27} Fs_{26} Wo_{47}) to ferrosalite (En_{13} Fs_{39} Wo_{48}). Olivine makes up some 5% of this rock. It ranges from Fo_{12} to Fo_{22} with a mean of Fo_{17}. This mineral carries $\bar{x}$ MnO = 2.58% and $\bar{x}$ CaO = 0.95%. The main Fe - Ti oxide is titanomagnetite and it carried $\bar{x}$ TiO_2 = 16.4%, $\bar{x}$ Al_2O_3 = 0.83%, and $\bar{x}$ MgO = 0.17%. Natrolite and apatite were also found in the groundmass.

PETROLOGICAL DISCUSSION. The geochemistry, mineral chemistry and petrography of 12 authenticated Mesozoic volcanic and subvolcanic rocks from northeastern New South Wales has been described. They are all silica-undersaturated alkaline, but not peralkaline, rocks. One would not expect these rocks to be comagmatic as they congealed from magmas that were emplaced over a period of some 100 Ma, however they all have many broad geochemical and petrographic similarities as is illustrated by Figures 2 to 9. Most of them belong to two main petrographic groups; that is, an alkalic basic group which consists of the alkali basalts and hawaiites, and a hyperalkalic group which consists of tephrites and phonotephrites.

The Mg-values (i.e. 100 Mg/Mg + Fe^{++}, where Fe_2O_3/FeO = 0.2) of all the rocks were calculated. Only the basanite from Mt Binalong, and the alkali basalt from Gunnedah West, were found to have Mg-values greater than 60; that is, their Mg-values were 63.5 and 63.4 respectively. As primary magmas that equilibrated with the olivines in the upper mantle are usually considered to have Mg-values greater than 66 (B.V.S.P. 1981:528), it is inferred that all rocks studied probably congealed from derivative magmas that had experienced magmatic differentiation prior to their congealation. The basanite, the alkali basalts and the hawaiite from Namoi River (Mg-value = 58.7) probably experienced relatively little magmatic differentiation; the other hawaiites (Mg-values 54.7 and 56.0) are likely to be products of more extensive differentiation; whereas the remainder of the rocks all have Mg-values of less than 50, together with differentiation indices of more than 40, and they are regarded as being products of extensive magmatic differentiation.

This study augments the petrological model developed by Ferguson (1986:3-5) in which the Mesozoic is regarded as being an era of widespread alkaline volcanism. It also corraborates and extends the data introducted by Embleton et al. (1985) in their study of the age of the volcanic rocks of the Sydney Basin. They found that volcanic activity peaked at 180 Ma, and they proposed that this period of enhanced volcanic activity occurred in

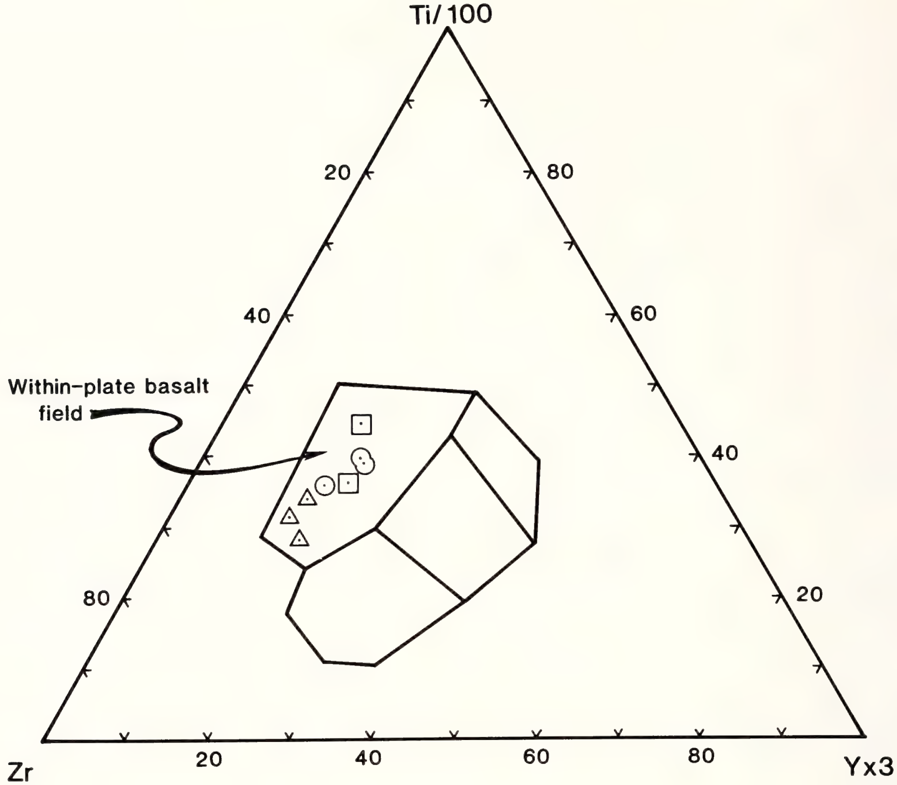


Figure 5. A Ti-Y-Zr discriminant diagram (after Pearce and Cann 1973), where (▲) = basanite, (◻) = alkali basalt, (⊙) = hawaiite and (△) = tephrite.

response to widespread tension within the lithosphere. It is proposed that this tensional regime extended to the north of the Sydney Basin, and deep fracturing of the lithosphere facilitated the upward movement of alkaline magma in an intraplate tectonic environment.

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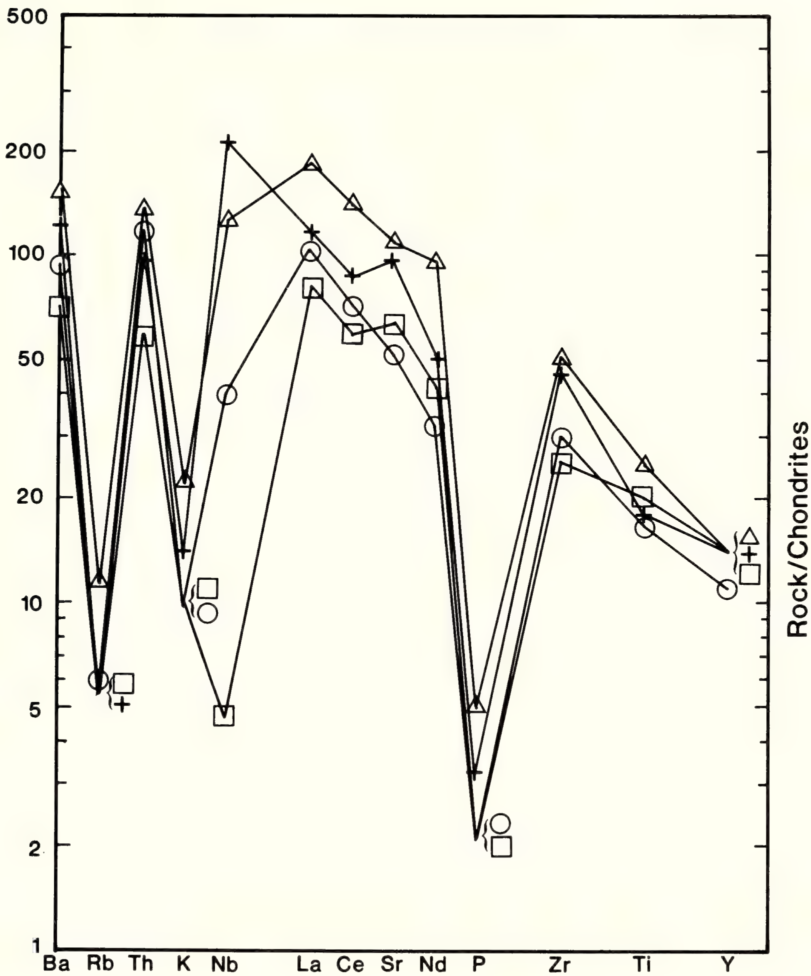

Figure 6. A chondrite-normalized "spidergram" in which the element abundances of the mean Mesozoic alkali basalts (□) and tephrites (Δ) of northern New South Wales are compared with Tertiary basaltic rocks from the Monaro (⊙) (after Kesson 1973: 100) and the Liverpool Range (+) (after Schön 1985:81, and Pers. Comm.).

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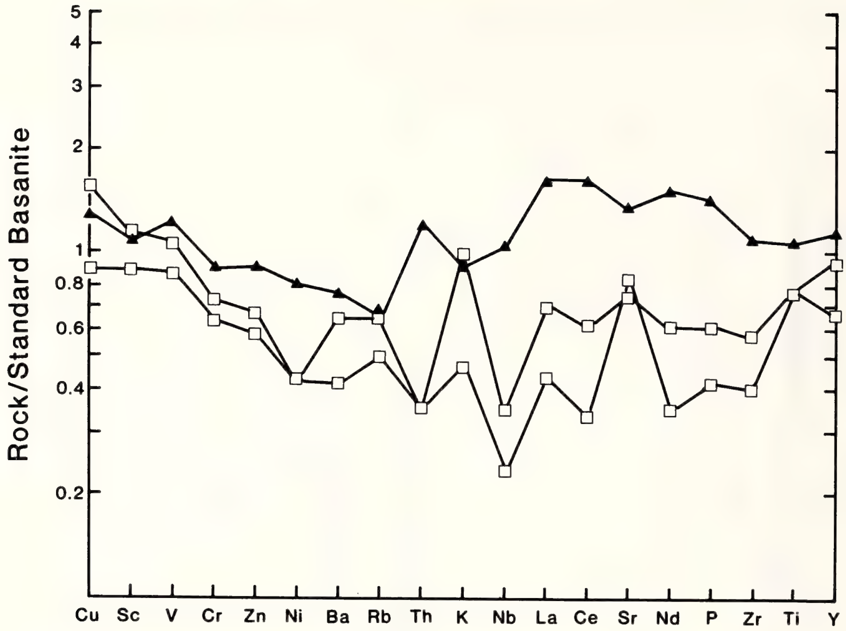

Figure 7. An extended "spidergram" showing the element abundances of Mesozoic (▲) and alkali basalt (□). The datapoints were normalized using a basanite from Mt. Shadwell, Victoria (Frey et al., 1978: 483).

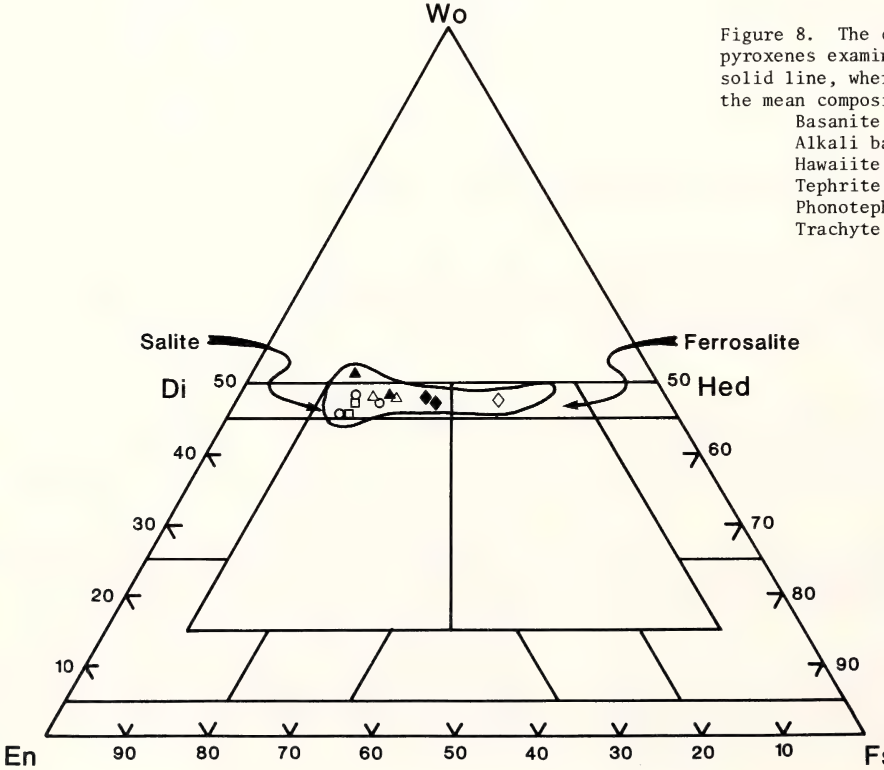

Figure 8. The compositions of all the pyroxenes examined are enclosed within the solid line, whereas the symbols represent the mean compositions of each sample.

Basanite = (▲)
Alkali basalt = (□)
Hawaiite = (○)
Tephrite = (△)
Phonotephrite = (◆)
Trachyte = (◇)

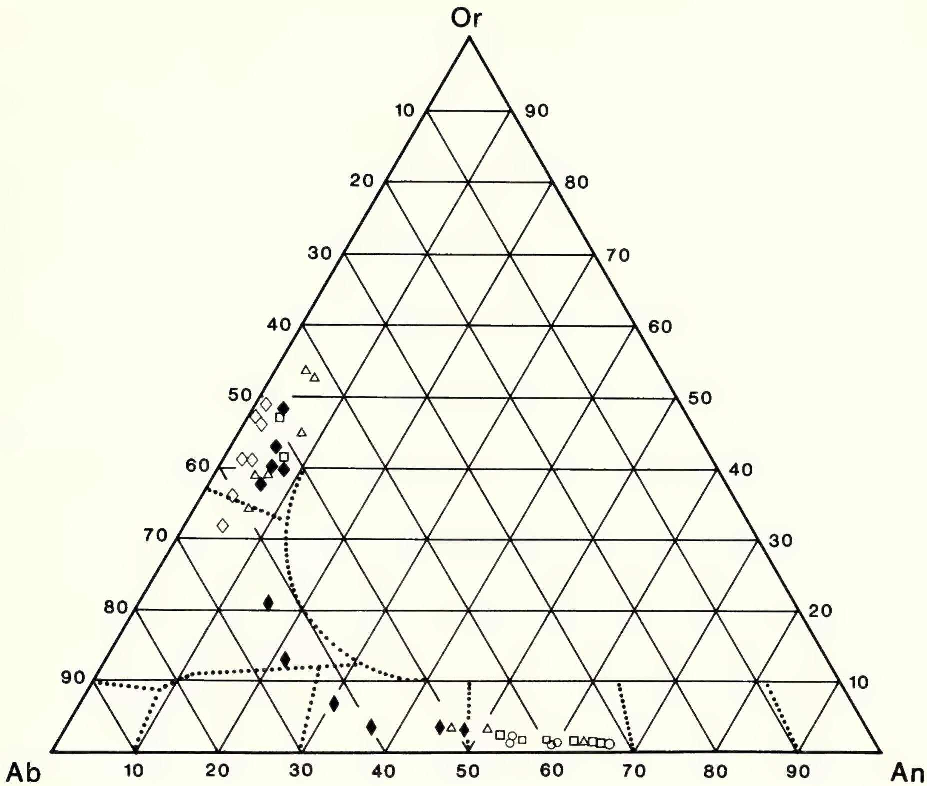

Figure 9. The compositions of the feldspars found in the rocks studied, where (▲) = basanite, (◘) = alkali basalt, (⊙) = hawaiite and (△) = tephrite, (♦) = phonotephrite, and (◇) = trachyte.

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J.A. DULHUNTY, E. MIDDLEMOST, and R. BECK.
DEPARTMENT OF GEOLOGY & GEOPHYSICS,
UNIVERSITY OF SYDNEY,
SYDNEY, NSW 2006

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TABLE 1

K-Ar analytical data and calculated rock ages

Locality	Spec. No.	K Wt %	Rad ^{40}Ar p.p.m.	Rad ^{40}Ar / Total ^{40}Ar	Age Ma
Mount Bodangora	K 18	2.633 2.617	.0285 .0278	.846 .844	148 ± 6
	K 20	2.729 2.755	.03738 .03608 .03599	.832 .748 .285	182 ± 7
Mount Dangar	K 19	2.644 2.654	.03798 .03749	.776 .857	194 ± 7
	K 21	2.488 2.511	.03681 .03868 .03810	.842 .858 .670	206 ± 8
Mount Binalong	K 35	1.456 1.445	.01630 .01493	.717 .599	148 ± 6
	K 38	.855 .888 .933 .957	.008023 .007540	.252 .572	119 ± 7
Nerada	K 36	.997 .956	.009858 .01070	.488 .586	145 ± 9
Boonoomarah Ridge	K 34	1.840 1.844	.02682 .02696	.745 .764	198 ± 8
Mount Talbareeya	K 32	4.356 4.370	.05341 .05499	.783 .866	170 ± 6
Emerald Hill	K 37	.779 .805	.01040 .01077	.652 .591	183 ± 8
Gunnedah West	K 33	.701 .670	.008883 .01019	.511 .596	189 ± 19
Namoi River	K39	1.214 1.249	.01927 .02008	.377 .356	217 ± 9

Constants used: $^{40}K/K = 1.193 \times 10^{-4}$ g/g $(\lambda_e + \lambda'_e) = 0.581 \times 10^{-10}$/year $\lambda_\beta = 4.962 \times 10^{-10}$/year

Table 2 Chemical Composition of Mesozoic Igneous Rocks of N.S.W.

No.	18	19	20	21	32	33	34	35	36	37	38	39
SiO_2	48.47	45.98	49.27	46.02	56.25	43.52	45.46	39.90	46.91	47.10	45.81	45.41
TiO_2	1.95	2.58	1.80	2.50	0.70	2.14	2.97	3.01	2.16	2.20	2.19	2.39
Al_2O_3	15.94	15.94	16.13	16.26	17.14	14.67	15.83	11.09	14.85	16.02	15.38	14.48
Fe_2O_3	2.82	2.78	2.90	2.61	3.30	3.80	2.18	4.35	2.70	2.16	3.15	2.23
FeO	9.98	10.07	9.81	10.16	4.22	7.40	8.90	9.80	8.36	9.20	8.73	9.33
MnO	0.22	0.19	0.23	0.20	0.20	0.16	0.18	0.23	0.16	0.19	0.19	0.18
MgO	2.63	4.01	2.40	3.65	0.70	8.89	4.75	11.34	7.30	6.41	6.99	7.67
CaO	5.36	6.79	5.28	6.58	2.80	8.41	9.82	10.56	9.27	9.49	8.94	9.70
Na_2O	5.18	4.89	4.91	4.94	6.10	3.02	4.36	4.01	3.62	3.95	4.01	3.64
K_2O	2.99	3.02	3.13	2.80	5.16	0.84	2.24	1.67	1.15	0.96	1.03	1.35
P_2O_5	1.40	1.73	1.46	1.89	0.34	0.34	0.84	1.20	0.49	0.50	0.58	0.75
CO_2	<0.10	<0.10	<0.10	<0.10	<0.10	0.45	<0.10	<0.10	<0.10	<0.01	<0.10	<0.10
H_2O^+	1.96	1.75	2.11	1.28	2.19	4.97	2.13	1.93	2.13	1.55	2.51	2.51
H_2O^-	0.25	0.15	0.34	0.18	0.51	0.94	0.18	0.23	0.54	0.23	0.36	0.20
Sum	99.15	99.93*	99.77	99. 07	99.61	99.54	99.91**	99.34***	99.64	99.96	99.87	99.84
Or	18.24	18.23	19.02	16.96	31.54	5.34	13.58	3.83	7.01	5.78	6.28	8.22
Ab	33.21	34.07	35.42	26.27	40.13	22.73	14.75	0.00	25.22	25.45	23.64	19.24
An	11.78	11.77	13.09	14.27	4.29	25.80	17.45	7.56	21.55	23.58	21.60	19.76
Lc	0.00	0.00	0.00	0.00	0.00	0.00	0.00	4.98	0.00	0.00	0.00	0.00
Ne	6.52	6.04	3.96	8.99	7.18	2.58	12.51	18.96	3.47	4.66	6.17	6.76
Di	5.30	8.34	3.44	5.48	6.76	13.54	22.19	31.13	18.46	17.28	16.62	20.20
Ol	14.61	15.01	14.93	15.53	6.09	21.95	9.01	21.31	16.14	15.02	17.08	16.49
Mt	3.18	3.16	3.14	3.15	1.83	2.86	2.74	3.48	2.74	2.79	2.93	2.87
Il	3.82	5.00	3.52	4.87	1.37	4.37	5.79	5.90	4.23	4.26	4.29	4.67
Ap	3.35	4.09	3.48	4.49	0.81	0.85	2.00	2.87	1.17	1.18	1.39	1.79
D.I.	57.97	51.51	58.40	52.21	78.84	30.64	40.84	27.77	35.70	35.89	36.09	34.22

Sc	10	9	9	9	6	19	22	23	24	22	19	23
V	25	71	12	62	<3	155	212	221	192	178	175	192
Cr	<1	21	<1	10	<1	199	60	281	227	179	174	239
Ni	6	23	3	15	3	148	36	279	144	88	103	155
Cu	32	41	27	36	5	39	71	56	69	62	60	72
Zn	118	91	129	100	86	78	96	124	90	95	98	99
Ga	28	27	28	25	27	20	28	25	24	26	25	26
Rb	52	58	60	38	90	18	34	24	24	14	18	21
Sr	657	1643	597	1675	271	696	997	1125	634	745	786	813
Zr	455	298	492	308	543	111	263	310	159	137	137	187
Nb	62	60	66	61	89	14	46	66	22	19	20	29
Ba	630	880	641	869	1199	195	443	352	301	217	319	250
Pb	<2	4	3	<2	2	<2	2	<2	<2	<2	<2	<2
Th	8	8	8	8	8	2	5	7	2	3	3	6
U	<1	<1	1	<1	3	<1	<1	<1	<1	<1	<1	<1
Y	51	31	53	31	38	16	27	28	23	21	22	22
La	71	78	76	82	78	20	50	76	32	26	31	40
Ce	138	149	157	166	141	31	99	153	57	52	59	80
Nd	73	77	91	88	62	17	55	76	30	29	35	40

Key to Table 2

18. Specimen K18, phonotephrite from Mt Bodangora
19. Specimen K19, tephrite from Mt Dangar
20. Specimen K20, phontephrite from Mt Bodangora
21. Specimen K21, phontephrite from Mt Dangar
32. Specimen K32, trachyte from Mt Talbareeya
33. Specimen K33, alkali basalt from Gunnedah West
34. Specimen K34, tephrite from Boonoomarah Ridge
35. Specimen K35, basanite from Mt Binalong
36. Specimen K36, alkali basalt from Nerada
37. Specimen K37, hawaiite from Emerald Hill, Monbrook
38. Specimen K38, hawaiite from Mt Binalong
39. Specimen K39, hawaiite from Belah Park near the Namoi River

Table 3 Representative electron microprobe analyses of clinopyroxenes

Spec.No	1	2	3	4	5	6	7	8	9	10	11	12
SiO_2	50.35	49.40	49.55	48.16	48.71	47.27	45.39	46.76	48.71	48.82	48.99	48.79
TiO_2	1.38	1.84	1.75	2.35	2.43	2.47	3.30	2.94	1.35	1.56	0.68	0.48
Al_2O_3	2.58	2.73	2.65	3.83	3.38	5.29	7.46	4.43	2.36	2.72	1.89	1.61
Cr_2O_3	0.28	0.03	0.05	0.04	0.01	0.02	0.07	0.02	0.00	0.00	0.00	0.03
FeO	7.95	8.14	9.92	9.04	9.44	7.04	7.28	10.16	14.37	14.79	16.09	19.56
MnO	0.15	0.14	0.22	0.20	0.18	0.11	0.10	0.17	0.29	0.32	0.54	0.59
MgO	14.19	14.23	13.26	12.87	12.81	12.41	11.66	11.97	9.55	9.31	8.49	5.95
CaO	21.97	22.64	22.14	22.87	22.63	23.92	23.51	22.29	21.79	21.91	21.97	21.61
Na_2O	0.33	0.39	0.50	0.52	0.40	0.62	0.58	0.58	0.68	0.80	0.72	0.70
K_2O	0.00	0.00	0.03	0.01	0.01	0.01	0.00	0.00	0.02	0.01	0.04	0.01
Sum	99.17	99.56	100.06	99.89	100.02	99.15	99.35	99.32	99.12	100.24	99.42	99.33
Si	1.89500	1.86203	1.87092	1.82215	1.84015	1.79158	1.72062	1.79043	1.89645	1.88337	1.91924	1.94221
Al	0.11453	0.12129	0.11774	0.17086	0.15047	0.23633	0.33306	0.20004	0.10807	0.12353	0.08733	0.07537
Ti	0.03903	0.05225	0.04980	0.06695	0.06902	0.07025	0.09394	0.08471	0.03943	0.04525	0.02004	0.01439
Cr	0.00828	0.00101	0.00138	0.00128	0.00042	0.00068	0.00222	0.00056	0.00000	0.00000	0.00000	0.00102
Mg	0.79532	0.79910	0.74557	0.72534	0.72088	0.70077	0.65841	0.68246	0.55399	0.53488	0.49523	0.35291
Fe	0.25008	0.25645	0.31299	0.28597	0.29804	0.22299	0.23063	0.32532	0.46776	0.47684	0.52696	0.65074
Mn	0.00485	0.00446	0.00694	0.00634	0.00589	0.00347	0.00306	0.00543	0.00951	0.01043	0.01799	0.02003
Ca	0.88554	0.91367	0.89533	0.92667	0.91555	0.97081	0.95433	0.91400	0.90864	0.90511	0.92172	0.92118
Na	0.02373	0.02833	0.03654	0.03810	0.02946	0.04522	0.04283	0.04303	0.05164	0.05991	0.05482	0.05416
K	0.00000	0.00019	0.00144	0.00037	0.00036	0.00025	0.00015	0.00007	0.00075	0.00044	0.00214	0.00043
Numbers of ions on the basis of 6 oxygens												
En	41.2	40.6	38.2	37.4	37.3	37.0	35.7	35.5	28.7	27.9	25.5	18.3
Fs	12.9	13.0	16.0	14.8	15.4	11.8	12.5	16.9	24.2	24.9	27.1	33.8
Wo	45.9	46.4	45.8	47.8	47.3	51.2	51.8	47.6	47.1	47.2	47.4	47.9

Table 4 Representative electron microprobe analyses of olivines

Spec.No	1	2	3	4	5	6	7	8	9
SiO_2	38.51	37.76	37.65	35.99	35.97	33.06	30.31	30.46	30.55
TiO_2	0.01	0.01	0.06	0.00	0.09	0.06	0.04	0.05	0.13
Al_2O_3	0.09	0.11	0.07	0.00	0.08	0.05	0.04	0.12	0.13
Cr_2O_3	0.04	0.02	0.00	0.00	0.00	0.01	0.00	0.02	0.04
FeO	20.19	25.79	27.17	31.97	34.36	45.51	58.28	60.36	60.70
MnO	0.25	0.34	0.63	·0.50	0.64	1.09	2.44	2.54	2.80
MgO	41.56	37.29	34.48	32.70	29.77	19.99	7.65	5.60	4.98
CaO	0.29	0.34	0.75	0.43	0.45	0.59	0.88	1.06	1.04
Sum	100.93	101.66	100.81	101.59	101.35	100.36	99.64	100.21	100.37
Si	0.98412	0.98386	0.99790	0.97077	0.98339	0.97429	0.97798	0.98804	0.099181
Al	0.00273	0.00337	0.00221	0.00000	0.00259	0.00163	0.00171	0.00465	0.00481
Ti	0.00012	0.00016	0.00115	0.00000	0.00178	0.00125	0.00097	0.00122	0.00306
Cr	0.00071	0.00038	0.00000	0.00000	0.00000	0.00015	0.00000	0.00046	0.00105
Mg	1.58170	1.44736	1.36104	1.31375	1.21245	0.87783	0.36766	0.27041	0.24070
Fe	0.43120	0.56180	0.60205	0.72089	0.78533	1.12116	1.57216	1.63675	1.64740
Mn	0.00551	0.00760	0.01419	0.01135	0.01472	0.02718	0.06670	0.06969	0.07706
Ca	0.00786	0.00948	0.02122	0.01238	0.01319	0.01876	0.03040	0.03688	0.03612
	Number of ions on the basis of 6 oxygens								
Fo	78.6	72.0	69.3	64.6	60.7	43.9	18.9	14.2	12.7
Fa	21.4	28.0	30.7	35.4	39.3	56.1	81.1	85.8	87.3

Key to Table 3

1. Titaniferous salite from hawaiite, Mt. Binalong (38)
2. Titaniferous salite from alkali basalt, Gunnedah West (33)
3. Titaniferous salite from alkali basalt, Nerada (36)
4. Titansalite from alkali basalt, Gunnedah West (33)
5. Titansalite from tephrite, Boonoomarah (34)
6. High-Ca titansalite from basanite, Mt. Binalong (35)
7. High-Ca titansalite from basanite, Mt. Binalong (35)
8. Titansalite from hawaiite, Emerald Hill (37)
9. Titaniferous salite from phototephrite, Mt. Bodangora (20)
10. Titaniferous salite from phototephrite, Mt. Bodangora (18)
11. Ferrosalite from trachyte, Mt. Talbareeya (32)
12. Ferrosalite from trachyte, Mt. Talbareeya (32)

Key to Table 4

1. Olivine from hawaiite, Namoi River (39)
2. Olivine from alkali basalt, Gunnedah West (33)
3. Olivine from basanite, Mt. Binalong (35)
4. Olivine from alkali basalt, Nerada (36)
5. Olivine from tephrite, Mt. Dangar (19)
6. Olivine from phonotephrite, Mt. Bodangora (18)
7. Olivine from trachyte, Mt. Talbareeya (32)
8. Olivine from trachyte, Mt. Talbareeya (32)
9. Olivine from trachyte, Mt. Talbareeya (32)

Table 5 Representative electron microprobe analyses of feldspars

Spec. No.	1	2	3	4	5	6	7	8	9
SiO_2	51.02	52.14	52.34	52.21	52.75	53.76	65.58	65.49	65.16
TiO_2	0.10	0.15	0.17	0.13	0.13	0.11	0.05	0.01	0.18
Al_2O_3	30.04	29.20	29.17	28.90	29.01	28.21	18.96	18.77	18.81
Fe_2O_3	0.71	0.82	0.85	0.66	0.53	0.58	0.28	0.14	0.40
MgO	0.06	0.04	0.11	0.08	0.07	0.10	0.00	0.03	0.00
BaO	nd	0.01	nd	nd	nd	nd	nd	nd	nd
CaO	13.70	13.06	13.30	12.31	11.81	11.32	0.55	0.24	0.79
Na_2O	3.80	3.87	4.04	4.53	5.25	5.11	6.62	6.10	4.87
K_2O	0.20	0.24	0.32	0.26	0.36	0.30	7.21	8.40	9.26
Sum	99.71	99.53	100.30	99.08	99.91	99.49	99.25	99.18	99.47
Si	9.35765	9.54940	9.53497	9.59819	9.62167	9.80933	11.88540	11.91470	11.86930
Al	6.49185	6.30285	6.25085	6.26012	6.23496	6.06444	4.04890	4.02388	4.03750
Fe	0.09767	0.11315	0.11684	0.09278	0.07243	0.07857	0.03788	0.01921	0.05509
Ti	0.01364	0.02072	0.02340	0.01769	0.01760	0.01450	0.00740	0.00093	0.02452
Mg	0.01734	0.01186	0.03005	0.02326	0.01979	0.02784	0.00000	0.00733	0.00000
Na	1.37911	1.37308	1.42712	1.61251	1.85459	1.80519	2.32272	2.15013	1.71988
Ca	2.69127	2.56169	2.59533	2.42375	2.30972	2.21121	0.10694	0.04650	0.15492
K	0.04743	0.05707	0.07340	0.06009	0.08410	0.07056	1.66732	1.94823	2.15103
Ba	-	0.00083	-	-	-	-	-	-	-
	Number of ions on the basis of 32 oxygens								
Or	1.2	1.4	3.4	1.5	2.0	1.7	40.7	47.0	53.4
Ab	33.5	34.4	46.0	39.4	43.7	44.2	56.7	52.9	42.7
An	65.3	64.2	50.6	59.1	54.3	54.1	2.6	1.1	3.9

Key to Table 5

1. Labradorite from hawaiite, Mt. Binalong (38)
2. Labradorite from alkali basalt, Gunnedah West (33)
3. Labradorite from tephrite, Boonoomarah (34)
4. Labradorite from hawaiite, Mt. Binalong (38)
5. Labradorite from hawaiite, Namoi River (39)
6. Labradorite from hawaiite, Mt. Binalong (38)
7. Sanidine from trachyte, Mt. Talbareeya (32)
8. Sanidine from trachyte, Mt. Talbareeya (32)
9. Sanidine from tephrite, Mt. Dangar (19)

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The Mineralogical Relationship between some Arid Zone Soils and their Underlying Bedrocks at Fowlers Gap Station, New South Wales (N.S.W.) Australia

E. G. AKPOKODJE

ABSTRACT. The clay mineralogy of metasedimentary quartzites, phyllitic shales and their overlying desert loam soils from Fowlers Gap Station, Australia, were studied. The mineralogic variations in the rock sequence may be attributed to broad changes in both provenance and the environment of deposition. The smectite, illite and chlorite found in the soils formed on the phyllitic shales were all inherited from the bedrock. Kaolinite, on the other hand, is the weathering product developed under a subtropical climate in the Tertiary. The same clay minerals, except chlorite, also occur in the soils developed on the crest and colluvial slopes of the quartzites. Kaolinite was inherited from the bedrock, whereas significant proportions of the smectite and illite are considered to have been partly formed from the intercalated clay and the closely associated phyllitic shales, where they occur as relics on the weathered Tertiary surface.

INTRODUCTION

Arid zone soils, (especially Desert Loams) occur extensively over different rock types in Arid Australia but their relationships to the underlying bedrock are not always clearly defined. A good knowledge of the genetic relationship between these soils and the underlying bedrock is very important because it provides a broad basis upon which a preliminary grouping of aridic soils in Australia could be made. Such a general classification is essential, if a detailed systematic study of these extensive soils is to be successfully carried out. In addition, the study of the mineralogical relationship would be invaluable in understanding weathering and soil forming processes in the area.

The rocks and soils described in this paper are found in the Australian semi arid zone, at Fowlers Gap Arid Zone Research Station, situated 110 kilometres north of the mining city to Broken Hill (Fig. 1), Stace *et al.* (1968) first described the extensive occurrence of reddish clayey soils (Desert Loams) in the study area and suggested that they have weathered primarily *in-situ*, from the underlying bedrock or alluvium. Chartres (1982) studied the micromorphology of the soils and concluded that the silt and clay pellets, as well as the carbonate modules in the soils, are aeolian in origin. However, neither study examined in detail the exact contribution of the bedrock to the clay mineralogy of the soils.

The study area is underlain by highly complex sequences of Precambrian rocks (schistose quartzite, metaquartzite, phyllitic shales, calcareous shales and dolomite/dolomitic shale). The extent of weathering alteration in a rock, depends on the climate, the mineralogy and texture of the rock, as well as the geomorphology of the terrain. Under similar climatic conditions, the weathering rates of rocks are largely determined by their mineralogical compositions. In the study area, both hard and soft metasediments (quartzite and phyllitic shales) have been subjected to varying degrees of weathering alteration under two climatic condtiions (Early to Mid Tertiary): (a) Humid conditions (Late Tertiary) (b) arid conditions (Late Tertiary/Early Quaternary to present). As a result, the weathering profiles of the rocks are characterised by humid and arid weathering features (Beavis, 1981; Beavis *et al.*1982).

In this paper, the mineralogy of the arid zone soils at Fowlers Gap Station is discussed in relation to their underlying bedrock in order to assess the nature of the genetic relationship,if any, that exists between rocks and soil developed on them.

METHODS OF STUDY

During field study, 30 major test pits/trenches were excavated and about 200 soil samples were taken for laboratory analysis. Two main laboratory tests were carried out, namely; particle size distribution and X-ray diffraction of both the whole soil and the clay fraction. Rock samples were obtained from diamond drill boreholes which were sunk to study the weathering profiles of the rocks (Beavis, 1981; Beavis *et al.* 1982). The clay fraction for XRD analyses was obtained by pipetting the required clay suspension using the settling time for 2m clay particles. The oriented clay fractions were examined by X-ray diffraction without further treatment, after saturation with ethlene glycol, on glycolation and heating at 350°C for one hour, after heating at 550°C for one hour and on solvation with HCl and glycolation.

1. Untreated
2. Treated with ethlene glycol
3. Glycolated sample heated to 350°C for one hour
4. Heated to 550°C for one hour
5. Treated with concentrated HCl and then glycolated.

Approximations of the mineralogical composition of whole soil samples were obtained from the powder run, using the method described by Schultz (1969) whereas those for the clay fraction were estimated by using the intergrated peak area intensity method (John, Grim and Bradley, 1954; modified by Jorgensen, 1964; Ruche and Olson, 1979).

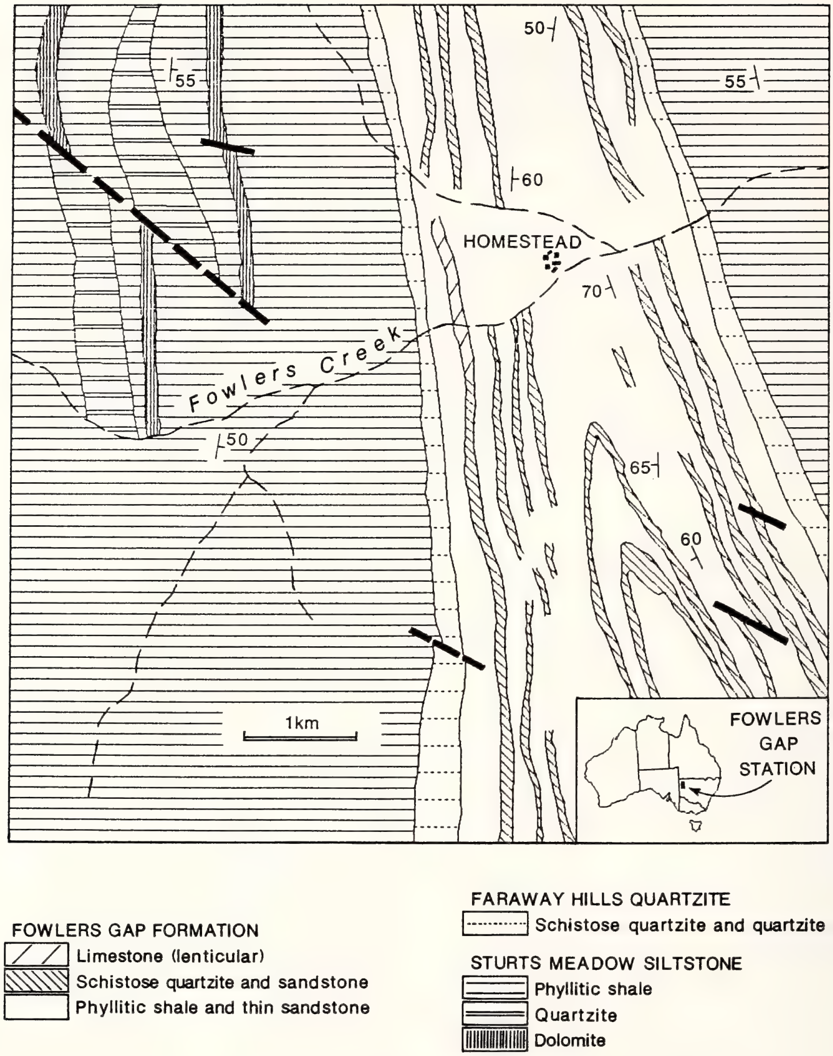

Fig. 1. Location and Geology of the Study Area (After Beavis et al., 1982).

GENERAL DESCRIPTION OF THE STUDY AREA AND THE SOILS

The area is underlain by complex and closely associated units of folded and steeply dipping (up to 70°) Precambrian metasediments (Sturts Meadows Siltstone, Faraway Hills Quartzite and Fowlers Gap Formation). These are unconformably overlain (especially on the eastern portion) by thick, unconsolidated Tertiary/Quaternary sediments and alluvium. The phyllitic shales are frequently calcareous, with thin to thick units of limestone and dolomite whereas the Faraway Hills Quartzite has a well marked but thin clay intercalation.

Geomorphologically, three broad regions can be distinguished, namely; (a) an undulating lowland with low ridges, (b) ranges and foothills and (c) extensive alluvial plains (Mabbutt, 1973).

The present climate is semi arid, (Bell, 1973) with a mean annual rainfall of 195mm. The mean daily maximum temperature reaches 38°C in January while the mean daily minimum temperature is as low as 3°C in July. However, it has been shown by Bowler (1976) that more humid subtropical conditions existed during the Tertiary.

The field study of soil profiles revealed that morphologically similar soil profiles occur over the different rock types and the unconsolidated alluvium. The soils usually have a thin (less than 10cm) non-pedal sandy/silty loam A horizon, which is covered by stones and rock fragments (mostly quartz and shales). The A horizon is clearly separated from the moderate reddish brown (5YR4/4), strongly pedal clayey B horizon with subangular blocky structure. Between depths of 100-120cm, the lower part of the B horizon (B_2) is usually characterised by highly gypsiferous,

carbonate-rich Bcs horizon (U.S.D.A. 1975). This is, however, absent in very shallow profiles, those on recent flood plains and in some of the profiles on the unconsolidated alluvium. The Bcs horizon is also associated with a stoney layer especially in profiles overlying the crests and colluvial slopes of the quartzites. Where the soil profile overlies the phyllitic shale, the Bcs horizon grades into highly/completely weathered C horizon whereas it often lies directly on slightly weathered quartzite/sandstone.

MINERALOGY OF THE ROCKS

The relative mineralogical compositions of the various rocks and their XRD traces are presented in Table I and Fig. 2 respectively. The high crystallinity of the dolomite crystals is evident in the sharpness of the reflections and the 104 peak which occurs at about 2.881 Å, and this corresponds very closely to that of stoichiometric dolomite (Ca^{++}:Mg^{++} = 1:1). Unlike the underlying dolomite, the phyllitic shales are characterized by a polymineralic composition, a well defined schistosity and the occurrence of diagenetic minerals such as pyrite, magnetite and siderite. Sericite (muscovite) and chlorite (iron rich variety) occur prominently, with the former dominating. The absence of smectites in the fresh rock supports the conclusion by a number of authors (Grim, 1968; and Weaver, 1967) that these minerals are unstable in strata older than Mesozoic.

The most important distinguishing characteristic of the Faraway Hills Quartzite is the dominance of quartz and kaolinite, which constitute about 90-95% of the rock. Chlorite is conspicuously absent from the intercalated phyllitic shales which occur within the unit.

The alternating units of phyllitic shale and schistose quartzite/sandstones of the Fowlers Gap Formation have highly varied petrographic, textural and mineralogical characteristics. Generally, the quartzite/sandstone units resemble the schistose quartzite of the Faraway Hills Quartzite in most respects, except for the occurrence of lenses of limestone. The majority of the phyllitic shales are strongly calcareous and diagenetic minerals such as pyrite, magnetite and siderite also occur.

RESULTS AND DISCUSSION

Particle Size Distribution

The results of the particle size distribution (Table 2) showed that the A horizons are mostly sandy or silty loams whereas the B horizons are clay and/or clay loam. The clay content of the B horizon increases significantly when more vigorous and prolonged mechanical agitation is applied. Such subplastic properties have also been described by Butler (1976) and Chartres (1982) in their studies of Desert Loams in the Australian arid zone. There is a marked texture contrast between the A and B horizons whereas the clay content remains relatively constant within the B_1 and B_2 horizons, but declines in the Bcs and C horizons. The high clay content of the B horizons of soil profiles overlying the quartzites (crests and colluvial slopes) appears unlikely to have been exclusively produced from the weathering *in-situ* of the siliceous bedrock under the present arid conditions. However, it is possible that some proportion of the clay could have been derived from the weathered Tertiary surface. The trend towards more arid conditions since the end of the Tertiary makes it difficult to exclude the added influence of aeolian activities.

MINERALOGY

Mineralogy of soils developed on the Quartzite/ Sandstones

The mineralogical composition of the weathered quartzite and the soils overlying the crests and colluvial slopes is given in Table 3. There are significant differences between the underlying weathered bedrock and the soils overlying their crests and colluvial slopes.

Quartz constitutes the dominant non-clay mineral in the B horizon whereas gypsum and calcium carbonate occur abundantly in the Bcs horizon or lower layers. The origin of the carbonate and gypsum, which do not usually occur in the fresh or weathered quartzite in the Australian arid zone, has previously been discussed (Akpokodje, 1984).

Kaolinite, illite, and smectite were all positively identified in the profiles that overlie the quartzite/sandstone. In addition to these clay minerals, profiles associated with both the quartzite and the phyllitic shales also contain chlorite in the C horizon. Kaolinite dominates in the soil profiles overlying the crests of the ridges whereas smectite predominates in the soils on the colluvial slopes (Fig. 3). In fact, smectite was not detected in the shallow soils (regosols) found on the ridge crests. The smectite peaks are asymmetrical, with high backgrounds on the low angle (2Θ) side of the reflection. The high background is most probably indicative of randomly mixed layer clays (Reynolds and Hower, 1980). Although, there is no significant change in the clay mineralogy with depth, there appear to be some differences in the composition of the illite present in the various soil profiles. In the very shallow soils on ridge crests, the illite peak is symmetrical and does not show any sign of a distinct mixed layer clay or degraded illite, whereas in the deeper crestal soils, the presence of a randomly mixed layer clay is indicated by the occurrence of a separate peak at 10.5A. The peak may be due to palygorskite. However, the much deeper soil profiles on the colluvial slopes do not have a separate peak at 10.5Å. The only indication of any mixed layer clay in the 10A peak, is shown by the high background (or tail on the low angle side of the peak). The major mineralogical change in these soils appears to be the partial breakdown of illite to smectite through the intermediate randomly mixed layer illite - smectite clays, which may form a separate reflection. In the more advanced stage of this illite - smectite transformation (as in the deep soils on the colluvial slopes), the reflection of the intermediate mixed layer clay has apparently been incorporated into the smectite peak.

The underlying quartzite/sandstone bedrock is dominated by quartz and kaolinite; and these minerals are inherited by the overlying soils. However, all the smectite, illite and mixed layer clays in the soils are most unlikely to be directly derived from the bedrock. Since the soils on the ridge

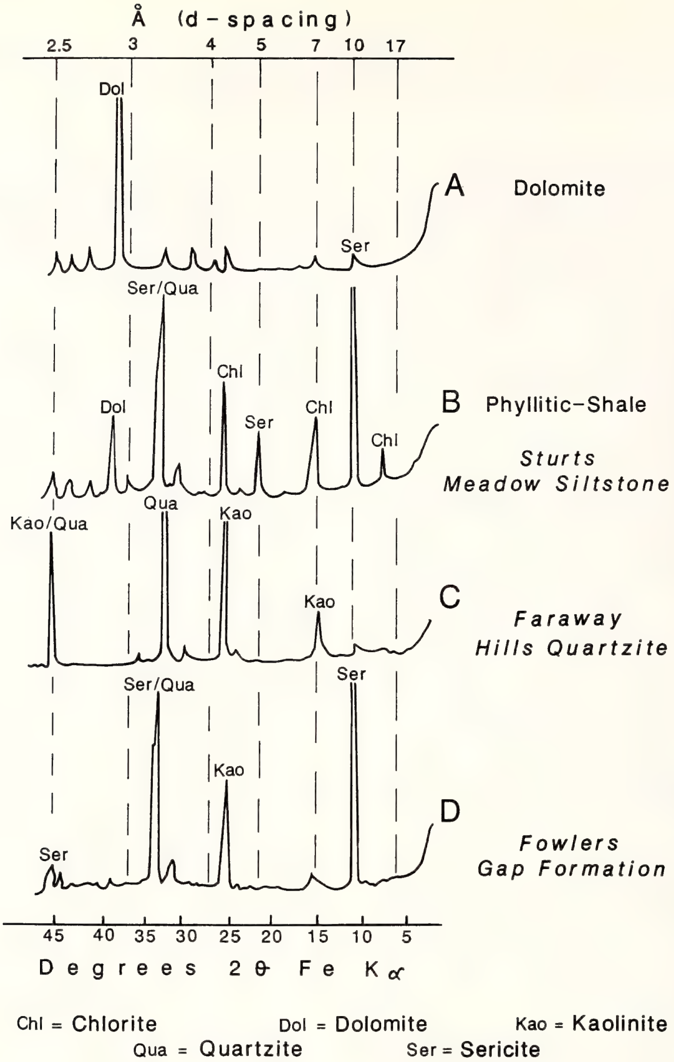

Fig. 2. XRD traces of samples from the rock units at Fowlers Gap.

crests occupy the highest terrain in the area they are not of alluvial origin but are rather the product of aeolian activity or represent relics of the weathered Tertiary surface. Relatively wet subtropical conditions existed in the Early to Mid Tertiary whereas conditions drier than the present day arid climate also prevailed in the late Tertiary/Early Quaternary (Bowler, 1976). These two previous climatic conditions are responsible for the observed mineralogical alterations and some degree of aeolian activities.

Mineralogy of Soils developed on the Phyllitic Shales

As a result of the low topographic situation of these soils, some of them usually contain eroded soil material from the ranges. In discussing the mineralogy of the phyllitic shale soils, emphasis will be placed on those soil profiles that are relatively free from these eroded materials. The mineralogical composition of the weathered phyllitic shales and the soils found on them are summarized in Table 4. The major difference between these soils and those on the quartzite is the significant proportions of illite and chlorite present in them, especially in the C horizon. In addition, these two clay minerals definitely increase with depth. At the upper soil layers (A & B horizon), the illite is made up of both de-

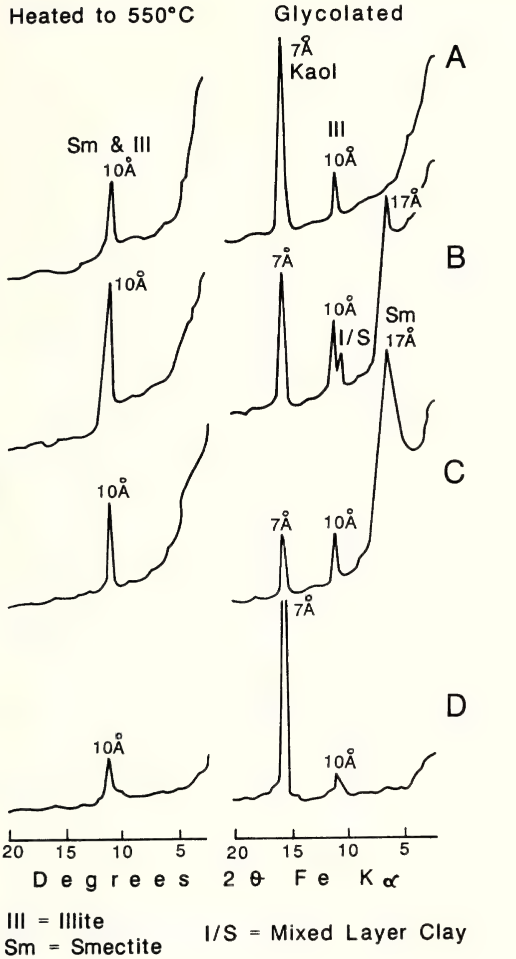

Fig. 3. XRD traces of clay minerals of the weathered quartzites and soils developed on the quartzite. A: Shallow regosols on ridge crest, depth 15 cm; A horizon. B: Deep soil on ridge crest - depth 35cm; B horizon. C: Very deep soil on colluvial slope, depth 55 cm; B horizon. D: weathered quartzite, depth 400 cm.

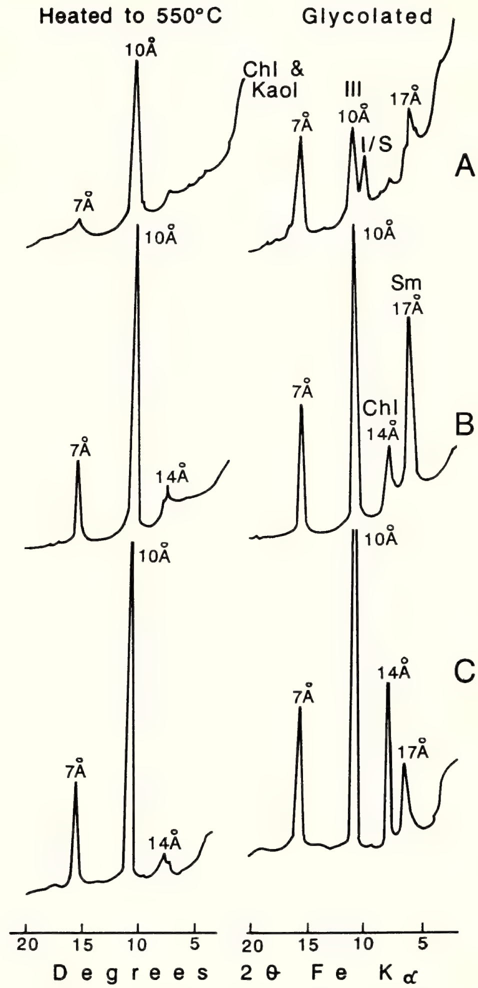

Fig. 4. XRD traces of clay minerals of the weathered phyllitic shale and soils developed on the shales. A: Depth 50 cm, B horizon. B: Depth 145 cm, Bcs/C horizon. C: weathered phyllitic shale, depth 700 cm.

graded and non-degraded varieties (Fig. 4) as indicated by two separate peaks at 10Å and 11Å respectively. But in the C horizon, the second peak at 11Å has disappeared, leaving only a high background or tail on the low angle (2Θ) side of the 10Å reflection. The partial decomposition of the illite at the upper soil horizon (A & B) has resulted in the formation of the illite-smectite mixed layer clay whereas the illite is still relatively undecomposed in the subsoil where the weathering intensity is lower.

Smectite occurs throughout the soil profiles with its reflection becoming more symmetrical with increasing depth (Fig. 4). The asymmetrical nature of the smectite peak in the surface layers (A & B horizons) is again most probably due to the presence of illite-smectite mixed layer clays. However, these clays do not occur in any significant amount in the subsoil (due to low weathering intensity), hence the more symmetrical nature of the smectite reflection.

Both kaolinite and chlorite occur in all the soil profiles. There is a gradual decrease of the former and a corresponding increase of the latter with increasing depth (Fig. 5). The variation with depth is partly due to the different weathering resistance of the two clays and also as a result of inheritance from the bedrock. Kaolinite forms under intense weathering (subtropical/tropical) conditions and its decrease with depth reflects the

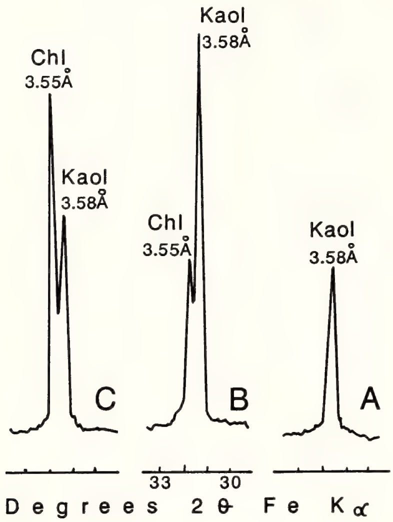

Fig. 5. Slow scan XRD traces of soils on phyllitic shales showing variation of chlorite and kaolinite with depth.
A: Depth 45 cm, B horizon.
B: Depth 130 cm, Bcs horizon
C: Depth 175 cm, C horizon.

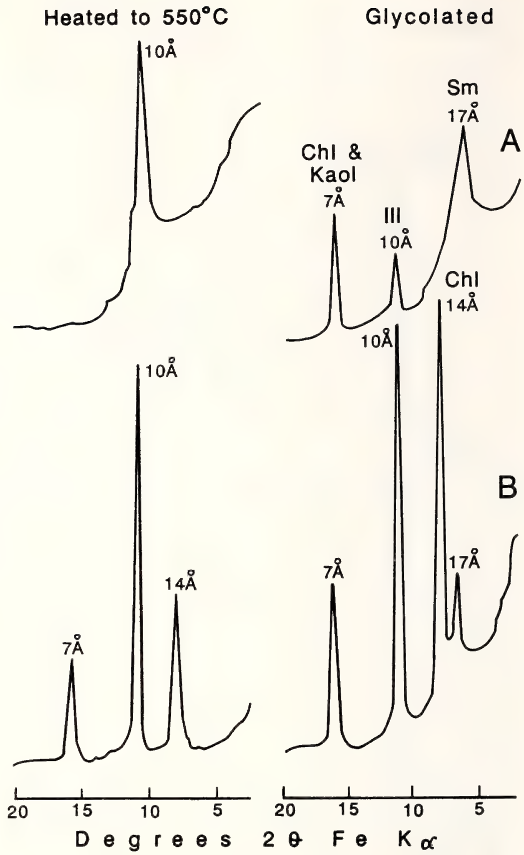

Fig. 6. XRD traces for clay minerals of the red-brown soil aggregates and the enclosing grey soil of the shales.
A: Red-brown soil, depth 185 cm.
B: grey soil, depth 185 cm.

reduction in weathering intensity. On the other hand, chlorite decomposes in the first stages of weathering and does occur in soils that have been intensely weathered. This, most probably explains its absence from the surface layers (A and B horizons) where weathering was most intense. At depth, it is stable because of the reduced weathering intensity and in addition, the increasing proportion with depth also indicates inheritance from the bedrock.

Reddish brown soil patches that are similar in colour and mineralogy (Fig. 6) to those in the B horizon, occur abundantly (down to a depth of about two metres or C horizon) within the greyish residual soils of the shales. Beavis *et al.* (1982) attributed the occurrence of kaolinite and smectite in the highly/completely weathered phyllitic shale, to the Tertiary subtropical weathering. The reddish brown soil patches were therefore most likely formed during this period, and the clay minerals could then be regarded as relics from the weathered Tertiary surface. With respect to soils overlying the phyllitic shales, inheritance and weathering (especially during previous wetter climates) are considered to have played significant roles in the origin of the clay minerals since all of them were also found to exist in the underlying weathered phyllitic shales.

CONCLUSION

The mineralogical compositions of the phyllitic shales and schistose quartzites/sandstones reflect variations in the depth of deposition as well as changes in the weathering intensities at the provenance. The main clay minerals (smectite, kaolinite, illite and chlorite) in the soils can all be interpreted (in most parts) in terms of inheritance from the bedrocks and reconstitution through chemical weathering which occurred during the wetter subtropical climate of the Tertiary. However, the present level of development and the distribution of the soils are most probably due to limited degree of weathering, leaching and erosion during the wetter periods, superimposed by the effects of aeolian activities in a latter drier phase.

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Department of Geology
University of Port Harcourt
P.M.B. 5323
Port Harcourt
NIGERIA

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TABLE I
RELATIVE MINERALOGICAL COMPOSITION OF THE MAJOR ROCK TYPES

	Percentage of whole rock sample						(%) CLAY SIZE FRACTION		
MINERALS / ROCK TYPES	Quartz	Dolomite	Calcite	Feldspar	Pyrite	Clay Min.	Illite	Chlorite	Kaolinite
Dolomite	0-10	80-90	0.5	0.5	-	0.5	70-80	0-70	-
Phyllitic Shales	20-30	0-5	0.20	3-10	5-20	20-40	60-75	10-30	-
Schistose Quartzite	70-80	-	-	5-15	-	10-20	0-5	-	95 - 100

TABLE 2
PARTICLE SIZE DISTRIBUTIONS OF THREE PROFILE TYPES

TYPE OF PROFILE	DEPTH (cm)	HORIZON	SAND 2-0.06 mm %	SILT 0.06 - 0.002 mm %	CLAY 0.002 mm %
1	0 - 5	A	70	20	10
Formed on crest of	5 - 60	B	40	25	35
quartzite and sandstone	60 - 85	Bcs	65	30	5
ridges	85 - 145	Bcs/C	70	28	2
2	0 - 5	A	63	25	12
Formed on colluvial	5 - 80	B	30	20	50
slopes of quartzite/ sandstone ridges	80 - 120	Bcs	60	22	18
	120 - 210	Bcs/C	72	19	9
3	0 - 10	A	55	30	15
Formed on colluvial	10 - 75	B	20	34	48
slopes underlain	75 - 110	Bcs	21	43	36
by phyllitic	110 - 200	Bcs/C	17	58	25
shales					

TABLE 3
APPROXIMATE MINERALOGICAL COMPOSITION OF THE WEATHERED QUARTZITE AND SOILS OVERLYING THE CREST AND COLLUVIAL SLOPES

	% WHOLE SOIL/ROCK SAMPLE							% CLAY SIZE FRACTION			
MINERALS SOIL & ROCK TYPES	Quartz	Carbonate	Gypsum	Feldspar	Iron Oxides	Pyrite	Clay Minerals	Smectite	Illite	Chlor-ite	Kaol-inite
Soils formed on crests of quart-zite and sand-stone ridges	13-60	0-45	0-20	2-30	0-3	-	17-30	0-30	10-30	-	50-70
Soil formed on colluvial slopes of quartzite ridges	30-40	0-5	0-35	0-5	0-5	-	25-60	25-75	10-20	-	17-30
Weathered quartzite bedrock	70-90	-	-	5-25	0-5	-	10-20	0-5	0-5	-	80-95

TABLE 4
APPROXIMATE MINERALOGICAL COMPOSITION OF THE WEATHERED PHYLKLITIC SHALE AND THE OVERLYING SOILS

	% WHOLE SOIL SAMPLE							% CLAY SIZE FRACTION			
MINERALS SOIL & ROCK TYPE	Quartz	Carbonate	Gypsum	Feldspar	Iron Oxides	Pyrite	Clay Minerals	Smectite	Illite	Chlorite	Kaol-inite
Soil over-lying the - phyllitic shale	25-45	2-30	0-35	0-10	0-5	-	15-45	7-30	30-60	0-30	0.30
Weathered phyllitic shale bed-rock	20-30	0-25	0-5	2-15	0-5	0-15	2-40	5-20	50-70	15-35	0-5

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A Geophysical Survey of Culoul and Mellong Creek Valley Fills: Implications for Valley Development in Sandstone Terrain

S. J. Riley and H. M. Henry

ABSTRACT. Geophysical surveys of the Mellong Plateau valley fills supplemented and calibrated against limited drilling revealed a system of wide valleys with up to 10 metres of fill. Streams with dimensions similar to those that presently occupy the valley floors may have excavated the valleys. If they did then there has been a complex history of valley aggradation and incision with the most recent period being one of aggradation.

INTRODUCTION

The Mellong Plateau, 80 km north-west of Sydney, N.S.W. Australia (Fig. 1) is an anomalous area of gentle relief in the midst of a highly dissected sandstone highland which is at least Tertiary in age (Bishop *et al.*, 1982). Several of the low gradient streams on the "plateau" flow westwards through highlands and against the general topographic dip which is towards the east (Fig. 1). There are a number of significant geomorphic questions posed by the area that require detailed analysis of the streams and their valley fills (Henry, 1987).

This paper describes a geophysical investigation of two areas in the Mellong Plateau designed to determine the extent of valley fills. Details of the sites and methods employed are given. The geophysical interpretations are tested against, and calibrated by, data obtained by augering, coring and thin-section analysis. Finally, some tentative interpretations of the development of the valley systems are given.

REVIEW

Very little work has been done on the valley fills of the sandstone plateaux of the Sydney region. Notable studies are those of Holland (1974), Buchanan (1979) and Young (1985, 1986). The majority of the studies have concentrated on the swamps, and in general have examined swamps developed in low order drainage basins. These swamps usually have shallow fills and commonly are characterised by reducing environments. There is a dearth of knowledge about the valley fills of the larger, low gradient plateau streams which are dominated by oxidising environments.

The larger upland valleys are of some significance because their form and orientation have often been cited as evidence for various theories of the tectonic development and the drainage evolution of eastern Australia as related to the Lapstone Monocline (see Henry, 1987). These arguments will not be reviewed here. There is an obvious need to describe the exact nature of the valley fills so that future discussion of these plateau valleys can be based on better information.

The Mellong Plateau was selected for this study because it represents an unusual form of terrain in the Sydney region and because it appears to be closely related to several structures that are of importance in the tectonic history of eastern Australia (e.g., the Lapstone Monocline).

Examination of valley fills in the Mellong Plateau is not easy. There are no significant exposed sections, so recourse has to be made to drilling and subsurface exploration techniques. The valley fills are composed of locally derived sandstone detritus which has been re-cemented to a hardness and morphology similar to weathered sandstone, hence the differences between bedrock, alluvium and colluvium are not marked in terms of mineralogy and texture. In the field there is real difficulty in differentiating between the the sandstone bedrock and the reworked alluvium and colluvium. Whenever the fabric is destroyed, as in augering, there is no significant difference between the Triassic bedrock sandstone and the Quaternary and possibly Tertiary valley fill material.

When this study was first begun by H.Henry it was initally thought that the valleys were Kehltal types (Louis, 1964) with shallow colluvial cover over highly weathered Triassic bedrock. The chance intersection of a thin bed of grey clay at approximately 3 m depth in a pit alongside Culoul Creek, below what had been thought

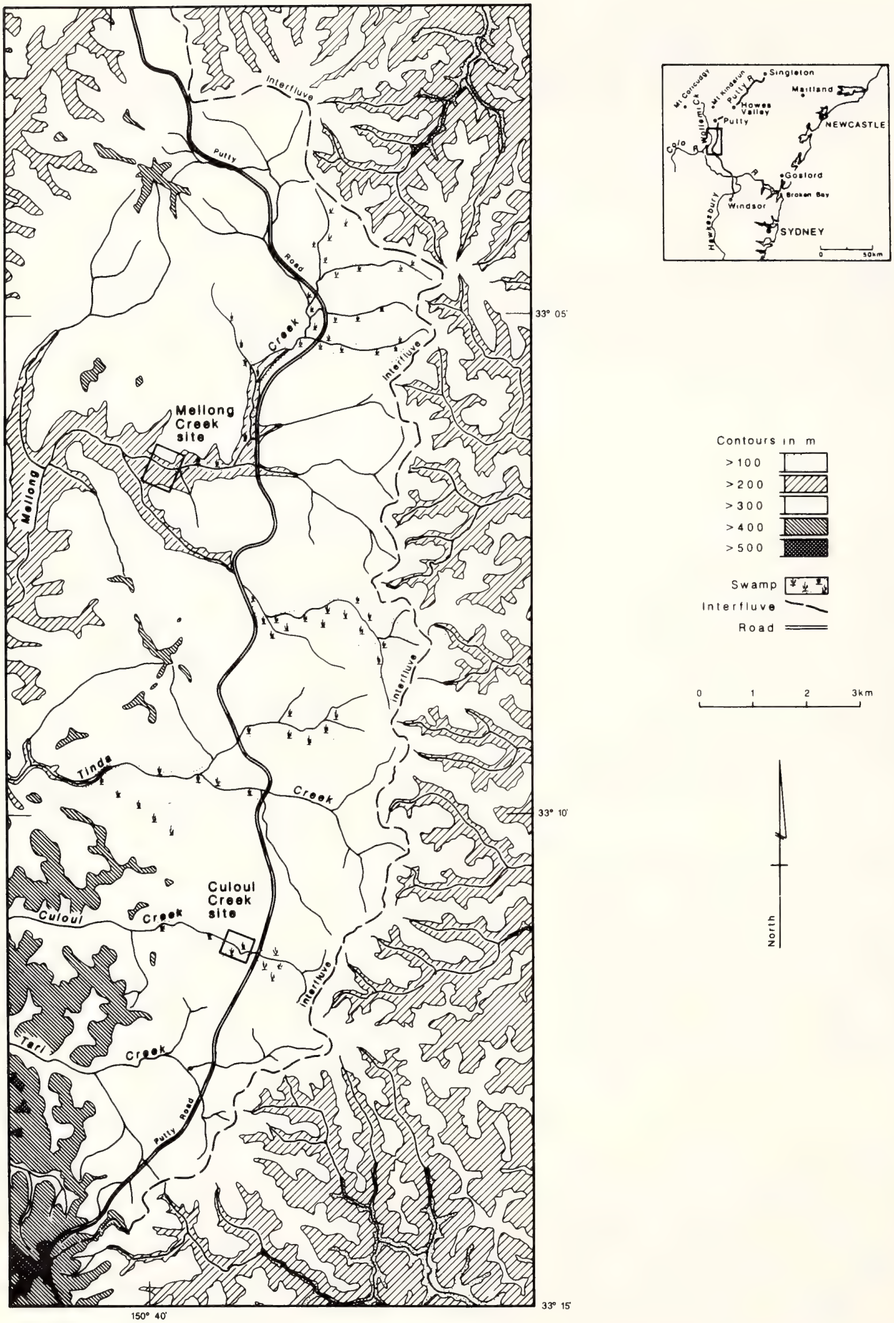


Figure 1. Location of Putty Road, Culoul and Mellong Creeks and two sites on the Mellong Plateau at which geophysical surveys and drilling was undertaken.

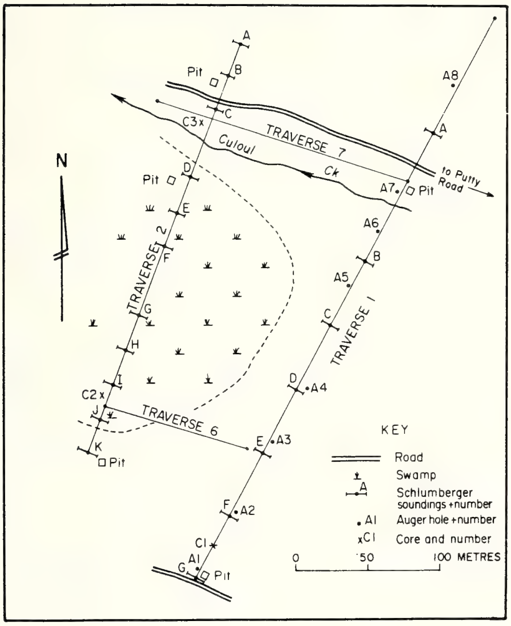


Figure 2. Culoul Creek site showing the location of traverses, auger holes, cores and resistivity soundings. Note, all surveying was relative to a local datum.

to be weathered *in situ* sandstone (see subsequent discussion), yielded pollen that is Quaternary in age. It was concluded that the valleys are far more complicated in alluvial and colluvial structure than had been originally envisaged.

Exploratory drilling with a powered auger was initially undertaken to define the extent of the valley fills but it did not produce conclusive evidence of the position of the bedrock contact. The weathered sandstone offered resistance to drilling similar to that offered by the cemented valley fill and the retrieved samples were similar. Thin-sections were needed to define the *in situ* bedrock material in a conclusive way. However, in order to obtain undisturbed samples for thin-sections an extensive program of coring would have been required - a time consuming and costly exercise.

Geophysical investigation using refraction and resistivity methods is a low cost alternative to an extensive program of coring. It is a major aim of this paper to show the value of these geophysical techniques in delineating the bedrock contact and the stratigraphy of the valleys as well as to discuss the results of the investigation.

SITE DESCRIPTION

Two sites were selected, one on Culoul Creek, 500 m west of the Putty Road and the other on Mellong Creek, 2 km west of the Putty Road (Fig. 1). Each of these streams occupy wide shallow valleys with a number of swamps. They flow against the general topographic gradient and are two of the largest streams on the Plateau. It is argued that their genesis, geometry and stratigraphy are typical of other streams in the area. More details of the sites are given in Henry (1987).

The Culoul Creek site (Fig. 2) straddles Culoul Creek and extends across a swamp to the south of the stream. The valley is at least 600 m wide in the area investigated. Investigations concentrated on the valley floor, but traverses extended upslope onto the colluvial covered footslopes (Unit 6 of the 9 unit landsurface model; Dalrymple *et al.,* 1968). The upper 0.5 m of the fill has been disturbed by man in places. Limited channelization in Culoul Creek will not influence the present study. Colluvial slopes are covered in open sclerophyll forest.

The Mellong Creek site is to the south of Mellong Creek and immediately upstream of a bedrock constriction (Figs. 1 and 2). A sandstone escarpment runs parallel to the stream 50 to 80 m north of the stream (this precluded the need for surveys on the north side of the stream). Bedrock also crops out 400 to 600 m south of the stream in an escarpment which has large re-entrants (or embayments), at the centres of which are alluvial fans of low angle. The area appears to have been minimally disturbed by man. Investigations extended from the creek south to the bedrock escarpment (Fig. 3).

METHODOLOGY

Seismic refraction and resistivity surveys are frequently used to define the bedrock geometry of infilled valleys (e.g. Dury, 1962; Watkins and Spieker, 1971; Mangun, Kunze and Szebo, 1981; Overmeeren, 1981). However, in the majority of these studies the contrasts between the fill and bedrock are substantial. For the Mellong Plateau, where the bedrock and fill are composed of similar materials, there is no certainty that the geophysical methods will successfully differentiate the contact. The general problems of refraction surveys in shallow materials are given in Domzalski (1956), Green (1974), Whiteley and Greenhalgh (1979) and Sjogren (1984).

Refraction surveys were undertaken along a network of interlocking traverses. Seven traverses were surveyed, four at Culoul Creek (Fig. 2) and three at Mellong Creek (Fig. 3). The Mellong Creek traverses were tied to bedrock.

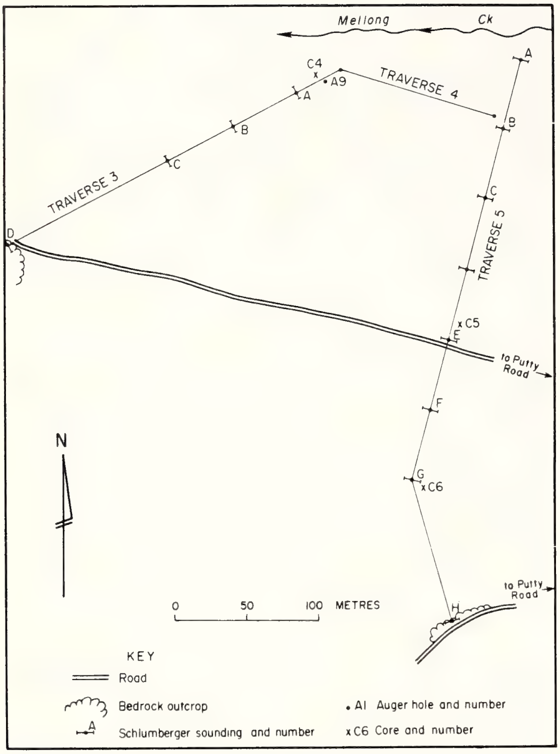


Figure 3. Mellong Creek site showing the location of traverses, auger holes, cores and resistivity soundings. Note, all surveying was relative to a local datum.

Resistivity surveys were undertaken along the four traverses that are transverse to the stream and valley axes. Details of the methods are given in the following.

Seismic refraction

A Geometrics ES-1210 12-channel signal enhancement seismograph recorded sledge-hammer generated signals. A 2 cm thick steel striker plate, approximately 15 cm square, provided contact with the ground. An inertial switch was attached to the hammer. It was not uncommon for the plate to be forced at least 5 to 10 cm below the surface with the 1 to 10 blows needed to generate the signals required to satisfactorily record the time of arrival of the first wave.

Except for traverse 1, geophones were spaced at 10 metre intervals over a 100 m long line. Shots were taken from either end of each geophone spread (reciprocal shots). Weathering shots, spaced at 1 m intervals between the first two and last two geophones of each spread, defined the seismic velocities of the upper fill material. Shots 30 m from the end of each line were also taken in order to confirm the velocity characteristics of the major refractors (by providing overlapping curves on the time-distance diagrams).

In the case of traverse 1 geophones were spaced at 15 m intervals over 165 m long spread. Overlapping shots were placed 45 m from the ends of each line.

The first arrival times are clearly identified for spreads of 100 m. Some difficulty was experienced in identifying the first arrivals when the distance from geophone to signal source exceeded 100 m. The problem was accute at several places because of noise generated by swaying trees. Road traffic caused minor problems at Culoul Creek.

Time-distance diagrams were interpreted by the reciprocal method (Hawkins, 1961) and by half intercept times (Dobrin, 1976). There are suspected velocity inversions in the fill (Figs. 4 and 5) and some sections of the fill may also have velocities that gradually increase with depth.

Resistivity

Vertical soundings were undertaken along four traverses (1,2,3 and 5) using the Schlumberger four electrode system (Keller and Frischknecht, 1966). Electrode arrays were oriented downvalley in order to reduce complications that occur with sloping contacts (Koefoed, 1979, p.231). The need to follow the strike of the contacts precluded soundings on traverses oriented along the valleys.

Array length (between electrodes) extended to 100 m for most soundings, although several extended to 200 m. Wider arrays were not undertaken because preliminary augering suggested depths of fill between 5 and 10 m. Furthermore, the available coring equipment could not extend to depths greater than 20 m.

Ninety five percent of potential readings were in the mV range. An AC current of 2.5 Hz was used. Electrodes were brass pegs, approximately 2 cm in diameter.

Apparent resistivites ranged from several thousand to hundreds of ohm-metres (Fig. 4). The highest readings were associated with sand covered surfaces (e.g. traverse 5). Resistivites were low in the swamps.

Initial interpretations were graphically derived using methods outlined in

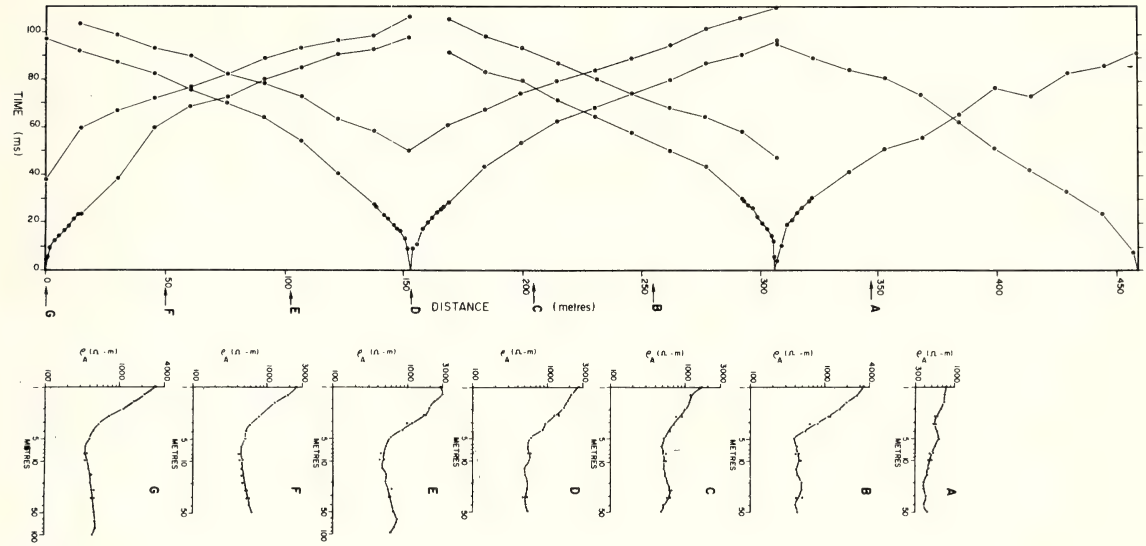


Figure 4a. Resistivity and refraction results for traverse 1.

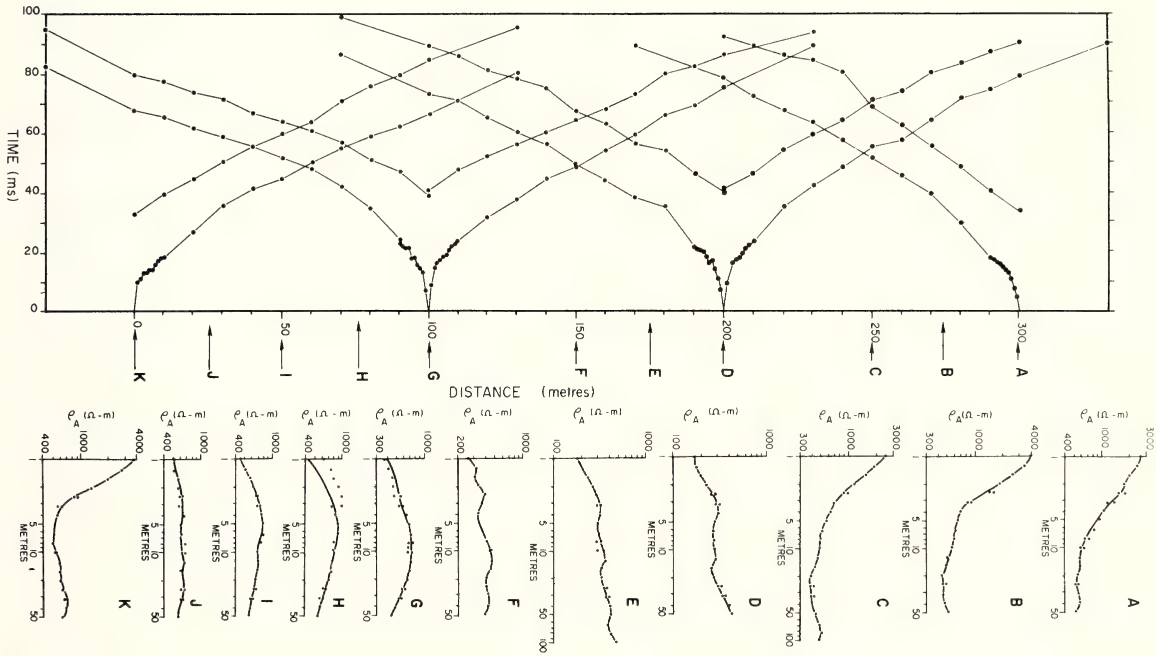


Figure 4b. Resistivity and refraction results for traverse 2.

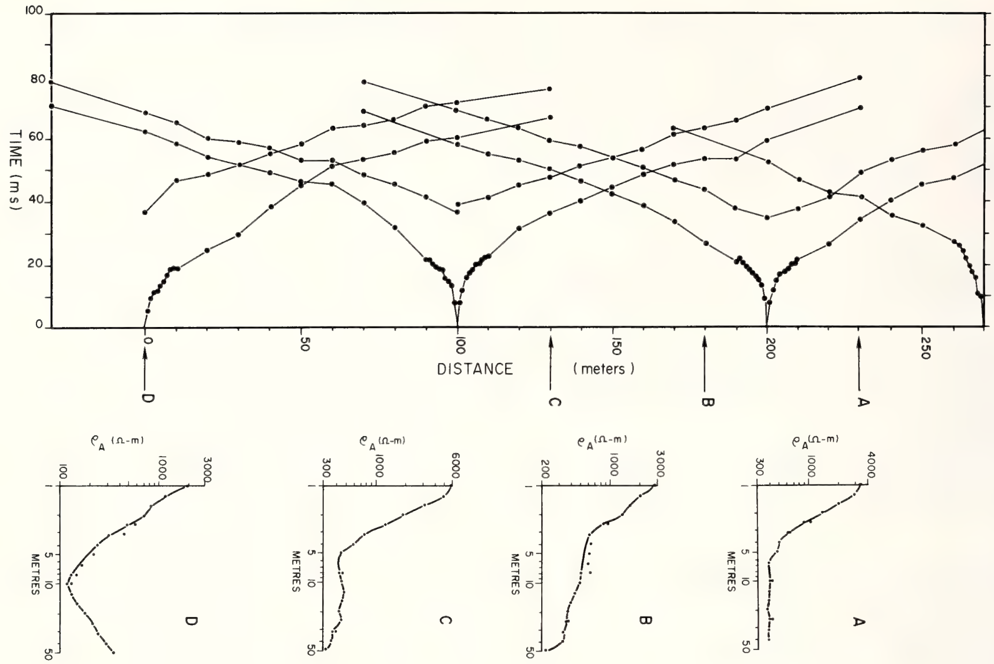


Figure 4c. Resistivity and refraction results for traverse 3.

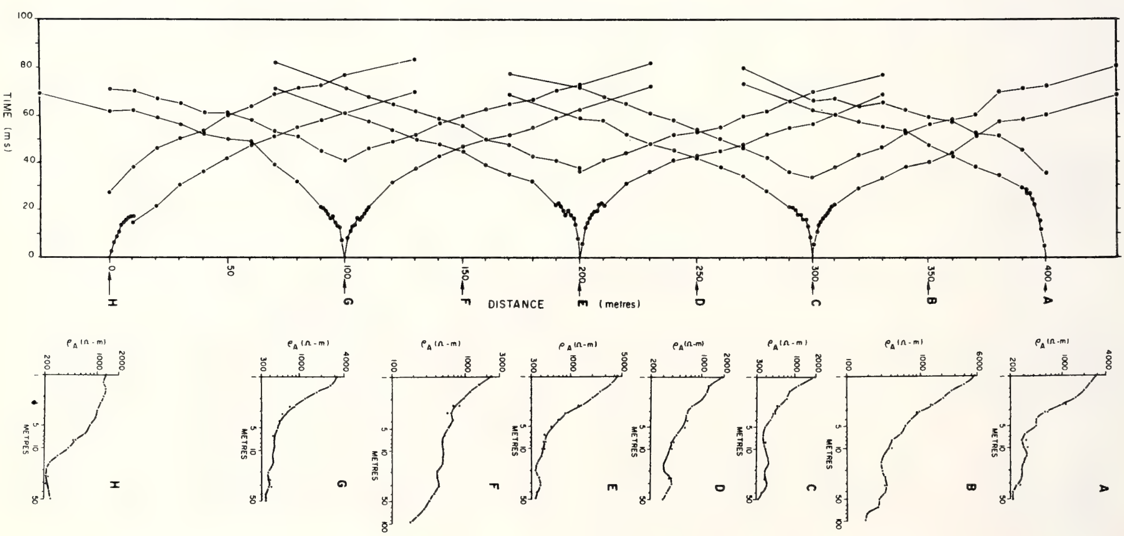


Figure 4d. Resistivity and refraction results for traverse 5.

Orellana and Mooney (1966). Final modelling was undertaken using the program of Jupp and Vozoff (1975). The goodness of fit of the models is high (Table 1).

INTERPRETATION

An attempt was made to define the bedrock contact using the individual techniques. It was found that refraction suggested considerable depth to bedrock at several sites. From the available auger drill information these depths are unlikely. Errors probably result from velocity inversions within the sections or from gradational boundaries where the bedrock interface is obscured by weathering. Resistivity modelling did not give unambiguous interpretations of layer thickness. The assumption of simple horizontal beds of near infinite extent is often not true in shallow valley fills.

Because of the obvious problems of interpretation using the individual techniques it was necessary to combine the analyses of the data in the interpretation stage. The resistivity and refraction data were not analyzed in isolation from each other and the final interpretation, which combines the analyses, will not show the merit of either technique relative to the other. There are unresolved differences between the two techniques in the positioning of bedrock. These differences are most clearly seen in traverse 5, whose stratigraphy is the most complex of the four traverses (Fig. 9).

The original resistivity and refraction data are presented in diagrammatic form (Figs. 4 and 5) to enable the reader to assess the nature of our interpretations. The most important guideline that we used in developing interpretations was that the depth of the fill should be greatest towards the centre of the valley and shallowest on the margins. In applying this guideline attention was paid to the geometry, particularly slope, of the bedrock walls on the valley margins. All interpretations were undertaken before the coring program.

Grain size analysis was undertaken using the methods of Folk (1974). Bedrock identification of cores was on the basis of microscopic analysis of thin sections of cored material undertaken by Henry and Conaghan (see Henry, 1987). Transported material was differentiated from bedrock by the presence of disjunctive grain fabrics and grain margin modifications.

Drilling was undertaken with a GEMCO GH4 trailer-mounted drill. Coring was not continuous. Cores were taken with a thin walled sampling tube using a drop hammer at depths considered significant in terms of presence or absence of bedrock.

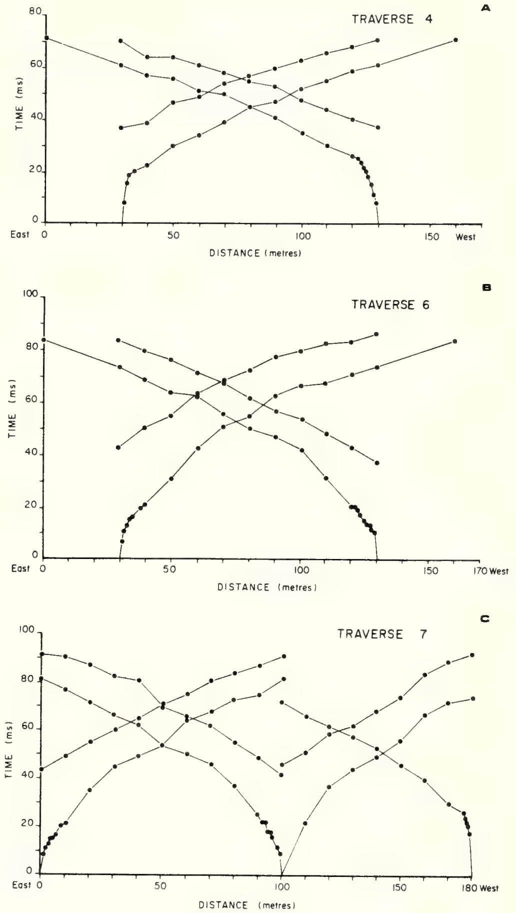


Figure 5. a,b,c. Seismic refraction time-distance diagrams for traverses 4, 6 and 7 respectively.

RESULTS

Traverse 1: Culoul Creek

Two distinct refractors are evident from the time-distance diagram (Fig. 6). The upper layer is between 0.5 and 1.5 m thick and corresponds to the upper soil in which bioturbation has opened the fabric. The second layer has velocities between 600 and 1000 m/sec and is interpreted as the weathering zone or alluvial/colluvial fill. The lower layer, with velocities in excess of 1500 m/sec is undoubtedly less weathered or unweathered bedrock. The lack of distinct breaks of slope in several of the time-distance curves suggests a gradual increase in seismic velocity between the second and third layers (Fig. 4a). This gradual transition is

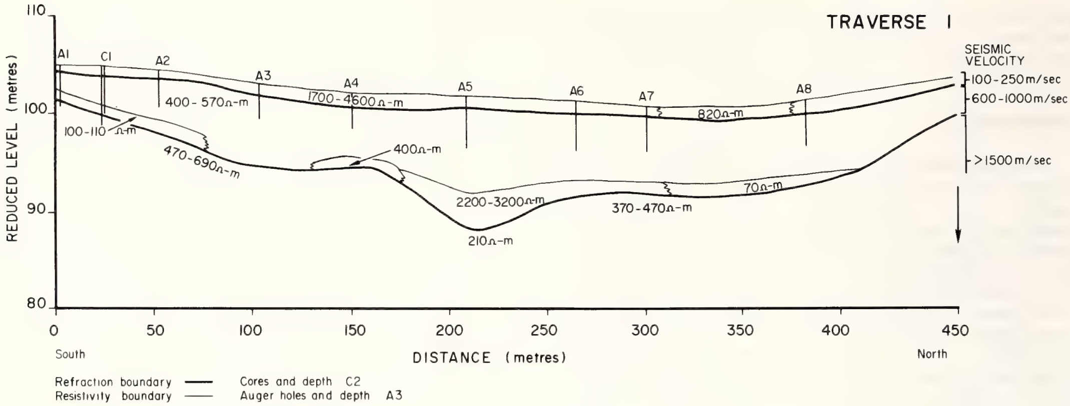


Figure 6. Interpretation of geophysical survey of traverse 1. The solid lines indicate the boundaries of seismic refractors. The seismic velocity is given on the right side of the cross section. The thin lines indicate the boundaries of layers defined by the resistivity survey, the values of resistivity in ohm-metres is given within the diagram.

consistent with a progression from weathered to unweathered bedrock.

Layer depth and resistivity is not constant along the traverse (Fig. 4a and 6). Models generally fit the data with a small error although the goodness of fit is less to the north of the traverse (Table 1). There may be several thin beds of significantly different resistivities in the profile but the data do not allow them to be defined precisely. There is an upper layer of poorly consolidated sand which corresponds to the upper of the 3 layers defined by the seismic survey. A second layer, with resistivities between 400 and 570 ohm-metres is also consistent along the traverse and is interpreted as a partially, if not fully, saturated sand-clay deposit. The basement material has a resistivity of between 400 and 600 ohm-metres, although resistivity is lower in the deepest section of the profile.

The model of the subsurface stratigraphy of traverse 1 shows a valley fill, approximately 10 m deep at the centre of the traverse. On the colluvial footslopes the fill is 2 to 5 m deep. A clay lens may occupy the deepest section of the fill. The bedrock interface is capped with a low resisitivity layer in two sections; possibly saturated sands perched on top of the less permeable bedrock.

A number of auger holes were located along traverse 1 in the early part of the study (Figs. 6 and 10). Except for auger hole 1 all failed to reach what is recognized as bedrock by the seismic investigation. The textural information gives no clear evidence of bedrock presence or absence (Table 2). Two cores were retrieved from the southern end of the traverse (C1) near auger hole 1. Both interesected bedrock and the thin sections cut from them show bedrock at a depth of approximately 10 m, which corresponds well with the seismic interpretation. The subsurface bedrock profile of traverse 1 shows a wide valley with a smaller inset-valley and overlapped terrace or structural bench to the south.

Traverse 2: Culoul Creek

Three layers are indicated by the seismic refraction survey (Fig. 7). These layers have the same characteristics as those defined by the seismic refraction survey of traverse 1. The bedrock contact, as indicated by the refraction data, is irregular. These irregularities are also indicated by the resistivity survey.

Except for the resistivity survey at 275 m the models fit the data with a small error (Table 1). A general pattern of a thin surface layer, a thick second layer and a thin layer on bedrock is discernable. The resistivity of the inferred bedrock is similar to that for the inferred bedrock of traverse 1. There is a discrepancy between the depth to bedrock indicated by resistivity and that indicated by seismic refraction at either end of traverse 2.

No auger holes were located on traverse 2 although pits are located at the northern and southern ends (Fig. 2). Three cores were retrieved from the traverse, one at the southern end and two at the northern end (Fig. 7). The two at the northern end are close to each other and

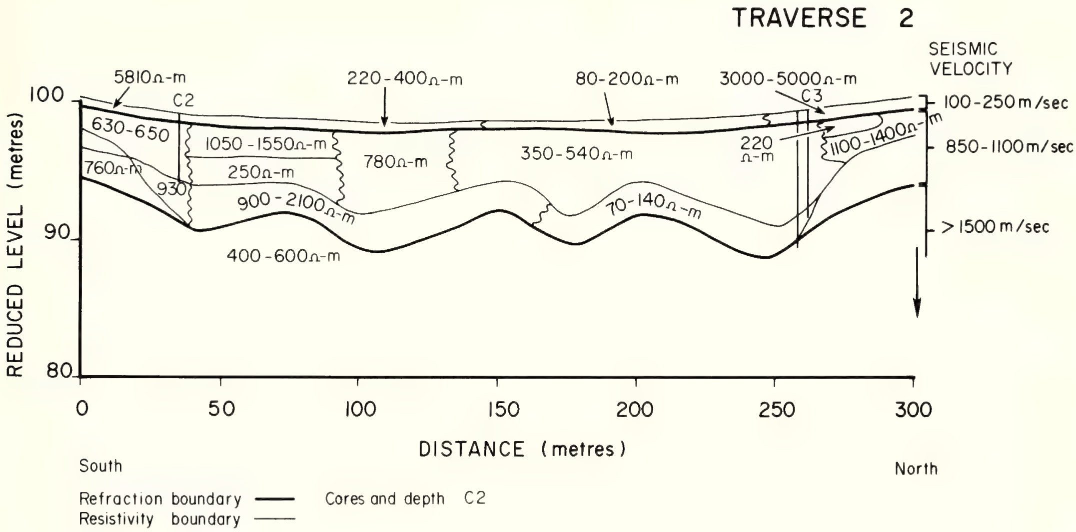


Figure 7. Interpretation of geophysical survey of traverse 2. The solid lines indicate the boundaries of seismic refractors. The seismic velocity is given on the right side of the cross section. The thin lines indicate the boundaries of layers defined by the resistivity survey, the values of resistivity in ohm-metres is given within the diagram.

were obtained at two different times because it was necessary to repeat the drilling when it was discovered that sufficient core had not been obtained from the first hole. The two holes and resultant core are treated as one (C3). It was in a pit excavated in the northern end of traverse 2 that a thin bed of grey clay was intersected which yielded Quaternary pollen (Henry, 1987). The southern core probably did not reach bedrock, but bedrock was found in the northern core at a depth of 9 m.

The reduced level of the inferred bedrock contact of traverse 2 is similar to that of traverse 1. However, the depth of fill of traverse 1 is greater than that of traverse 2.

Traverse 3: Mellong Creek

The seismic refraction data suggest a deep channel at this constriction point on Mellong Creek (Fig. 3). There is an upper layer of sand and thick bed of fill, pinching out to the bedrock contact in the south and thinning to the north (Fig. 8).

The resistivity survey confirms the refraction data (Fig. 4). There is considerable variation in the resistivity of the inferred bedrock material, ranging from 130 ohm-metres in the south to 500 ohm-metres in the north. This variation probably results from differences in saturation and degree of weathering.

An auger hole and core are located at the northern end of the traverse (Figs. 8, 10, Tables 2 and 3). Bedrock is indicated at 7 m. The site was one of the few at which augering and drilling indicated bedrock during the drilling phase, namely, there was a drammatic increase in resistance to penetration over a depth of less than 0.5 m.

There is a fill in the deepest section of the bedrock profile which may be clay rich material.

Traverse 5: Mellong Creek

The subsurface stratigraphy of traverse 5 is more complicated than that of the preceding traverses. There is less certainty about the position of the bedrock contact.

Seismic refraction suggests an undulating bedrock profile with the fill deepening towards the north (Fig. 9). The bedrock contact is lower at traverse 5 than it is at traverse 3, which suggests a reversal of valley slope. However, the difference in elevation is less than 1 m, which is within the error of the techniques.

The three layers with velocities of 100 to 300, 700 to 1100 and greater than 1500 m/sec found in traverses 1, 2 and 3 are also present in traverse 5 (Figs. 4d and 9).

Table 1

Goodness of fit of resistivity survey.

Position (m)	Standard error	Noise to signal ratio	Mean% error	Average predicted residual error (%)
Traverse 1				
0	4.09	3.59	3.34	4.22
50	3.86	4.62	2.96	4.03
100	6.89	5.01	5.96	7.03
160	9.37	9.77	7.31	8.99
210	8.30	13.8	6.48	9.43
260	15.3	12.8	11.85	14.46
350	4.59	11.7	3.59	4.52
Traverse 2				
0	3.97	4.05	3.04	3.96
25	3.83	32.5	2.99	3.63
50	2.30	10.6	1.63	2.73
75	1.94	6.14	1.37	2.16
100	2.28	7.16	1.78	2.50
150	7.36	31.3	5.75	9.63
175	5.54	13.1	4.52	5.50
200	6.35	16.4	4.96	6.70
250	2.81	2.84	2.29	2.90
275	14.1	11.8	10.91	13.98
300	4.66	4.12	3.96	4.64
Traverse 3				
0	15.1	9.27	12.80	15.46
130	7.10	4.59	5.32	6.85
180	9.63	6.83	8.08	10.67
230	5.66	5.27	3.88	5.36
Traverse 5				
0	6.49	5.07	4.59	6.24
50	8.67	5.54	6.54	9.59
100	6.44	9.28	4.55	6.32
150	8.22	10.1	5.81	10.23
200	8.67	6.89	6.12	8.45
250	6.55	6.83	4.95	7.01
300	5.50	5.39	3.89	5.45
400	4.64	3.72	3.63	4.46

Resistivity survey suggests considerable inhomogeneity in the fill and in the bedrock. The models fit the data with only a small error (Table 1).

In the north, over the top of the inferred bedrock, a high resistivity layer of 1 to 2 m thick underlies a low resistivity layer of similar thickness. It is possible that the high resistivity layer is a clay rich valley fill covered with a saturated or near saturated sandy layer. Alternatively, a case could be made that the high resistivity layer is weathered bedrock.

Bedrock resistivities range between 50 and 300 ohm-metres. These values are less than those indicated in the other three traverses, but are not significantly different. For example, in traverse 3, exposed bedrock has a resistivity of 130 ohm-metres (Fig. 8) The difference in the resistivity of bedrock of traverse 5 may be a result of different lithologies as well as different degrees of weathering and saturation.

Three cores are located on traverse 5, at c6 and c5, two of which are located near each other, namely c5 (Figs. 3 and 9). Bedrock was not identified at 4 m in c6 whereas at c5 it is at a depth of 6 m or possibly more. It is assumed that c6 intersected bedrock at approximately 10 m because the thin walled sampling tube failed to penetrate the material at the bottom of the hole and in fact was severely bent. There is no doubt that the combined resistivity and refraction surveys are consistently identifying the bedrock contact.

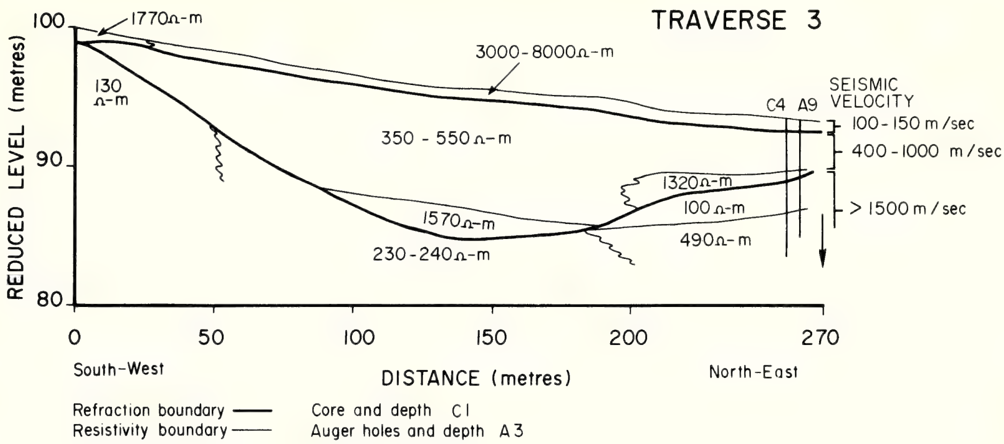


Figure 8. Interpretation of geophysical survey of traverse 3. The solid lines indicate the boundaries of seismic refractors. The seismic velocity is given on the right side of the cross section. The thin lines indicate the boundaries of layers defined by the resistivity survey, the values of resistivity in ohm-metres is given within the diagram.

Table 2

Textural analysis of samples from selected auger holes
Culoul and Mellong Creeks

Traverse number	Auger Hole number	Depth (m)	Gravel	Sand	Silt	Clay
			Percentage of			
1	A1	0.5	0.5	78	15	7
		1.2	2.2	53	9	36
		2.7	0.3	70	11	19
	A2	0.5	0.2	79	11	10
		1.2	0.5	71	12	16
		2.0	0.3	58	11	31
		2.7	0.4	62	10	28
		4.1	0.4	65	9	25
	A6	0.2	0.	86	11	3
		0.5	0.	79	7	14
		1.8	0.	9	21	70
		2.1	0.	33	17	50
		5.8	.3	57	9	34
		7.3	4.0	74	3	19
		9.1	0.6	67	9	23
	A7	0.5	0.8	74	10	15
		1.2	0.3	62	7	31
		4.3	0.2	75	6	19
		7.3	0.1	50	15	35
3	A9	0.6	2.7	74	11	12
		1.2	1.0	58	11	30
		2.6	0.4	59	14	26
		7.2	1.5	65	8	25
		7.8	Drill would not penetrate further			

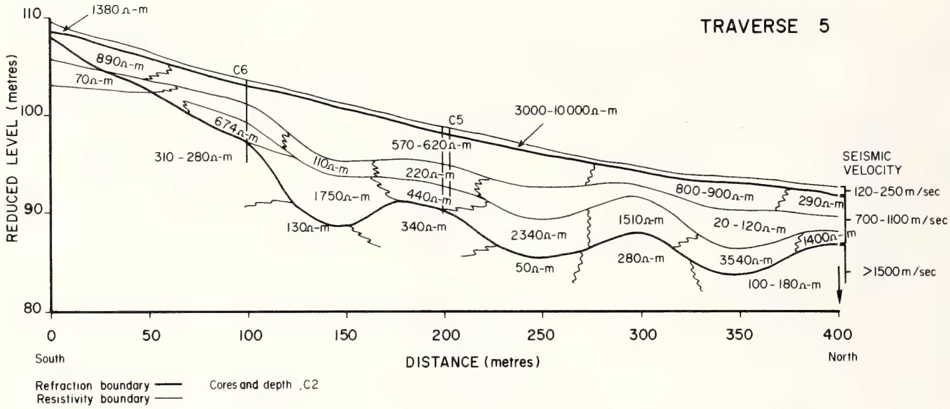
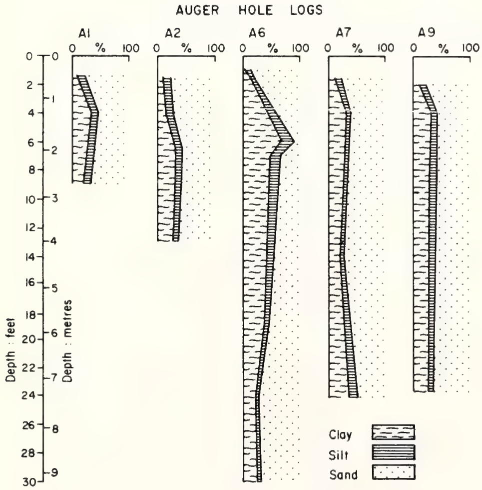


Figure 9. Interpretation of geophysical survey of traverse 5. The solid lines indicate the boundaries of seismic refractors. The seismic velocity is given on the right side of the cross section. The thin lines indicate the boundaries of layers defined by the resistivity survey, the values of resistivity in ohm-metres is given within the diagram.



DISCUSSION

There is some ambiguity in the definition of bedrock using either resistivity or refraction techniques alone. However, when the techniques are combined and a certain degree of rationality is imposed upon decision making by the geometry of the valley the techniques appear to define the bedrock contact. In some cases there is ambiguity resulting from differences obtained from either technique. The probable errors are only of the order of 1 to 2 m in the deepest section.

Refraction consistently shows 3 layers, an upper soil layer of 100 to 200 m/sec, a layer of fill, with velocities between 700 and 1100 m/sec, and bedrock with velocities in excess of 1500 m/sec.

The resistivity survey shows considerable inhomogeneity in the stratigraphy of the traverses. The upper soil layer is consistently defined. In the case of sand it has high resistivities and in the case of swampy areas the resistivity is low. The alluvial fill, below the upper soil horizon, has a range of resistivities for each of the sites, depending on degree of saturation and lithology. A majority of the sites have fill with a resistivity between 350 and 800 ohm-metres. Bedrock has a lower resistivity than the fill at some sites, but not at all sites. There is no clear distinction between alluvial and colluvial fill and bedrock on the basis of resistivity alone.

The investigation suggests that geophysical methods may be used to define the extent of valley fills in the Mellong Plateau but that it is necessary to undertake some coring, or at the very least augering to confirm the reliability of the interpretations. There is a potential error in the estimate of depth to boundaries of the order of 1 m and the two methods of refraction and resistivity do

Table 3

Bedrock-transported characterisation from some thin sections taken from cores; Culoul and Mellong Creeks

Traverse number	Core hole number	Depth (m)	Description
1	c1	2.7	Probably transported
		4.4	Probably transported.
		6.1	Probably transported.
		12.2	Possibly bedrock
2	c2	5.5	Probably transported
		6.1	Probably transported.
		8.5	Probably transported.
		9.4	Claystone, probably bedrock
2	c3	5.8	Probably transported.
3	c4	6.1	Probably transported
		7.3	Probably bedrock.
5	c5	4.3	Probably transported.
5	c6	3.0	Probably transported
		6.2	Might be transported.

Further details in Conaghan and Henry (in preparation).

not always yield data that support each other. The problem of ambiguity in the thin-sections is referred to in Henry (1987) and will be more fully discussed in a later paper (Conaghan and Henry, in prep).

Valley floor development

Studies elsewhere in the sandstone terrain of the Sydney region have indicated the significance of catastrophic events in scouring valley floors and causing significant change in channel geometry (Henry, 1977; Erskine, 1986). Young (1986) argues that the 'topogenous mires' (upland swamps) are probably excavated on occassions by the concurrence of large floods and surfaces devegetated by bushfires. The model probably works well for small scale features in valley floors but cannot take account of fills of larger valleys.

The inhomogeneity in valley fill shown by the resistivity survey suggests a complex history of aggradation and degradation. The latest phase has probably been one of aggradation. The degree of cementation of the majority of the fill by iron suggests a considerable period for emplacement of the fill. Unfortunately there are no quantitative data on the rates of iron cementation so it is not possible to assess whether the deposits are of Holocene age or older.

At no time was any carbonaceous material retrieved other than in the upper near surface layers. The oxidising environment that dominates the area is not conducive to the survival of organic material. One would expect that the frequent bushfires would produce ample charcoal, but none was found in the deeper layers.

There is an absence of cemented colluvial and alluvial material at the surface and in fact the surface of the valley floor is covered with a thin veneer (up to 1.5 m thick) of loose sand which is organically stained in the top 20 to 40 cm. The only sites at which the cemented material is found is in the bed or creeks that have incised to a depth in excess of 1 m. It is possible that bioturbation operates so rapidly that no cemented material will survive at the surface for any length of time before it is broken down into its constituent particles (Humphries and Mitchell, 1983). If the valley floor and slopes were in a phase of either degradation or stability some cemented material would be found, even if in fragments. It is unlikely that bioturbation would destroy all the cemented material and reduce it to a disaggregated mass of sand and clay. Possibly the sites have not been found, however it is more likely that the most recent period of valley development on the Mellong Plateau has been one of aggradation with the covering of the surface by colluvial and alluvial sands. Whether this aggradation was triggered by man (western and aboriginal), by

climatic change, or by some other factor is unknown.

There is no evidence of streams with morphologies different from the present ones preserved in the valley floor morphology or evident in the stratigraphy. Streams similar to the present, which are discontinuous, may have been present during the entire period of evolution of the valleys. The interspersal along the stream line of swamps, deep pools (resembling a chain-of-ponds, Eyles, 1977) and small channels (3-5 m wide and 1-2 m deep) suggests a considerable spatial heterogeneity in channel development which probably means that temporal development of streams is spatially heterogeneous (if the ergodic argument can be applied).

The program of sampling and coring undertaken in this study was aimed at describing the stratigraphy of the valleys. The techniques were not suitable for retrieval of samples for thermoluminescent or palaeomagnetic dating. Absolute dating should be the subject of a future project in the area.

CONCLUSION

Resistivity and refraction surveys of valleys in the Mellong Plateau defined the extent of valley fills which are much deeper than had been anticipated. The valley fills at this stage cannot be used as evidence for climatic or tectonic changes in the region. It appears that the latest phase of geomorphic change has been aggradational, but there is no evidence to suggest what triggered the aggradation.

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S.J.Riley
School of Earth Sciences
Macquarie University 2109
Australia

H.M.Henry
11 5th Avenue
Cremorne 2090
Australia

(Manuscript received 3.11.1986)
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Mellong Plateau, Central Eastern New South Wales: An Anomalous Landform

H. M. HENRY

ABSTRACT. The Mellong Plateau, between Colo Heights and Putty in central eastern New South Wales, lies east of the topographically higher Lapstone Monocline (Fig. 1); through the latter the plateau's creeks drain west in transverse valleys against the regional topographic gradient. In spite of its location on top of the Macdonald River -- Wollemi Creek drainage divide, the plateau is extensively alluviated, large swamps occurring on all main creeks and tributaries. The plateau's anomalously low relief; wide, shallowly incised valleys; valley-side embayments; and westerly-flowing transverse drainage are described. More than 200 samples of fill were collected to depths up to 10 m; 71 of these were analysed granulometrically. Clay beds were less common in the fill than anticipated; clayey and muddy sand predominated. Difficulties were experienced in determining the alluvium/colluvium and bedrock boundaries, so seismic and resistivity methods were used to supplement drilling, and a limited coring programme was undertaken with thin-section examination of core fabrics. Alternative explanations of Mellong Plateau transverse drainage are discussed. Gaps in present knowledge, e.g. the age of the Lapstone Monocline, speculative existence of a vanished former cover, do not permit any positive conclusion, but genetic diversity seems indicated.

INTRODUCTION

Situated between the Colo River and Putty in central eastern New South Wales, the Mellong Plateau is a conspicuous low relief feature on topographic maps e.g. St. Albans, N.S.W., 1:100,000, sheet 9031 (Division of National Mapping, 1973); and Landsat imagery, e.g. digitally enhanced 01034-23185, Bathurst Scene (C.S.I.R.O., 1972). Surrounded by dissected ridges and gullies of regional extent, the plateau's flatness is anomalous. The plateau is 20 km long and 3 to 8 kms wide and it extends from the foot of Culoul Range 20 km north of Colo Heights to Staircase Hill south of Putty (Fig. 1). The plateau surface rises imperceptibly to the east, culminating in a topographically very subdued rim-ridge known as the Mellong Range. This range is 340-390 m high in the subject area, only a few metres higher than the adjacent Mellong Plateau to its west (average elevation approximately 300-340 m), but it is topographically significant because for its entire length from Colo Heights to Kindarun Mountain it forms the drainage divide between the Macdonald River and Wollemi Creek, both streams of regional importance. The plateau's creeks are examples of transverse drainage flowing west from the plateau's western boundary through elevated land 200 m higher than the Mellong Range, and hence against the regional topographic gradient. This westerly-climbing first-order topographic feature is considered to be the northern extension of the Lapstone Monocline from the Colo River (Galloway 1967; Bembrick *et al.* 1980).

This paper examines the erosional and depositional features of the Mellong Plateau discussing its geomorphic features, in particular the wide, flat valleys, valley fill, transverse drainage and the plateau's survival as a relict landform.

PLATEAU-IN-PLATEAU FORM

Bembrick *et al* (1980) divided the Sydney Basin into eight "structural units" or physiographic elements, six of which were described as plateaux -- the Blue Mountains, Hornsby, Woronora, Illawarra, Sassafras and Boyne Mount Plateaux. Topographic variation within some of the plateau areas permits sub-areas being distinguished as separate plateaux, e.g. Newnes Plateau (Blue Mountains), and Somersby Plateau (Hornsby): in this sense the Mellong Plateau is a component part of the Hornsby Plateau.

Unlike other sub-plateaux of the Sydney Basin, the flat surfaces of which form the high levels of their respective landscapes, the Mellong Plateau consists of wide, shallowly incised valleys lying between topographically subdued interfluves, the latter increasing in height westward. In fact it is the valley surfaces west of the Mellong Range that form the Mellong Plateau -- the interfluves more properly define the relict surface of the Hornsby Plateau.

The whole area, comprising both interfluves and valley floors, is referred to as the Mellong Plateau in this paper.

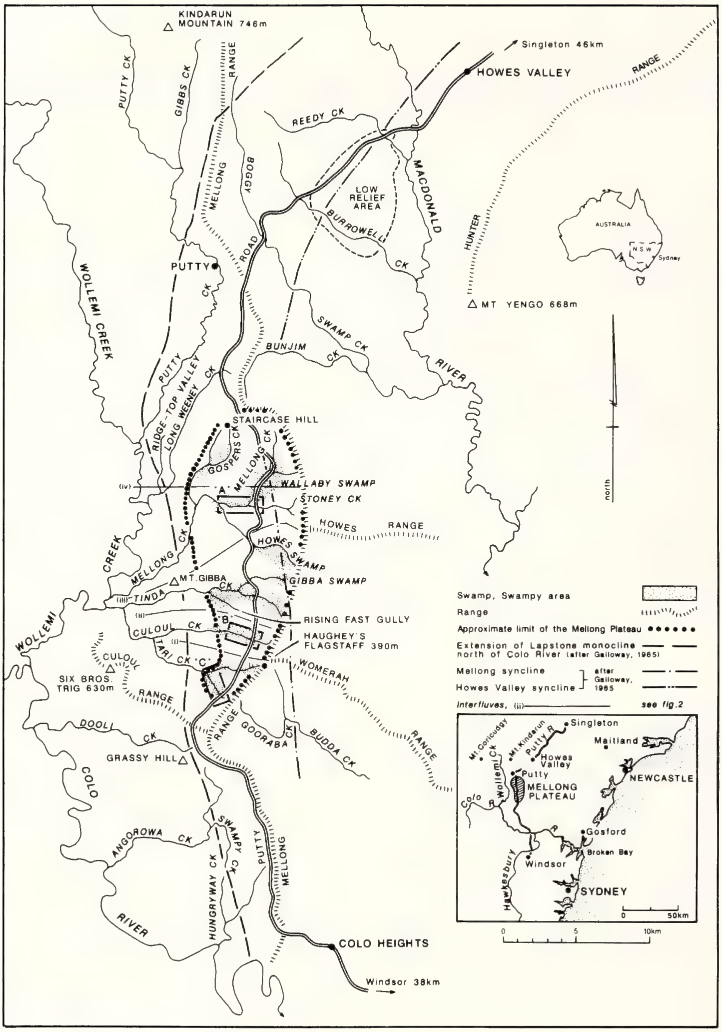

Figure 1 Locality Map of the Mellong Plateau Area

LAPSTONE MONOCLINE AND MELLONG SYNCLINE

Lapstone Monocline

M.C. Galloway (1965, 1967), mapping the Upper Colo-Putty area, described it as structurally dominated by the Lapstone Monocline. Galloway adopted the base of the Hawkesbury Sandstone as his main mapping horizon because it was the stratigraphic boundary most nearly ubiquitous in his study area; he acknowledged the difficulty of identifying the Hawkesbury Sandstone - Narrabeen Group boundary with certainty.

Galloway found the inclination of the Lapstone Monocline between Upper Colo and Culoul Range to be 2° much less than south of the Colo River, increasing to 3° between Culoul Range and Putty in the Mellong Plateau area. Recent fieldwork by the author has shown that such dips are exceeded at sites as much as 3 km east of the Lapstone Monocline as delineated by Galloway. 23 readings with mean dips of 12° (azimuth 99°) were obtained in a cutting on the Putty Road near Grassy Hill (grid reference 813203 on St.Albans 1:100,000 topographical map, Division of National Mapping, 1973: all grid references refer to this map); at the head of Angorowa Creek (grid reference 812176) the mean dip from 11 readings was 12° (azimuth 92°); on an east ridge 1 km from the Putty Road (grid reference 819190) 11

readings gave a mean dip of 8° (azimuth 86°). Galloway's finding that most primary bedding planes are flat-lying with dips not exceeding 3° was confirmed, but the data indicates that the monocline is not a simple linear structure, and in the Culoul Range-Grassy Hill area at least its effects extend east of Galloway's line. This is in accordance with the conclusions of recent workers -- see Branagan (1975), Pedram (1983), and Quereshi (1984) for references to the topographic character of the Lapstone Monocline.

Galloway considered that thin ridge-cappings along Culoul Range were Wiannamatta sandstone and shale, or belonged to the Mittagong Formation (as now defined) between the Wiannamatta Group rocks and the Hawkesbury Sandstone. There is a shale capping at 630 m at Six Brothers Trig towards the western end of the Culoul Range (Fig. 1). At Haughey's Flagstaff (Fig. 4) on the Mellong Range 10 km east of Six Brothers Trig at an elevation of 390 m ASL a hard layer of rock at the bottom of a shale quarry was identified by J. G. Byrnes in company with the writer as a phosphatic mottled stone considered to be restricted to very basal Ashfield Shale in the Sydney area (Byrnes, GS 1983/420). If the outcrops at Six Brothers Trig and Haughey's Flagstaff are adopted as the base of the Ashfield Shale, vertical displacement between them amounts to 240 m, or more if the Culoul Range outcrop is a mudstone interval within the Hawkesbury Sandstone.

Mellong Syncline

The Mellong Syncline, a small plunging fold, has been plotted by Galloway (1967) sub-parallel to the Lapstone Monocline from Wallaby Swamp in the upper Mellong valley to south of Culoul Range (Fig. 1); it intersects the Mellong Range near Gibba Swamp, diverges east from the Mellong Plateau and extends south. Galloway (1965) considered the Mellong Syncline to be probably continuous with the Howes Valley Syncline further north; he saw an analogy between the Mellong Plateau and a smaller low-relief area of about one-fifth its size in the Burrowell Creek-Reedy Creek-Howes Valley area, and postulated a "general topographic low" including both synclines and resulting in swamps and alluviation.

The following dissimilarities in the settings of the two low-relief areas may be noted:

1. The southern end of the Mellong Plateau, where all characteristic features of the plateau plainly appear, is adjacent to the Lapstone Monocline and west of the Mellong Range watershed, but lies 4 km west of the Mellong Syncline.

2. The Burrowell Creek-Reedy Creek-Howes Valley low-relief area lies 8 km east of the Mellong Range watershed and 5 km east of the Lapstone Monocline. The Howes Valley Syncline traverses it in a NNE-SSW direction. The Hunter Range, a major regional watershed, lies 6 km to the east beyond the Macdonald River which is entrenched 100-200 m below the low-relief area. The core of the low-relief area has physiographic elements suggestive of former topographic domal relief with radial drainage to the flanking Burrowell and Reedy Creeks by their tributaries Spring, Back Swamp, Running and Monkey Ground Creeks and direct drainage to the Macdonald River.

The Mellong Plateau and the Howes Valley low-relief area are surrounded -- and separated -- by many kilometres of deeply dissected plateau. Apart from their common low-relief they reveal few topographic similarities.

The marked parallellism of the Mellong Syncline and Lapstone Monocline, and the Howes Valley Syncline and Lapstone Monocline, suggests the possibility of a

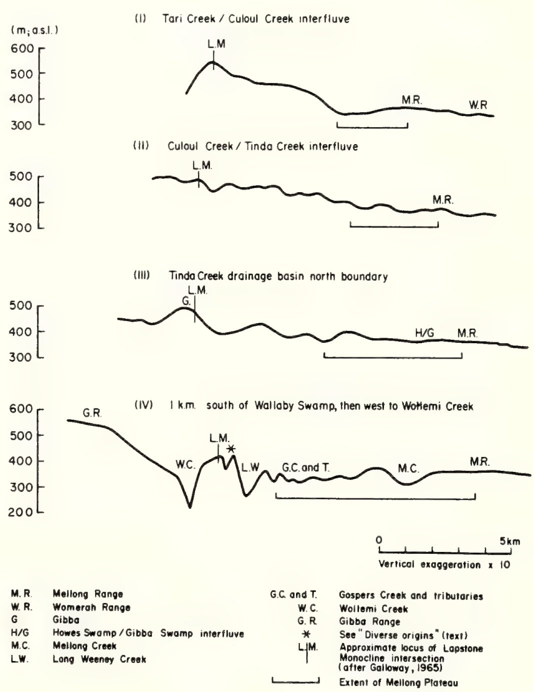

Figure 2 Interfluve Profiles in Mellong Plateau Area

TABLE 1

MAXIMUM ELEVATIONS OF NORTH-SOUTH ZONES IN MELLONG PLATEAU AREA

Zone	Width of zone (km)	No. of highs	Mean heights (m)
Immediately west of Macdonald River	3	56	289
Adjacent zone to west	3	57	313
Next adjacent zone to west	3	57	337
Zone immediately east of Mellong Range	3	56	346
Mean Mellong Range height at nodal points of ridges	-	18	367
Adjacent zone to west (east Mellong Plateau)	2	37	365
Lapstone Monocline zone	2	34	455

TABLE 2

MEAN ELEVATIONS ALONG NINE PAIRS OF RIDGES AT 200 m INTERVALS

	Mean Elevations (metres a.s.l.)
Immediately east of Mellong Range	351
Mean Mellong Range height at nodal points of ridges	367
Immediately west of Mellong Range	367

common, broadly synchronous origin. As plotted by Galloway the synclines have a rough end-to-end relationship (Fig. 1).

PRESENT-DAY GEOMORPHIC STATUS OF MELLONG RANGE

In conceptualising the Mellong Plateau it is necessary to consider whether the Mellong Range was a positive geomorphic feature before creeks flowing east to the Macdonald River reached their present westward terminations. Nowhere in the subject area is the Mellong Range a conspicuous topographic feature today (Fig. 2). By grouping maximum elevations in each square kilometre on the St. Albans topographic map grid in north-south strips of width as indicated in Table 1 its cryptic character is revealed.

The last-mentioned zone in Table 1 is bounded by north-south lines joining grid reference points 78.39 and 78.22, and 80.39 and 80.22 on the St. Albans 1:100,000 topographical map and represents the first 2 km-wide north-south zone within the area deemed elevated by the Lapstone Monocline.

If the mean heights of nine approximately corresponding ridges running east and west on either side of the Mellong Range are noted at 200 m intervals for a distance of 2 km on the 1:25,000 Six Brothers and Wirriba topographic maps (Central Mapping Authority of N.S.W., 1977) the figures in Table 2 are obtained. (See Appendix for important differences between place names used on the Six Brothers and Wirriba 1:25,000 and on the St. Albans 1:100,000 topographic maps respectively).

There is good correspondence between the two sets of data but it remains unclear whether the Mellong Range in the study area, notwithstanding its function as a drainage divide, is more than the present eastern boundary of the Mellong Plateau. Field observations suggest that it has minor range-like character from Tari Creek to Gibba Swamp with interfluves descending east and initial minor fall westward; in the Mellong Creek area interfluves to the west of the Mellong

Range are approximately level for 2 km and residual highs occur west of Mellong Creek valley. Further north near Staircase Hill, and extending for 10 km south-east of Mt. Kindarun, the Mellong Range is flanked to east and west by sub-parallel creeks ranging from 3 to 5 kms apart. It should be noted (Figs. 1, 3) that the only sectors of the Mellong Range showing abrupt increase in elevation are where that range and the Lapstone Monocline converge/coincide (Grassy Hill area) or intersect (north-east of Putty).

At the western edge of the Mellong Plateau there is a pronounced increase in the height of interfluves (Fig. 2). The locus of this marked topographic inflection approximately corresponds to the Lapstone Monocline as mapped by Galloway (1965), adopted by Bembrick *et al.* (1980), and represented on the St. Albans-SBFA Structural Synthesis map (Mauger *et al.*, 1984). It appears as a fairly strong lineament on Landsat imagery (op.cit.). From the summit of Mt.Yengo (668 m.), situated 25 km to the northeast, the Mellong Range cannot be distinguished in the maze of ridges east and west of the Macdonald River, but a low ramp-like structure, a short distance west of the Putty Road which is glimpsed at two or three points, rises in the direction of Mt. Coricudgy (1235 m) on the horizon--the eastern front of that structure is taken to be the Lapstone Monocline. In the Grassy Hill area where the Lapstone Monocline and Mellong Range converge (Fig. 1) the Putty Road is not visible from Mt. Yengo.

SUBDUED RELIEF, RESTRICTED VERTICAL EROSION AND VALLEY HEAD FEATURES

A low ridge of 35 m maximum local relief between Gibba and Howes Swamps separates the Mellong-Gospers and Tinda-Culoul-Tari Creek systems, which drain the Mellong Plateau. The mean relief of all interfluves on the plateau is less than 40 m, with minimum relief about 25-30 m.

Along the western fall most Mellong Plateau creek heads are semi-circular or fan-shaped in plan due to retardation of vertical erosion at a level slightly above 300 m -- the approximate base- level height of the Mellong Plateau surface. The elevations of creeks at the Putty Road are: Mellong 295m, Tinda 315m, Culoul 340m and Tari Creek 345m. With restricted lateral erosion and minor wasting of low slopes the result is a stagnant-appearing landscape, the general aspect of which reflects the comparative ineffectiveness of wasting and erosion on the Mellong Plateau under existing climatic conditions.

At the southern end of the plateau between the topographic nodes from which ridges extend east and west the Mellong Range is no more than a drainage divide: Tari, Culoul and Tinda Creeks descend from saddles between the nodes at slopes of less than 3°. North of Howes Swamp the wide headwater valleys of the 2 km-long east tributaries of Mellong Creek are closed by a ridge of 20-30 m relief (the Mellong Range) with slopes of 8° to 12°. Although the Mellong Range has not yet been breached in this area it is under

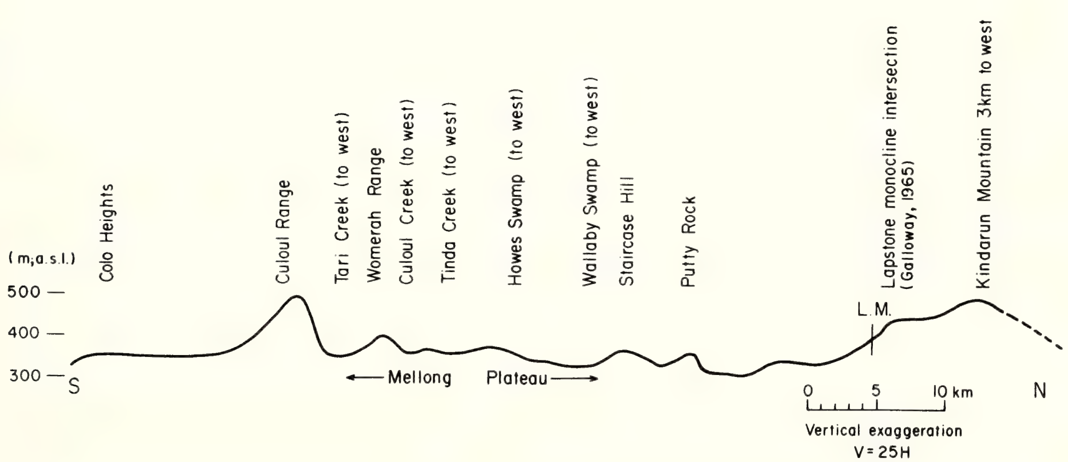

Figure 3 North-South Profile of Mellong Range

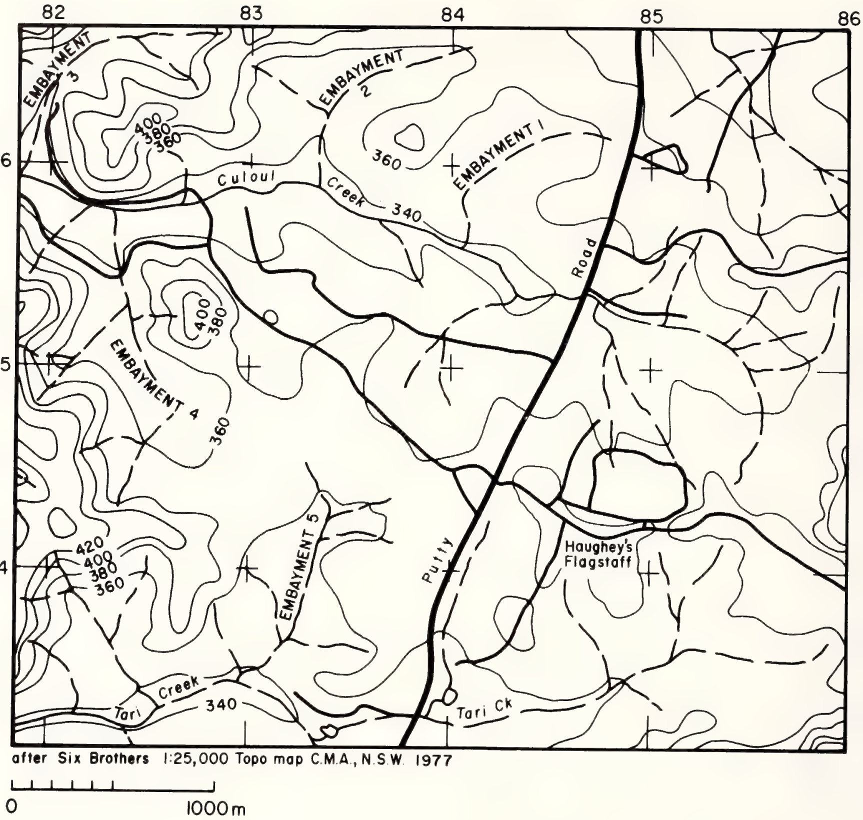

Figure 4 Culoul Creek-Tari Creek Area Showing Embayments. Drainage lineaments within Embayments 1 to 3 on Culoul Creek and 4 and 5 on Tari Creek, although not presently discernible in the field, are discernible in 1961 air photos and appear on the Six Brothers 1:25,000 topographic map (1977); see text.

attack in many places as evidenced by the intricate interfingering of steep gullies within the range. This contrasts with the western edge of the plateau where creeks with gradients of less than 18' drain the plateau through swamps and maintain their low gradients for up to 3 km into the gullies.

EMBAYMENTS

Wide and deep embayments along valley sides are characteristic of the Mellong Plateau and are found on all its creeks; typical examples occur on Culoul Creek (Fig. 4). 1 km west of the Putty Road on the north side of the creek where the valley is 800 m wide with a gradient of 17' there is an embayment 500 m wide at its entrance (Fig 4; Embayment 1). From its entrance 300 m north of the creek, the embayment slopes upward at 1°; 250 m from its entrance the floor flattens for 200 m. The embayment remains almost flat to within 50m of side and rear walls where it is closed by a sandstone ridge. There are no signs of a watercourse nor any visible drainage lines in the embayment, nor any between the embayment and Culoul Creek, but air photos taken in 1961 (St. Albans,N.S.W., 514, 5072-5073) show a small watercourse within the embayment extending to the creek.

There are two more embayments (Fig 4; Embayments 2 and 3) on the north side of Culoul Creek within 3 km of Putty Road: their overall dimensions are similar to those of Embayment 1 -- they are equally flat but narrower (300-400 m) and longer. They too presently lack surface drainage lines but small creeks appear on 1961 air photos. Together with Rising Fast Gully, a tributary of Tinda Creek, Embayments 1, 2 and 3 have reduced to near flatness 75% of the interfluve separating Culoul and Tinda Creeks.

On the south side of Culoul Creek an embayment (Fig 4; Embayment 4) less than 200 m wide at its entrance enlarges until it is almost 500 m square at the rear of a high (400 m ASL) hill. Embayments 4 and 5 (the latter on Tari Creek) have almost levelled 60% of the Culoul Creek-Tari Creek interfluve at this point.

SWAMPS

The extensive flat areas of the Mellong Plateau are occupied by swamps; eight swamps each exceed 1 sq.km in area (Fig. 1). On each main creek a large swamp has its eastern boundary within 1 km of Mellong Range extending, in some cases with minor interruptions, to the plateau's western margin (Howard, 1982). In addition most creeks have tributaries with swamps comparable in size to their own -- in some cases a single swamp occupies both main creek and tributary. Gibba Swamp lies at the head of a tributary of Tinda Creek; Rising Fast Gully with a swamp nearly 2 km long is another tributary of Tinda Creek. Tari Creek has two tributaries with swamps. Mellong Creek flows for 5 km through Mellong Swamp, its tributaries including Howes Swamp Creek, Wallaby Swamp, and swamps on Gospers Creek and other tributaries.

Most Mellong Plateau swamps dry out at the surface after less than three months of drought so the delineation of swamp boundaries depends on identification of swamp plant species, soils and topography (Howard, *op. cit.*). A.R.M. Young (1983) described the shallow upland valleys of the Woronora Plateau as "infilled by organic-rich material rather than peat". On the Mellong Plateau not only is there little accumulation of peat but, at depths less than 1 m, sediments in swampy areas typically exhibit little or no organic staining, sediment colours of 10YR 6/1-4 and 7.5YR 7/1-3 (Munsell code) being common. PH values range from 5 to 6, both inclusive. Well-developed podzols are widespread in silty and clayey sand on flats and low slopes but are not found on all sites.

Climatic data for east Woronora Plateau, Mellong Plateau and Richmond Meteorological Office, the nearest weather station to the Mellong Plateau, appear in Table 3. From the Richmond data it seems likely that mean evaporation on the Mellong Plateau exceeds mean precipitation in all months.

THE VALLEY FILL

Extent and Depth of Fill

The second edition of the 1:250,000 Sydney Geological Map (Rose, 1961) showed Quaternary alluvium on the Mellong Plateau to be restricted to middle and lower Mellong and Gospers Creeks with minor patches on Tinda and Culoul Creeks. The third edition (Bryan, 1967) extended Quaternary alluvium to all main creeks and major tributaries, multiplying the alluviated area 20 times, illustrating the difficulty of estimating accurately the lateral extent of alluviation on the plateau.

Determining the depth of fill on the Mellong Plateau presents at least equal difficulties. H. Martin (1979, 1981) commented on the difficulty of distinguishing between decomposed sandstone and indurated clayey sand while drilling. The difficulties are compounded when sediment is derived from the same rock as the bedrock on which it lies and recementation is well advanced. 154 samples were collected from 34 augered holes on Culoul,

TABLE 3

METEOROLOGICAL DATA FOR THE EAST WORONORA AND MELLONG PLATEAUX AND FOR RICHMOND, N.S.W.

Locality	Altitude (m)	Distance from coast (km)	Mean Annual Rainfall (mm)	Evaporation/ Precipitation Relationship
E. Woronora Plateau	300-500	7	1500 (1)	P exceeds E in all months
Mellong Plateau	300-400	80	850 (2)	Not known
Richmond M.O.	40	50	806 (3)	E exceeds P in all months

(1) A.R.M. Young (1986). (2) Howard's (1982) estimate.
(3) Bureau of Meteorology (1979); Richmond M.O. (pers. comm. 1986).

Mellong and Tari Creeks to depths of up to 10 m, and 5 backhoe trenches were excavated to 3.5 m, but the bedrock/fill boundary remained uncertain at many sites.

Seismic and resistivity surveys were carried out on a network of intersecting traverses on Culoul and Mellong Creeks (Riley and Henry, this issue). Seismic refraction showed three layers, the upper layer 0.5-1.5 m thick corresponding to a layer of poorly consolidated sand; this layer was equally well defined in resistivity surveys. The second layer, taken to correspond to the fill or weathering zone, showed a range of resistivities probably depending on degree of saturation as well as composition. The third layer was interpreted as less weathered or unweathered bedrock.

Refraction data suggested persistent depths of fill normal to the creek at sites where drilling data and thin section examination of undisturbed samples suggested such depths were unlikely -- simple horizontal beds of nearly unlimited extent are not commonly found in shallow valley fills. It was considered that probable errors overall were of the order of not more than 1 to 2 m in the deepest sections. Distinction between fill of alluvial as opposed to colluvial affinity was not possible from the data. Thin-sections resulting from a limited programme of coring provided fabric evidence in many cases that samples taken at depths to 9 m were unlikely to be in situ Hawkesbury Sandstone (Conaghan and Henry, in preparation). The more compelling evidence included the presence of disjunctive grain fabrics in quartz sand grains demonstrably unrelated to the grains' present textural context, e.g. detrital grains exhibiting grain-margin modification variously through (a) stylolitic (pressure-) solution in the absence of a mutual grain contact relationship, and (b) syntaxial quartz overgrowth with compromise boundaries wholly unrelated to neighbouring grains. The silt component of some samples was abnormally high, exceeding that normally found even in the relatively clay-rich massive facies of the Hawkesbury Sandstone. In spite of uncertainties thin-sections broadly confirmed data from seismic and resistivity work and were consistent with drilling results.

The overall evidence obtained supports the following conclusions regarding the depth of valley-fill on the Mellong Plateau:

1. The fill depth is commonly 6 m to more than 10 m near major creeks and swamps.

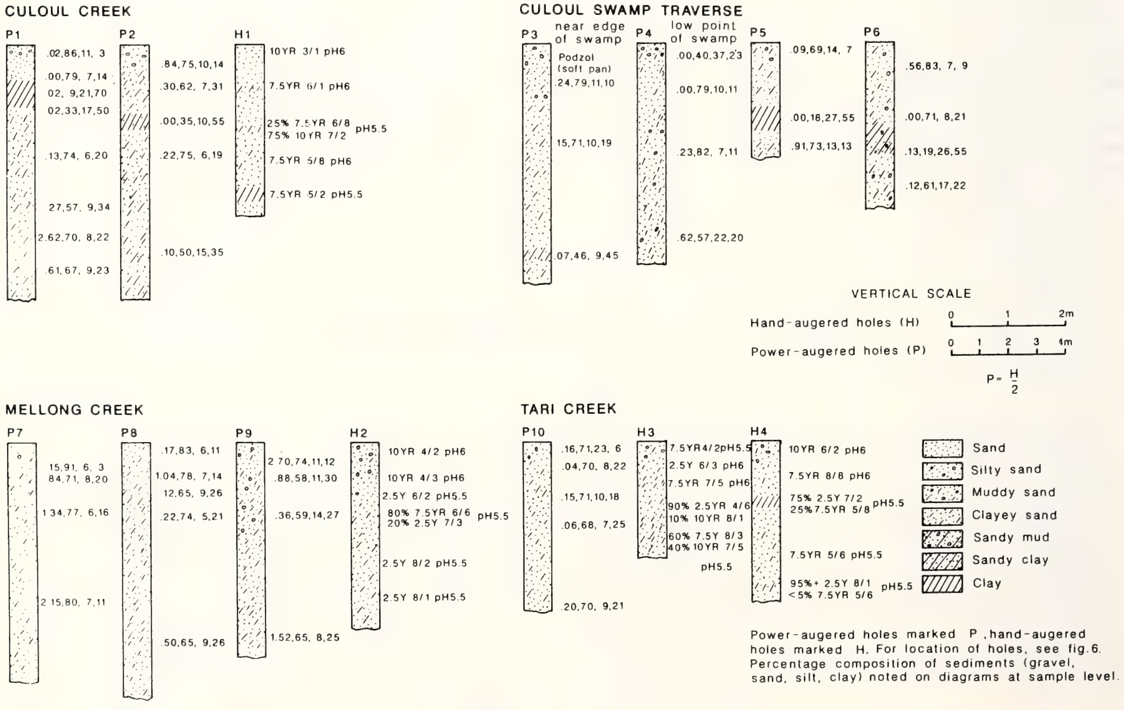

Figure 5 Selected Vertical Sections from Drilling on Mellong Plateau Creeks

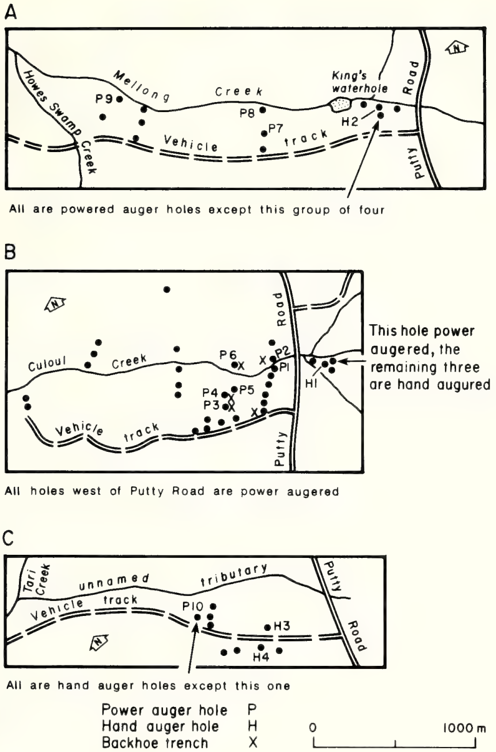

Figure 6 Drilling Sites on (A) Mellong Creek (B) Culoul Creek and (C) a tributary of Tari Creek. For granulometric profiles of numbered holes, e.g. H3, see Fig 5. For location of drilling areas, see Fig. 1.

2. Fill 6 m deep occurs extensively on flat areas no longer swampy, in some cases 300 m from the nearest creek: e.g. south of Mellong Creek below Howes Swamp Creek junction; in the Tari Creek south tributary area; and in some embayments.

3. Fill 3 m deep extends up 1.5 slopes for more than 200 m in some areas: e.g. at the Culoul Creek and Mellong Creek seismic survey sites where undisturbed samples were obtained to depths of 7-10 m.

The above conclusions are consistent with the findings of two Australian glass manufacturers which in 1968-70 independently investigated Mellong Creek as a source of industrial sand. One company drilled 74 holes down Mellong Creek for 5 km from the Putty Road and, according to drilling notes, reached "bedrock" only twice. The driller reported that the whiter sand was found mainly below the water beds in the area in narrow bands 100-200 yards wide and 12-30 feet deep, usually with white/yellow clay below. Beyond the whiter sands lay poor sands, gravels or clays, ranging in colours from yellows to bright reds. The company estimated that a five years supply of sand was available but considered iron content to be too high even after treatment to make colourless glass. The second company also considered that substantial sand deposits existed but in view of sources closer to Sydney decided not to mine Mellong Creek.

Petrography of the Fill

In addition to more than 200 samples collected by powered auger and from backhoe trenches, extensive hand augering was carried out. 71 samples were analysed granulometrically, using a Rotap sieving machine and hydrometer (Figs. 5, 6).

Gravel

Mean gravel content of 71 samples from Culoul (10 holes), Mellong (6 holes) and Tari Creeks (2 holes) was 0.52%. Gravel clasts and and grains were predominantly composed of sub-angular quartz with mean diameter less than 5 mm. Ironstone pebbles were present sporadically but rarely amounted to more than 20% of the gravel fraction.

Sand

Standard (1969) found that detrital quartz grains made up 68% of the whole Hawkesbury Sandstone outcrop -- 74% including secondary quartz cement. Sand mean size was 1.77ø. Standard's samples from Culoul Range were mean grain size 1.48ø (deviation 0.88ø). Mean grain size of 71 Mellong Plateau samples analysed during the present study was 1.52ø (deviation 0.76ø). Using Folk's (1968) classification both Standard's and the writer's Mellong Plateau samples are moderately sorted medium sand. This was also W. Gregory's finding from stratigraphically overlapping vertical sections measured through the Hawkesbury Sandstone immediately east and west of the Mellong Plateau near Tari Creek (Gregory, pers com. 1986).

A marked disparity between Standard's skewness data and Mellong Plateau samples was evident. Standard's Hawkesbury Sandstone samples were strongly fine-skewed with mean 0.39 and values ranging from -0.02 to 0.70; the writer's Mellong Plateau samples gave a mean value of 0.03, near symmetrical, with values ranging from -0.20 to 0.30. All 10 values below -0.05 came from Mellong Creek. Mean kurtosis of all Mellong Plateau samples was 1.17.

Quartz detrital grains, including overgrowths, made up 93% of the sand component of ten samples from Culoul Creek fill at depths to 9 m (Table 4). Standard found that quartz made up more than 90% of all detrital grains; his data are similar to those obtained from samples at Culoul Creek.

Clay

F.C. Loughnan examined 12 samples from Culoul Creek at depths from 1 to 5 m and commented on X-ray data as follows (written communication 1980): "There is a certain degree of sameness: apart from quartz all contain kaolinite, which is generally disordered, and a small amount of mixed-layer material. The mixed-layer material generally somewhat resembles clay vermiculite. Grim and I described the occurrence of a similar material in the ferruginous zone of weathered sequences developed on the Wiannamatta and Hawkesbury shales of the Sydney area (Geol. Soc. Aust. 8, 245-258, 1962). A similar material formed as the result of weathering of micas and illite has been recorded from other parts of the world also. It is apparently degraded micas that have lost K and adsorbed either Al3 or Fe3 or both in the interlayer position. They seem to be stable or relatively stable to weathering. A semblance of weathered succession similar to that obtained from the Sydney area is lacking. In my opinion this neither proves nor disproves the concept that the material has been weathered in situ, and it may only be due to the technique being used being ineffective in this case."

Heavy Minerals

The heavy mineral fraction of 13 samples taken from the vicinity of Culoul Creek at depths ranging from 0.3 m to 8 m showed rutile to exceed zircon by a proportion of more than two to one (Table 5).

The data are broadly consistent with results of Standard (1964; 1969) and Galloway (1972), particularly in relation to the predominance of rutile over zircon in the Hawkesbury Sandstone. No heavy minerals were found in Culoul Creek samples other than those identified by Standard and Galloway in Hawkesbury Sandstone.

Provenance of Fill

Mineralogical and granulometric data from all work are consistent with the plateau alluvial fill being entirely derived from Hawkesbury Sandstone with important contribution from the preferentially clay-rich massive facies. However, the possibility that a former Wiannamatta Shale cover contributed fines to the sediment retained on the plateau is not excluded by the data.

TABLE 4

PETROGRAPHIC COMPOSITION OF 10 ALLUVIAL FILL SAMPLES FROM CULOUL CREEK BASED ON THIN-SECTION MODAL ANALYSIS BY POINT-COUNTING

	%
Quartz Plutonic	79
Vein	12
Metamorphic	2
Sandstone fragments	2
Iron oxide and hydroxide	1.5
Micas	0.5
Other grains, including rock fragments other than sandstone, chert, heavy minerals, etc.	3

TABLE 5

AVERAGE PERCENTAGES OF HEAVY MINERALS BASED ON GRAIN-COUNTS OF 13 SAMPLES OF ALLUVIAL FILL FROM CULOUL CREEK

	%
Rutile	37
Zircon	17
Tourmaline	11
Opaques	29
Others	6

Textural Analysis of Sediments

Textural analysis of the 71 samples granulometrically assessed was carried out. Vertical sections of 14 of the 67 holes drilled are shown in Fig. 5. When grainsize composition of samples is related to sample depth it confirms assessments from drilling and seismic and resistivity data that the sediment fill is divided horizontally into two layers -- a sandy layer extending to a depth of from 60 cm to 1 m underlain by an immediately recognisable but gradational change to clayey sand. Data are summarised in Table 6.

Apparent disparities between the creek valleys may be misleading. One-third of Culoul Creek samples were taken in a traverse across Culoul Swamp and that data may be skewed in favour of the silt-clay component. However, all samples collected during the project were collected in swampy areas as delineated by Howard (1982) or at adjacent sites.

Mean grainsize of all samples was 3.96ø (standard deviation 3.43ø). In Folk's terms the mean sample would be described as very poorly sorted, strongly fine-skewed, very leptokertic, slightly gravelly, muddy medium sand. Using Folk's categories the samples would be classified as follows (frequencies):

TABLE 6

TEXTURAL COMPOSITION OF MELLONG PLATEAU SAMPLES OF ALLUVIAL FILL FROM POWER-DRILL AND HAND-AUGER HOLES (FIGS. 5 AND 6)

(Mean Percentages)

SAMPLE DEPTH	GRAVEL	SAND	SILT	CLAY
Surface to 1.0 m (25 Samples)				
Culoul Creek	0.40	70	12	18
Tari Creek	0.07	76	12	12
Mellong Creek	0.94	79	9	11
1.0 m to 2.5 m (27 Samples)				
Culoul Creek	0.21	47	14	39
Tari Creek	0.08	65	9	26
Mellong Creek	0.82	71	7	21
2.5 m to 9.0 m (19 Samples)				
Culoul Creek	0.65	63	12	24
Tari Creek	0.13	69	8	23
Mellong Creek	1.39	70	8	21
Surface to 9.0 m (71 Samples)				
Culoul Creek (41 Samples)	0.43	60	13	27
Tari Creek (7)	0.07	71	10	19

1. From surface to 1 m deep: sand 1, silty sand 4, muddy sand 12, clayey sand 8.

2. 1 m to 9 m deep: muddy sand 11, clayey sand 27, sandy mud 3, sandy clay 4, clay 1.

In view of the low gravel content of samples, and in order to distinguish between silt and clay fractions, Folk's scheme for gravel-free samples was employed.

DISCUSSION

Origin of flat, incised and widened valleys

Six hypotheses relating to the characteristic form of Mellong Plateau valleys are briefly reviewed.

Erosional bench hypothesis

Standard (1964) suggested that "the flat area north of Grassy Hill on the Putty Road appears to be caused by the erosion of a shale layer." The evidence does not seem to support this conjecture, unless the shale layer that Standard had in mind has now been stripped from the plateau everywhere except at Haughey's Flagstaff (Fig. 4). A traverse across Culoul Swamp undertaken to look for a clay or shale basement found a clay layer in only 5 out of 11 holes. Muddy sand only was found in a hole drilled to 7 m at the lowest point in the swamp (P5, Figs. 5 and 6). In a hole 50 m away and nearer Culoul Creek a clay bed occurred at 2 m (P6, Figs. 5 and 6).

Seven holes up to 10 m deep were drilled on Mellong Creek; a hole was drilled to 7 m in the middle of a dry swamp on Tari Creek south tributary; and another at the outlet to Six Brothers Waterhole -- in none did a clay bed appear. A hole was drilled to 9.4 m in the middle of Embayment 1 (Fig. 4) but no clay bed was found. A search along the base of embayment sides revealed no clay or shale beds of significant thickness.

While it is possible that clay beds partly underlie all Mellong Plateau swamps, the evidence suggests that the swamps do not depend on clay or shale basements for their existence. Indeed the relatively short time required by Mellong swamps to dry out out at the surface may be due to their absence. The muddy and clayey sands which predominate in the swampy areas investigated appear to be effective short-term aquicludes only.

Friable Bedrock Hypothesis

If friable sandstone as quarried near Clarence on the western Blue Mountains Plateau, and on Somersby Plateau near Gosford, ever underlay the Mellong Plateau's flat surfaces it might help to account for the wide valleys and embayments. S. Pecover of the N.S.W. Department of Mineral Resources, presently engaged in a survey of the State's industrial sand resources, associates friable sand deposits with (inter alia) increased localised wetting of the sandstone due to a "palaeo-drainage pattern on the Newnes Plateau considerably older than drainage regimes that have formed many of the deeply incised valleys throughout the western Blue Mountains" (Pecover, 1984). In drilling on the Mellong Plateau it was often found that weathered sandstone overlay more resistant bedrock. However, measured vertical sections from 240m-520m through the Hawkesbury Sandstone in the Tari Creek area (Gregory, pers. comm., 1986) did not encounter extremely friable sandstone, although much medium to soft sandstone occurs. From presently available evidence the Mellong Plateau does not appear to be an area where the Hawkesbury Sandstone is exceptionally friable.

The N.S.W. Department of Mineral Resources has not yet investigated the Mellong Plateau as a source of friable sandstone.

In situ weathered bedrock hypothesis

Joseph Carne, a former Deputy Principal Geological Surveyor of New South Wales, referred to the study area as follows (1908): " In the Putty district on the plateau level ["interfluves"] degradation is even now in progress. Large areas of loose sand studded with isolated turrets of stratified sandstone upwards of 50 feet in height mark the amount of decay in situ that the plateau has undergone where, owing to the absence of appreciable fall, erosive and transportive activities have not had scope." Carne's recognition that low gradients frustrating vertical erosion and sedimentary discharge have contributed very significantly to the plateau's geomorphic features was perspicacious but he did not attempt to explain the features distinguishing it from similar areas.

Reversed drainage hypothesis

The anonymous author of a chapter on the Blue Mountains and adjacent wilderness areas in a book recently published by the Australian Conservation Foundation (Anonymous, 1981) tentatively accounts for the flatness of the Mellong Plateau (more specifically for the "Mellong Swamps") by reference to a "series of creeks that originally drained east to the Macdonald River in shallow valleys." The hypothesis purports to explain the 'mature' valleys of the Mellong Plateau. However, any suggestion that drainage reversal is responsible for the plateau's geomorphic features encounters the following difficulties:

1. Dendritic drainage, particularly of Tinda, Tari and Mellong Creek valleys, is well developed both in gully tracts and on the plateau. Tari Creek is a tributary of Culoul Creek, and Culoul Creek of Tinda Creek; it seems impossible to relate tributary valleys to creeks that once drained eastward.

2. Bishop (1982) presented evidence of the considerable age of Sydney Basin river systems concluding that ".. the gross parallellism between Triassic, Late Cainozoic and modern drainage directions, and the lack of geological evidence for changed drainage directions from the Triassic to the Late Cainozoic, suggests that modern drainage systems [in the Sydney Basin] are of considerable antiquity." Although evidence from valley-fill basalts and datable sub-basaltic sediments is lacking there is no reason to suppose the Colo River-Wollemi Creek drainage system to be younger than the other major valleys of the Southern and Central Tablelands discussed by Young (1977, 1978a and b, 1981) and Bishop (1982). Wollemi Creek flows southeast from Mt Coricudgy for a distance exceeding the length of the Colo River above the Wollemi Creek junction; for more than 40 km above that junction its mean gradient is 8', approximately the same as the Colo River in the area. The gradient of Putty Creek, a tributary of Wollemi Creek, is 7' for 12 km above Wollemi Creek. Long Weeney Creek's gradient is 17' for 8 km above Wollemi Creek. Mellong Creek has a gradient of 13' for 18 km on the Mellong Plateau, steepening to 28' through the gorges (and the Lapstone Monocline) to Wollemi Creek.

In Sydney Basin terms the Colo River- Wollemi Creek complex appears to be an old drainage system.

No evidence exists to support the suggestion that Wollemi Creek or any predecessor has ever flowed east across the Mellong Plateau since present drainage patterns were laid down.

"Seepage hollow" hypothesis

R.W. Young (1977b; 1978) considered that shallow swampy valleys incised (Young's word) into the more friable beds at the top of the Hawkesbury Sandstone were "...not streamcut features but rather seepage hollows formed primarily by weathering of the highly permeable sandstone...[and] by water seeping downhill on a broad front.." Prima facie some valley heads on the Mellong Plateau appear to be of stream-cut origin: valley floors 300 m wide lie 30 -40 m below interfluves within half a kilometre of the Mellong Range. Valley form is particularly strongly marked in the 2 km-long tributaries of upper Mellong Creek previously referred to: if such valleys were headed by recission cols they might be regarded as evidence of significant former extension of their drainage basins to the east -- however such valleys rise at the foot of a ridge 30 - 50 m high (the Mellong Range). They are analogous in many respects to the embayments with flat floors inset in medium slopes, "nestling against the valley floor" in Schmitthenner's phrase (1925) describing valley-bottom dells

Valley widening by embayments hypothesis

Embayments demonstrate that the Mellong Plateau's most distinctive feature, the wide, positively incised, flat-floored valleys, can be explained without invoking the through-flow of creeks originating either east or west of the plateau. Both the form and orientation of many embayments makes it inconceivable that the creeks on which they abut have played any direct part in their formation or alluviation. While retreat of interfluves independent of embayment formation may be occurring, the scale of embayments relative to valley width makes it probable that embayment development has been the primary mode of valley widening.

In spite of their apparent stagnancy (sensu Crickmay, 1974) Mellong Plateau embayments are not passive landforms. They have been formed by the action of running water channelled or otherwise, which has also been responsible for distributing alluvium and colluvium over the embayments' flat floors and transporting part of the sediment into main valleys. Schmitthenner (*op. cit.*), frequently ambivalent about the role of running water in denudation, acknowledged that "run-off water is absolutely necessary for the formation, deepening and conservation of dells"-- which he considered to be the major means of plateau lowering. Major development of embayments probably occurred in periods wetter than the present. The presence of Quaternary pollen at the bottom of a sandy silty clay bed 3.5m deep on Culoul Creek almost in front of Embayment 1 (Fig 4) (A. McMinn, pers. com. 1983; H. Martin, written communication 1983) may be evidence of increased erosional activity in a high rainfall period of the Quaternary. On available evidence, however, it is impossible to decide whether heavy alluviation has been due to significant climatic change or a sequence of catastrophic events (Burkham, 1972; Henry, 1977).

Transverse Drainage

Common Explanations of Transverse Drainage

1.Superimposition

Four basalt residuals occur in the district bounded by the Colo River, Putty Valley, Wollemi Creek and the Macdonald River. None occurs on the Mellong Plateau. On the south side of Culoul Creek a hill 400 m above sea level designated "TV" on the Sydney Geological Map (3rd edition, 1965) revealed no sign of volcanics on field examination. Residual basalt lying on the Culoul Range immediately west of Six Brothers Trig at Jonathan's Nob at an altitude slightly above 600 m above sea level has been described as a flow and dated at 20.8 ma (Embleton *et al,* 1985). An exposure of basalt in the floor of an unnamed tributary of Tari Creek west of the Mellong Plateau at an altitude of 320 m (grid reference 783232) is undated and may be a plug. Two basalt residuals east of the Mellong Range and within 5 km south-east of Putty appear on the Sydney Geological Map (1965). Without more evidence the residuals referred to provide an inadequate foundation for a hypothesis that a former basalt cover explains the transverse drainage of the six valleys (Angorowa, Tari, Culoul, Tinda, Mellong and Long Weeney) which breach the Lapstone Monocline between the Colo River and Long Weeney Creek.

Young (1977, 1978 a and b, and 1983) and Bishop (1982) who used dated basalt fills to estimate the age of valleys in the south and south-west Sydney Basin appear to have found no evidence of leading drainage lines having been superimposed from basalt cover.

P.J. Conaghan has suggested (pers.

comm., 1986) that the intersection of the Lapstone Monocline and Mellong Range some kilometres north of Putty (Figs. 1 and 3)-- an anomaly of uncertain significance in view of ignorance as to the relative age of the features -- might be explained by the former existence of a basalt cover.

In spite of much speculation, no evidence has been presented that a significant area of the Sydney Basin was blanketed to a relevant depth by volcanic or other cover when present drainage patterns were laid down. Superimposition is considered to be an unlikely explanation of the Mellong Plateau's transverse drainage.

2. Headward Erosion

It is difficult to place limits to the capacity of streams to extend their channels by headward erosion. How else can creeks east of the Mellong Range flowing to the Macdonald River have extended westward until they presently threaten range and plateau in nearly forty places along the eastern edge of the plateau? However, it is by no means certain that similar headward erosion by Wollemi Creek tributaries explains the transverse drainage of the Mellong Plateau. Relevant observations include the following:

(i) There can have been no transverse drainage of the Mellong Plateau by headward erosion until the Wollemi Creek drainage system or its prototype was established.

(ii)(a) Westerly verging dendritic drainage characterises all four main creeks both on the Mellong Plateau as well as in the entrenched monoclinal ramp area immediately to the west. It seems impossible to reconcile the tributary valleys with creeks that once drained eastward, as would be likely to have occurred if drainage to the west had been closed.

(b) If in fact the Mellong Plateau once drained eastward and the east-flowing drainage was captured by headward erosion, the headward- eroding creeks have emerged from the gullies with pinpoint accuracy: there are no significant bends, boathook or otherwise, and end-to-end capture has occurred in every case.

The implications of 2(a) and 2(b) are that the Mellong Range, or some prior structure corresponding to it, is an ancient drainage divide, and the antiquity of the Mellong Plateau is similarly implied. Unfortunately there appear to be no empiric criteria by which a particular stream may be deemed unlikely to have been formed by headward erosion. Holmes (1965, p.595) has observed that it is seldom easy in practice to dispose of the possibility that transverse valleys have resulted from headward erosion.

3. Antecedent drainage

Sparks (1960) declared that antecedence should be the last resort of a geomorphologist attempting to explain transverse drainage because, except in ideal cases, it was undemonstrable. However, while difficulties arise in seeking to explain Mellong Plateau transverse drainage by superimposition or headward erosion, explanation by antecedent drainage is prima facie certainly not excluded. Obviously the possibility that the transverse drainage is antecedent is linked with the age of the Lapstone Monocline.

A simple model for antecedent drainage of the Mellong Plateau might include the following elements: (i) the proto-Macdonald River and proto-Wollemi Creek as ancient drainage lines (ii) the Mellong Range or its prototype as drainage divide (iii) monoclinal uplift resulting in (a) elevation at the western end of interfluves east of the monoclinal inflexion, (b) minor upstream tilting, and (c) as a consequence impeded drainage of the Mellong Plateau.

The small drainage basins and insignificant discharge of the creeks pose problems for the antecedent drainage hypothesis.

R.W Young (1978b) considered that anomalous reaches of the Nepean River in the Penrith area and the passage of the Hawkesbury River through the uplifted Hornsby Plateau below Windsor were examples of antecedent drainage; he also thought it likely that Nattai and Wingecarribee Rivers were antecedent streams. He pointed out that they lie within a narrow zone extending from the Bowral district to the lower Hawkesbury and believed them to be due to movement along the Lapstone Monocline and associated Kurrajong Fault. All six transverse valleys between Colo River and Long Weeney Creek fall within a northward extension of Young's zone of discordant structure and drainage.

4. Diverse origins

Oberlander's (1965) study of the drainage patterns of the Zagros Mountains region in Iran concluded that transverse valleys are likely to consist of separate segments of different origin. In the Mel-

long Plateau area such genetic diversity seems to be implied by the following circumstances and relationships:

(i) The Mellong Creek -- Gospers Creek area is the most extensive flat area of the Mellong Plateau. Mellong Creek flows for a distance of 18 km on a mean gradient of 13'. Intermittent tributaries occupy wide flat-floored valleys between interfluves so eroded that connecting ridges are not immediately discernible on aerial photography and Landsat imagery. Embayments abound. Mellong Creek joins Wollemi Creek through a gorge 7 km long, entering land uplifted by the Lapstone Monocline immediately below the upstream gorge entrance. Mellong Creek's mean gradient is 28' in the gorge, having previously steepened its bed upstream to 20' south of Gospers Creek. It seems unlikely that a fourth order stream (cf. Strahler, 1960) and its associated drainage network which bear so conspicuously the hallmarks of age should have depended on headward erosion to provide discharge to a higher order stream. Indeed the facts seem consistent with Mellong Creek being an antecedent stream. The following sub-paragraph, however, suggests an alternative possibility.

(ii) A meridional ridge-top valley separating and sub-parallel to Long Weeney and Putty Creeks lies immediately west of Mellong and Gospers Creeks (Fig. 1). The latter creeks appear to avoid the ridge by turning south. The Lapstone Monocline runs obliquely across the ridge-top valley feature whose trace the structural synthesis map of Mauger *et al.* (1984) classifies as a summarised first-order fracture trace trend. The divided ridge is under attack on both flanks by headward erosion of tributaries of both Long Weeney and Putty Creeks. There is nothing to distinguish the headward-eroding gullies from scores of similar gullies appearing in aerial photographs of the region. The latter gullies are commonly considered to be the product of post-rift rejuvenation of local streams (cf. Browne, 1969; Ollier, 1982). Do not these physiographic features suggest that the Lapstone Monocline is post-Mesozoic in age, or at least imply its major reactivation during the Tertiary? They may also suggest that Mellong Creek drainage was not originally directed south and that a previously west-tending drainage system was captured by headward erosion from Wollemi Creek.

(iii) Angorowa Creek, the principal west-flowing stream south of Culoul Range, runs southwest from Mellong Range, then in annular fashion around the Angorowa Creek Ellipse (Structural Synthesis Map, Mauger *et al,* 1984) to the Colo River. The ellipse is described as a "circular drainage pattern" by Mauger *et al.* In common with the Grassy Hill Ellipse immediately to the north it may belong to an older generation of landforms. Angorowa Creek valley appears to be at least in part antecedent.

CONCLUSION

Fairly straightforward structural, stratigraphic and geomorphic relations probably exist between the Mellong Plateau and the regional geology, but the relations are unclear. The plateau is an enigma, unduplicated on the same scale elsewhere in the Sydney Basin. It seems impossible to dissociate it from the Lapstone Monocline; the fact that the elevated land through which Mellong Plateau creeks flow lies within a Lapstone Monocline-bordering, northward extension of Young's zone of discordant structure and drainage provides some support for a hypothesis that transverse valleys west of the plateau are antecedent.

There is no indication of the age of geomorphic features in the surface geology. Valley-fill basalts, a primary source of information elsewhere in the Sydney Basin (Young, 1978b; Bishop, 1982) do not occur; sporadic plugs and flows north-west of Putty and along the Great Dividing Range throw no light on the age of Mellong Plateau landforms.

No datable samples were obtained during the present study below a depth of 1 m, other than the bed of brownish black clay at 3.5 m on Culoul Creek previously referred to. In personal discussions R. Morgan (former N.S.W. Government Palynologist), H. Martin and A. McMinn considered that the sediments underlying the surface zone were too oxidised for palynological assessment. The techniques used to recover cores for thin-sectioning were not suited to obtaining materials for thermoluminescent or palaeomagnetic testing: further work on the Mellong Plateau may identify remnants of a significantly older fill.

No reliable estimate of the age of Mellong Plateau landforms is possible, and no explanation of the transverse drainage of the plateau will be generally accept-

able, until three intractable problems have been resolved:

1. The question whether the Sydney Basin was blanketed to significant depths by a now vanished cover of speculative lithological nature and age(s) when present drainage patterns were being laid down requires a positive answer (Branagan, 1983).

2. Elucidation of the unclear relationship between the Lapstone Monocline and the regional divide (i.e. the Mellong Range) is essential.

3. The earliest age when the Lapstone Monocline became a significant geomorphic factor, as distinct from the minimum age of its present surface expression (Bishop *et al*, 1982), is crucial to determining the Mellong Plateau's history: explanation of the drainage pattern calls for different models according to the age assigned to the Lapstone Monocline. Current estimates of that structure's age (and, by extension, the age of other meridional folds involving Triassic rocks to the east) range from middle or late Tertiary (Young , 1978 b, 1983; Bishop *et al*, 1982) to late Triassic (J.G. Jones, pers. comm., 1987). Jones considers that the Lapstone Monocline and associated faults are of the same age as other major folds and faults affecting the Sydney Basin now shown to be Permo- Triassic in age (Bembrick and Lonergan, 1976).

Clearly interpretation of the Mellong Plateau's subtle features may long await definitive resolution.

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APPENDIX

DISCREPANCIES BETWEEN PLACE NAMES USED ON ST. ALBANS 1:100,000 AND SIX BROTHERS AND WIRRIBA 1:25,000 TOPOGRAPHIC MAPS

There are important discrepancies between place names used on the Six Brothers and Wirriba 1:25,000 topographic maps (Central Mapping Authority of N.S.W., 1977) and those used on the St. Albans 1:100,000 topographic map (Division of National Mapping, 1973) for the same features. Names on the Central Mapping Authority maps, which are approved names under the Geographical Names Act (N.S.W.) 1966, have been employed here. The most important discrepancy arises from an error in geographical interpretation. On the St. Albans 1:100,000 topographic map Gospers Creek ('Molong Creek') appears as the trunk stream of the Mellong Creek - Gospers Creek drainage system; Mellong Creek ('Howes Waterhole Creek') is shown as a tributary of Gospers Creek. If Strahler's (1960) stream orders analysis is applied to Mellong and Gospers Creeks respectively the following appears:

CREEKS	STREAM ORDERS 2 (frequencies)	3	4
Mellong	42	10	1
Gospers	8	3	0

On the Wirriba topographic map Mellong Creek and its tributary Howes Swamp Creek are shown as permanent streams while Gospers Creek is intermittent. See also relevant air photos, e.g. St. Albans, N.S.W., 514, 5110-5112 (1961).

It is apparent that Mellong Creek is the trunk stream.

H.M.Henry
11 5th Ave.
Cremorne 2090
Australia

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Volatile Leaf Oils of the Two Subspecies of *Melaleuca acacioides* F. Muell.

J. J. BROPHY, E. V. LASSAK AND D. J. BOLAND

ABSTRACT An examination of the volatile leaf oils of two recently described subspecies of *Melaleuca acacioides*, ssp. *acacioides* and ssp. *alsophila*, has shown substantial chemical differences between them. Subspecies *acacioides* from northern Queensland is almost exclusively sesquiterpenic with α-and β-selinenes accounting for about 80% of the oil, whilst subspecies *alsophila* from northern Western Australia is almost entirely monoterpenoid in character with p-cymene, geranial and terpinen-4-ol, each approximately 20%, as its main oil components.

INTRODUCTION

Melaleuca acacioides F. Muell. has a wide natural distribution across the northern part of Australia (see Fig. 1). The habit of the species ranges from a bushy shrub, 3-7m tall, to a small tree up to 9m in height. The species grows on a range of habitats from the edge of coastal salt marshes through to better drained sites on small rises flanking inland plains.

Barlow (1986) recently revised the taxonomy of *M.acacioides* and divided the species into three new taxa. These are *M.acacioides* ssp. *acacioides*, *M.acacioides* ssp. *alsophila* Barlow and *M.citrolens* Barlow. The last named species is morphologically different from *M. acacioides* though it occupies a similar range of habitats as *M.acacioides* ssp.*acacioides*. One notable distinguishing feature is the presence of a distinct citrus-like odour in the crushed leaves of *M.citrolens*, which is lacking in the foliage of ssp. *acacioides*.

We report here the results of a chemical examination of the volatile oils of both subspecies of *M.acacioides*, viz. ssp. *acacioides* and ssp. *alsophila*.

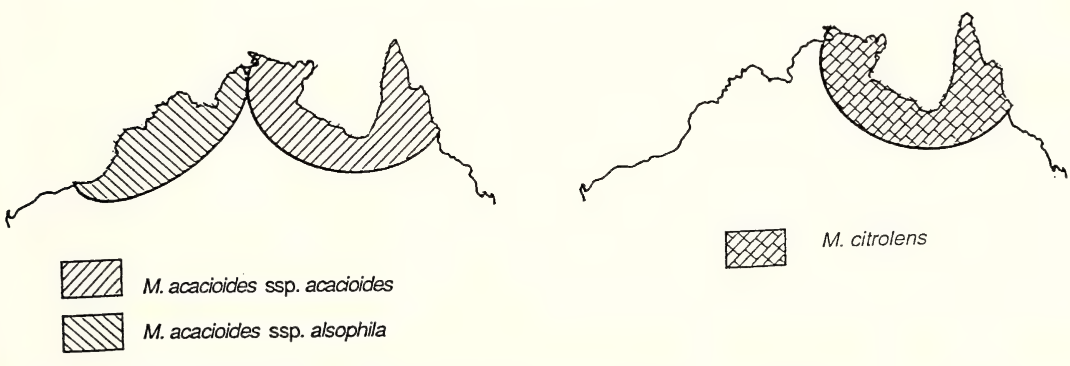

Fig. 1 Distribution of *Melaleuca acacioides* ssp. *acacioides*, *M.acacioides* ssp.*alsophila* and *M.cirolens* across Australia

TABLE 1
Compounds detected ins *Melaleuca acacioides* ssp. *acacioides*
from south east of Weipa

COMPOUND	%	COMPOUND	%
1. ethylbenzene	tr	25. $C_{15}H_{24}O$	.01
2. limonene	tr	26. $C_{15}H_{26}O$	.15
3. α-copaene	tr	27. globulol	.93
4. α-gurjunene	.01	28. viridiflorol	.10
5. unknown	.01	29. $C_{15}H_{26}O$	tr
6. unknown	.01	30. $C_{15}H_{26}O$	tr
7. caryophyllene	1.24	31. $C_{15}H_{26}O$	.60
8. β-gurjunene	.07	32. $C_{15}H_{26}O$	.04
9. unknwon	tr	33. $C_{15}H_{26}O$	1.74
10. $C_{15}H_{24}$	tr	34. $C_{15}H_{26}O$	.01
11. $C_{15}H_{24}$	1.67	35. $C_{15}H_{26}O$	.37
12. β-selinene	23.00	36. $C_{15}H_{26}O$	.20
13. α-selinene	54.10	37. $C_{15}H_{26}O$	.32
14. selina-3,7-diene	.96	38. $C_{15}H_{26}O$	.19
15. δ-cadinene	5.18	39. selin-11-en-4-ol	6.50
16. unknown	tr	40. $C_{15}H_{24}O$	tr
17. β-bisabolene	tr	41. unknown	tr
18. $C_{15}H_{24}$	.07	42. $C_{15}H_{24}O$	.01
19. $C_{15}H_{26}O$	.06	43. $C_{15}H_{24}O$	.75
20. $C_{15}H_{24}O$	.60	44. $C_{15}H_{24}O$	.02
21. $C_{15}H_{26}O$	tr	45. nerolidol	.07
22. $C_{15}H_{26}O$	.05	46. $C_{15}H_{24}O$	.46
23. methyleugenol	.07	47. $C_{15}H_{24}O$	.10
24. $C_{15}H_{24}O$	.10		

compounds are listed in order of elution from a SP1000 column.

RESULTS AND DISCUSSION

Tables 1 and 2 list the constituents of the leaf oils of single trees of *M.acacioides* ssp.*acacioides* from south-east of Weipa in northern Queensland and of *M.acacioides* ssp.*alsophila* from south-east of Derby in northern Western Australia respectively.

It is interesting to note that despite their botanical closeness, the two subspecies contain entirely different leaf oils. Except for limonene, present in significant amounts in ssp.*alsophila* and as a trace constituent in ssp. *acacioides* is almost entirely sesquiterpenoid whereas the lemon-scented oil of ssp. *alsophila* is, by contrast, virtually entirely monoterpenoid.

The composition of the oil of ssp. *alsophila* is unexceptional except perhaps for the presence of traces of 1,3,8-menthatriene, 1,4,8-menthatriene, anethole, jasmone and rose exide not previously reported from leaf oils of *Melaleuca*. Quantitative variation among individual constituents was quite marked comparing oils from single trees; the following being ranges observed for some of the major constituents: p-cymene, 21-42%; terpinen-4-ol, 15-27%; neral + geranial, 18-27%. It has also been observed that the tree with the highest terpinen-4-ol, neral/geranial content contained the least amount of p-cymene. 1,8-Cineole has not been detected in any of the oils analysed.

TABLE 2

Compounds detected in *Melaleuca acacioides* ssp. *alsophila* from south east of Derby

COMPOUND	%	COMPOUND	%
1. α-pinene	3.13	26. $C_{10}H_{18}O$	.07
2. camphene	tr	27. acetophenone	.08
3. β-pinene	.19	28. neral	8.00
4. myrcene	1.12	29. α-terpineol	2.80
5. α-phellandrene	.70	30. geranial	19.00
6. α-terpinene	2.04	31. $C_{10}H_{18}O$	tr
7. limonene	1.70	32. $C_{10}H_{18}O$	.20
8. β-phellandrene	.98	33. unknown	tr
9. $C_{10}H_{14}$	.25	34. citronellol	.10
10. γ-terpinene	2.50	35. anethole	tr
11. β-*trans*-ocimene	2.50	36. unknown	tr
12. ρ-cymene	21.20	37. unknown	tr
13. terpinolene	.70	38. nerol	.28
14. rose oxide	.03	39. unknown	tr
15. unknown	tr	40. ρ-cymen-8-ol	.05
16. 1,3,8-menthatriene	.05	41. geraniol	.86
17. $C_{10}H_{14}$	tr	42. a benzoate ester	tr
18. 1,4,8-menthatriene	tr	43. jasmone	.07
19. α,ρ-dimethylstyrene	.87	44. $C_{15}H_{24}O$	.05
20. unsaturated ester	tr	45. unknown	tr
21. linalool	.06	46. nerolidol	tr
22. $C_{10}H_{18}O$	.08	47. unknown	tr
23. $C_{10}H_{18}O$	.04	48. unknown	tr
24. terpinen-4-ol	27.20	49. $C_{15}H_{26}O$	tr
25. unknown	tr		

compounds are listed in order of elution from a SP1000 column

By far the most abundant compounds in ssp. *acacioides* were α- and β-selinenes, with the former being approx. 2-2.5 times more abundant than the latter. δ-Cadinene and selin-11-en-4-ol were also present in substantial amounts. Whilst β-selinene has been reported in the leaf oils of *Melaleuca armillaris* and *M.trichostachya* (Brophy and Lassak, 1983), and δ-cadinene in the oil of *M.alternifolia* (Swords and Hunter, 1978) α-selinene and selin-11-en-4-ol have not been previously reported from the genus *Melaleuca*. Selin-11-en-4-ol, the major alcohol in ssp. *acacioides* was identified by its mass spectrum (Tressl et al., 1983) and by dehydration with phosphorus oxychloride and pyridine to a mixture of β- and γ-selinenes.

Whilst the chemical differences between the two subspecies are substantial they may have only limited taxonomic significance. It is well known that *Melaleuca quinquenervia* (Cav.) S.T. Blake (syn. *M.viridiflora* Gaertn.) exists in two chemical forms characterised by different mono- and sesquiterpenoids (Hellyer and McKern, 1956). The presence of distinct citrus odours in both *M. acacioides* ssp. *alsophila* and *M. citrolens* may be significant. However, a study of the taxonomic relationship between

these two species in terms of their volatile oil compositions will require further investigation.

The high terpinen-4-ol and citral (neral/geranial) content of ssp. *alsophila* oil suggests that it may constitute a more fragrant alternative to the commercial *Melaleuca alternifolia* oil (Medicinal Tea Tree Oil) the bactericidal properties of which are well known (Lassak and McCarthy, 1983). In view of the rather poor yield (approx. 0.2%) and the variation in quantity of the principal components, a sampling of the native populations and a selective breeding programme would be necessary prior to cultivation on any significant scale.

EXPERIMENTAL

Collection of plant material and isolation of volatile oils

Melaleuca acacioides ssp. *acacioides* - Air dried leaves and terminal branchlets from one year old trees (two single trees and one bulk sample from twenty trees) grown from a known seed lot, (CSIRO, Division of Forest Research # S14146, SDS 330-334) obtained from south east Weipa in northern Queensland, and grown at Gympie, Queensland were steam distilled with cohobation as previously described (Lassak, 1979) for 8 hours to yield colourless oils in 0.4-0.8% yields. A separate sample of adult foliage, collected 7km north-west of Hann River crossing on the Laura to Coen road in Cape York Peninsula yielded 0.27% of volatile oil (on fresh foliage), n^{20} 1.5051, α^{20} + 7.6°.

Melaleuca acacioides ssp. *alsophila*. Air dried leaves from adult trees growing 50km south-east of Derby, West Australia (CSIRO, Division of Forest Research, CEP.K1) and air freighted to Sydney were distilled as above to yield colourless lemon scented oils in 0.03-0.22% yield.

Identification of components

Analytical gas chromatography (glc) was carried out on a Shimadzu GC6 AMP gas chromatograph. A SCOT column of SP 1000 [85m x 0.5mm] which was programmed from 65°C to 225°C at 3°C/min was used with helium carrier gas. For combined gc/ms the gas chromatograph was connected to an AEI MS12 mass spectrometer through an all glass straight split interface. The mass spectrometer was operated at 70 eV ionising voltage and 8000V accelerating voltage with the ion source at 200°C. Glc conditions for combined gc/ms were the same as for the analytical glc. Spectra were acquired every six seconds and processed by a VG Display Digispec data system. Glc integrations were performed on a Milton Roy CI-10 electronic integrator.

Compounds were identified by their identical glc retention time to known compounds and by comparison of their mass spectra with either known compounds or published spectra (Stenhagen et al., 1974; Heller and Milne, 1978, 1980, 1983; Tressl et al, 1983).

Dehydration of the selin-11-en-4-ol fraction of *M.acacioides* ssp.*acacioides* leaf oil.

The sesquiterpene alcohol fraction of the oil (obtained by silica gel chromatography and elution with 20% ether in pentane)(20mg) was treated with pyridine (0.5ml) and phosphorus oxychloride (100mg) and allowed to stand at room temperature for one hour. The reaction mixture was then added to water (10ml) and extracted with pentane (2 x 5ml). The pentane solution was washed with saturated sodium bicarbonate (2 x 5ml) and water (2 x 5ml) and dried over sodium sulphate. After evaporation of the pentane, the residue was chromatographed on silica gel. Elution with pentane gave the sesquiterpene hydrocarbon mixture which was analysed as above by gc/ms.

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J.J. Brophy
Department of Organic Chemistry
University of N.S.W.
P O Box 1
Kensington, NSW 2033

E.V. Lassak
School of Technology
Kalgoorlie College
P.M.B. 22
Kalgoorlie, WA 6430

D.J. Boland
CSIRO, Division of Forest Research
P O Box 4008
Queen Victoria Terrace, ACT 2600

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Analysis of a Small-Scale Fault at Bingi Bingi, N.S.W., and Speculations on its Relationship to a Large-Scale Transform Fault of the Tasman Sea

M. B. KATZ

ABSTRACT. South of Moruya, at Bingi Bingi Point, N.S.W., a NW trending aplite dyke of the Devonian Tuross Head Complex has been sinistrally drag folded by a ENE steeply dipping fault. A structural analysis of the faulted, drag folded dyke provides a preliminary solution that speculates by scale transfer, that the fault may be part of a Riedel shear system related to movements along a transform fault developed during the opening of the Tasman Sea which subsequently controlled later mafic dykes emplaced during the rifting of Australia from Antarctica.

INTRODUCTION

The development of the Tasman Sea and the separation of Australia from Antarctica in the Cainozoic was accompanied by widespread volcanism in Eastern Australia (Wellman & McDougall, 1974). Cainozoic mafic dykes mapped at Bingi Bingi Point, south of Moruya, N.S.W. (Brown, 1928; Halford, 1970) (Figs. 1 & 2) are probably related to the nearby basalt flows of the Moruya Province dated by Wellman & McDougall (1974) at about 30 m.y. According to Wellman & McDougall, the volcanism was, possibly, structurally controlled by tensional forces related to the continental rifting affecting SE Australia, during this period. A fault, which drag folds a Devonian aplite dyke, together with parallel mafic dykes are thought to be related to this Cainozoic structural event, and the fault is analyzed using stereographic methods described by Ragan (1985) and Wheeler (1987).

The scale transfer from outcrop scale, or smaller, to regional scale is an important concept first emphasized by Tchalenko (1970) in his study of Riedel shear zones. Similar shear zone geometries on all scales from microscopic (1 mm) to regional (10 km), over 10^7 orders of magnitude, were described by Tchalenko (1970), and these similarities suggested similar deformation mechanisms. This scale transfer structural study could be applied to coastal outcrop scale faults which could be similar in age and geometry to the larger scale transform faults. Fracturing occurs in rocks on all scales and the great transform faults of the crust actually consist of a wide zone of Riedel fault domains on various scales (Tchalenko, 1970, Courtillot et al, 1974). If the fault at Bingi Bingi is a Riedel fault related to these larger transform faults of the Tasman Sea, a structural analysis of the small scale fault may contribute to the understanding of the larger scale structure.

FAULT ANALYSIS

Geometry

The Tuross Head Tonalite outcrops at Bingi Bingi where it intrudes bodies of gabbro and gabbroic diorite, and in turn is cut by NW trending aplite dykes (Griffin, et al. 1978). The tonalite - gabbro - aplite complex is of Devonian age and is intruded by ENE trending mafic dykes of Cainozoic age (Halford, 1970). This ENE direction is also the preferred orientation of fractures, faults and shears which penetrate the outcrops. On the basis of the similar geometries and orientations of the faults and mafic dykes, they are assumed to be of similar origin. The feature of special interest is a steeply dipping ENE fault, about 4m wide, which sinistrally drag folds a thin (0.3m), aplite dyke (320, 45°NE) into an S-sygmoidal shape and displaces the dyke with an apparent strike slip separation of 11m (Fig. 3). The fault can be traced to the ENE where it cuts across and also drags and offsets a thicker (1·0m), subparallel, aplite dyke (Fig. 2). An examination of these relationships in the outcrop reveals that the fault has a component of strike-slip displacement (see Ragan, 1985, p. 103-104). Measurements of the poles of the narrower aplite dyke, deformed surface, Sd, plot as a great circle distribution on a stereographic projection (Wheeler, 1987) (Fig. 4). This suggests a simple cylindrical-type folding of Sd about a rotation axis B (50°,050), in the shear fault. At Bingi Bingi the exact dip of the steep fault is not easily determined, because of the nature of the exposure, however as B must lie in the fault plane and the observed strike trace of the fault is 065, the attitude of the fault must be 065, 78°NW (Fig. 4). Although the direction of the slip,or displacement vector A is not uniquely determined, it is assumed to be the line normal to B in the fault plane A B (Figs. 4 & 5). A can also be derived by the intersection of the undeformed aplite dyke (320, 45°NE) and the fault (065, 78°NW) which gives a line B which must be perpendicular to the shear direction A in the fault (Ramsay & Huber, 1983).

In plan view the shear fault appears to be a sinistral strike slip fault, however the rotation axis B (50°,050) is not vertical and the assumed slip vector or direction of net slip A (37°,225) is not horizontal as would be expected for an ideal horizontal strike slip fault (e.g. McDonald, 1980), so these attributes define an oblique slip fault. According to the criteria of Wheeler (1987) the true movement sense can be determined by observing the direction from A to the pole of the fault, Pf, which results in a sinistral-normal fault (Fig. 4). The apparent strike slip separation of 11m is made up of a net slip of 8.7m, with a strike slip component of 6.8m and a 5.4m dip slip component (Fig. 5).

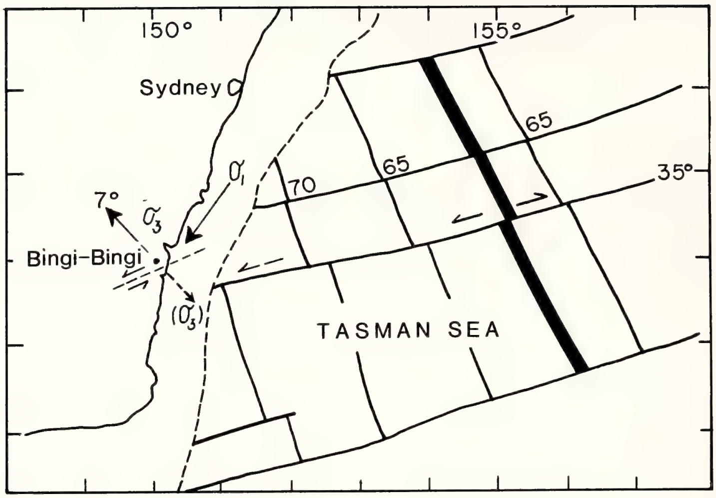

Fig. 1. Location of the Bingi Bingi fault showing transform directions in the Tasman Sea (Ringis, 1975) and the solutions for the secondary palaeostress field ($\sigma_1\sigma_3$) speculated to be operating during fault activation.

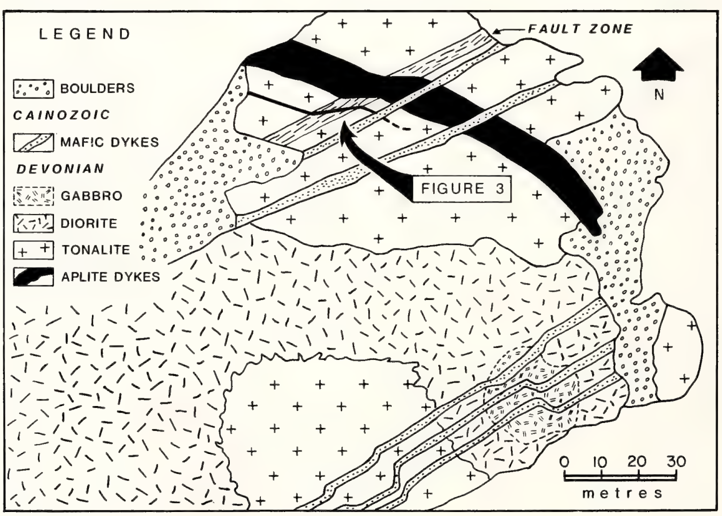

Fig. 2. Geological Map of Bingi Bingi Point (after Brown, 1928; Halford, 1970). Fault drag folded aplite dyke shown in detail in Fig. 3. Bends in the mafic dykes on the south side may be due to later local fault offsets.

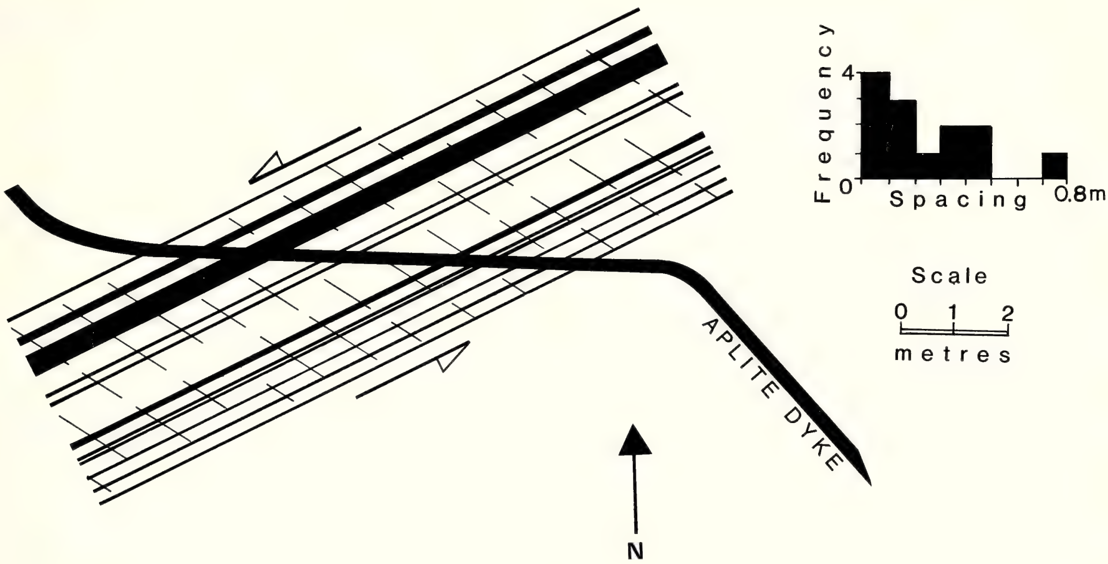

Fig. 3. Fault drag folded competent aplite dyke. Fault zone is made up of parallel, irregularly spaced shear surfaces. The black shear zones are more ductile. Oblique, penetrative, regular spaced fractures (NW-SE dashes) are interpreted as XY? (see Fig. 7). Histogram shows a tendency for a higher frequency of closely spaced shear surfaces.

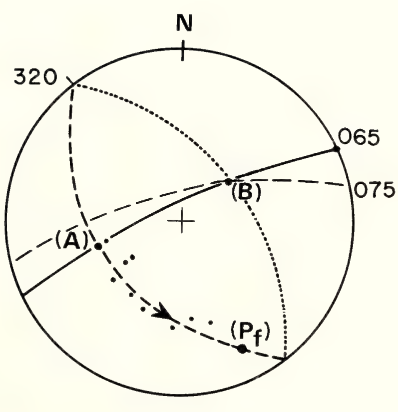

Fig. 4. Drag folded aplite dyke S_d polar plot (small dots) fits a great circle distribution (dashed) whose pole B is the rotation axis. Since B lies in the shear fault plane and the strike trace of the fault is 065, the cyclographic projection of the fault plane can be constructed (solid). The slip vector A is assumed to lie normal to B in the fault plane A B. A can also be determined as the line normal in the fault plane, from the line of intersection B, of the fault plane A B and the undeformed aplite dyke (320, 45 NE) (short dashes). B is also the pole to the plane containing the pole to the fault Pf and the slip vector A. The sinistral-normal movement on the fault can be determined by observing the direction of progressive bending (arrow) of the poles to the aplite dyke along great circle A P_f. Transform fault in the offshore Tasman Sea trends 075 azimuth. If B is common to both the shear fault and the transform fault the attitude of the transform fault can be constructed (broken). Lower hemisphere Wulff net.

Kinematics Speculations

If the fault is a Riedel (R) type shear fault (Tchalenko, 1970) it should be developed as a low angle (10°) splay shear off the main (transform) fault direction and should display the same sense of displacement. Conjugate Riedel (R') shears are developed at higher angles (80°) to the main fault and display the opposite sense of displacement. P shears are oriented approximately symmetrical to the Riedel shears (Fig. 6). These Riedel shears are observed at different stages and both the R and R' shears are typical of the peak shear strength stage of deformation. Post peak shear strength structures are typically P shears and finally, in the residual stage, the shears rotate into parallelism with the principal shear direction and all displacements take place along this single fault.

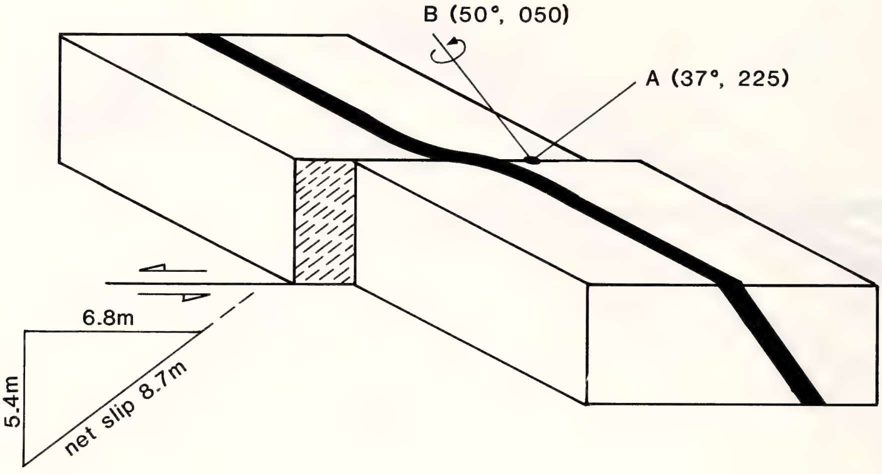

Fig. 5. Block diagram showing the relationship between the axis of rotation B and the slip vector A, in a steeply dipping sinistral, oblique slip fault (modified after McDonald, 1980). The net slip and strike slip, dip slip components are indicated.

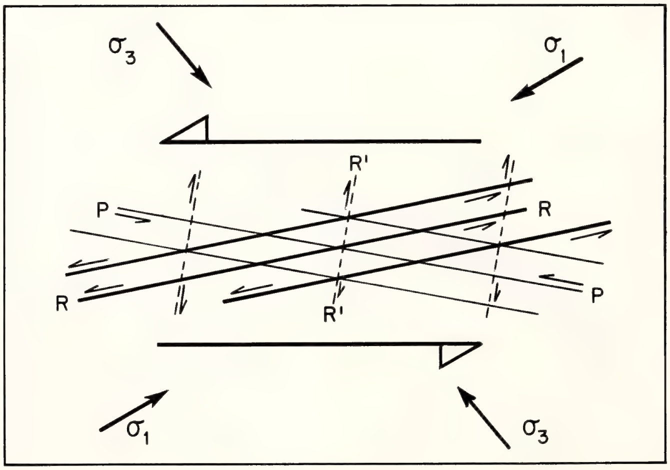

Fig. 6. The geometry of Riedel RR, conjugate Riedel R'R' and P shears PP developed from a sinistral shear couple. Resultant secondary stresses σ_1 and σ_3 .

The transform fault in the Tasman Sea offshore from the area has a trend of about 075 azimuth (Ringis, 1975). If the fault at Bingi Bingi is an R shear related to this transform fault it should be inclined at about 10° to the transform fault and it should have the same sense of transform movement. The attitude of the fault has been calculated as 065 azimuth, dipping 78° northwest and this and its sinistral nature is compatible with the transform faulting which lies west of the Tasman Sea spreading ridge (Ringis, 1975) (Fig. 1). If the transform fault-(Riedel) shear are part of the same system, the transform fault should have a common rotation axis B, and given its trend of 075 azimuth its attitude must be 075, dipping 70° northwest (Fig. 4).

Dynamic Speculations

Secondary stresses developed by the principal transform shear could have controlled the development of secondary structures that would accommodate the transform movements. These stresses can be estimated by stereographic techniques described

by Ragan (1985) and Ramsay and Huber (1983) (Fig. 7).

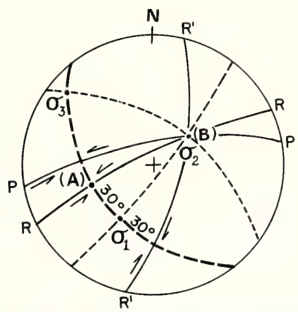

Fig. 7. Stereographic construction for solutions of σ_1,σ_2, σ_3 where B = σ_2 and is the pole to the $\sigma_1\sigma_3$ plane. σ_1 is located by measuring off 30° from A, in $\sigma_1\sigma_3$, in a direction compatible with the sinistral nature of fault A B, and σ_3 is located normal to σ_1 in the $\sigma_1\sigma_3$ plane. Planes $\sigma_1\sigma_3$ and $\sigma_2\sigma_3$ can also be constructed. The plane of flattening ($\sigma_2\sigma_3$ = XY?) is consistent with the trend of the fractures developed in the fault zone (see Fig. 3). If A B is a Riedel shear RR, the conjugate Riedel R'R' can be constructed by measuring off a 60° dihedral angle and the P shear by measuring off a 160° dihedral angle (see Fig. 6). Lower hemisphere Wulff net.

In the simple ideal case for the theoretical development of conjugate fractures in mechanically isotropic rocks during 2 dimensional strain (Ragan, 1985), the rotation axis B is parallel to σ_2 of the stress ellipsoid which is the pole to the $\sigma_1\sigma_3$ plane. The angle between A and σ_1 measured in the $\sigma_1\sigma_3$ plane is ideally about 30°, and σ_1 can be located by measuring off 30° along $\sigma_1\sigma_3$ in a direction that is compatible with the sinistral movement of the fault. Thus σ_1 is located on the $\sigma_1\sigma_3$ with an orientation of 40°, 216. σ_3 measured off 90° from σ_1, in the $\sigma_1\sigma_3$ plane, has an orientation of 7°, 313. The principal planes of stress ($\sigma_1\sigma_2$ = 042, 83°SE; $\sigma_2\sigma_3$ = 306,50°NE) can also be constructed (Fig. 7). If the fault A B is a Riedel shear RR it should, have a 60° dihedral angle relationship between it and its conjugate Riedel R'R' (Tchalenko, 1970) and this acute angle should be bisected by σ_1. The P shear PP should be symmetrically related to the Riedel shear RR (Figs. 6 & 7). Accordingly, R'R' and PP are estimated to have attitudes of 015 azimuth, dipping 65°E and 080° azimuth dipping 70°N respectively (Fig. 7). Regularly spaced, steeply dipping fractures in the fault that have a strike of 300° (Fig. 3), approach the value for the $\sigma_2\sigma_3$ plane, 306, 50°NE (parallel to XY?), (Fig. 7).

SPECULATIVE PLATE TECTONIC CONTROLS

The fault analysis has defined a steep, ENE trending sinistral-normal, oblique slip fault which is sub-parallel, to the nearby Cainozoic mafic dykes. The dynamic analysis indicates that the minimum stress, or direction of extension, σ_3=7°, 313 or roughly NW-SE, could also have developed the steep ENE trending mafic dykes. The mafic dykes appear to cut across the aplite dykes without displacing them (Fig. 2) and are possibly later manifestations of similar stresses that controlled the Bingi Bingi fault. Assuming that this dynamic analysis is correct and the faulting is of Cainozoic age and related to the plate tectonic history of the area, these orientations of σ_1 and σ_3 may indicate the secondary palaeostress field, during this period (Fig. 1). This period was one of active rifting with the development of the Tasman Sea (Jongsma & Mutter, 1978) and the separation of Australia from Antarctica. The Riedel fault activity and the later emplacement of the mafic dykes are probably related to these continental margin, plate tectonic events under the influence of a northwest-southeast horizontal tension.

Some simple geometric exercises can test the relationships between this Riedel shear fault and transform directions in the Tasman Sea (Ringis, 1975) (Fig. 1). The ratio of strike slip to dip slip on the fault can be determined on the basis of the orientation of the Riedel fault, with respect to the transform directions (Sumner & Thompson, 1974). Faults parallel to transform directions should show only strike slip displacements, while faults perpendicular to the transform directions should show only dip slip displacements. The Bingi Bingi, Riedel fault has an attitude 065, 78°NW, while transform directions in the Tasman Sea have a strike trace of about 075 azimuth. Therefore displacement on the sinistral-normal Bingi Bingi fault should be a combination of strike slip and dip slip. The fault shows a net slip of 8.7m, with a 6.8m strike slip (s) and a 5.4m dip slip (Fig. 5). As the fault has a 78° dip the vertical offset (d), or throw, is 5.3m. According to Sumner and Thompson (1974), the ratio d/s is proportional to the tan of the angle between the fault and the transform direction (Φ) (as measured about 10°), and the tan of the dip of the fault plane (Θ = 78°).

$$\tan \Phi \tan \Theta = d/s.$$

Solving for Φ gives about 9°30', which is in close agreement with the measured value of 10°.

CONCLUSIONS

The Bingi Bingi fault is a sinistral-normal oblique slip fault with a net slip of 8.7m. A stress analysis results in an estimate of extension of roughly NW-SE. This small scale fault is speculated to be related to larger scale, transform faults in the Tasman Sea initiated as Riedel shears at about 60 m.y. The subsequent rifting off of Antarctica from Australia used these earlier Riedel directions as preferential lines of mafic dyke emplacement estimated to be at about 30 m.y.

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Department of Applied Geology,
University of New South Wales,
P.O. Box 1, Kensington, N.S.W. 2033

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Doctoral Thesis Abstract (The Australian National University): A Stochastic Analysis of Scoring Systems

G. H. POLLARD

Many scoring systems can be seen as statistical tests of hypotheses. In tennis singles, for example, the scoring system used can be seen as a test involving two binomial probabilities p_a and p_b where p_a (p_b) is the probability player A (player B) wins a point initiated by player A (player B). Tennis singles is thus a "bipoints" game. The tennis scoring system is an inefficient test relative to the sequential probability ratio test (SPRT) based on pairs of these points. When $p_a + p_b > 1$ (the tennis context), an SPRT based on the "play-the-loser" (PL) rule is super-efficient. Chapter 2 shows that when $p_a + p_b > 1$ there is in fact a spectrum of super-efficient tests (with even durations) based on "partial-PL"(PPL) rules. The most efficient tests within this spectrum, when $p_a + p_b > 1$, are the SPRT based on the (full) PL rule. Chapter 3 extends this spectrum of tests to produce the total spectrum of tests (including those with odd durations).

Points within the tennis scoring system have different "importances" whereas points within any member of the above (efficient) spectrum of PPL systems are equally "important" when $p_a = p_b$. Intuitively, the differing importances of the points within the tennis scoring system contribute to the inefficiency of that system. Chapter 4 establishes a relationship between the efficiency of a bipoints scoring system and the importances of the points within it; a relationship which is used in Chapter 5 to show that the SPRT based on the PL rule has an optimal efficiency property when $p_a + p_b > 1$. Thus chapter 5 solves the classical hypothesis testing problem of finding the most powerful test for determining the greater of two binomial probabilities.

Chapter 6 shows that some complex SPRT systems can be decomposed into smaller independent components called "modules" which can in turn be analysed to produce values from which the asymptotic efficiency of the complete SPRT system can be evaluated. This module approach is then used in Chapter 7 to address the question as to whether the super-efficiency of the PL rule carries over to the case of tennis doubles.

The particular scoring system used in tennis is analysed in detail in Chapter 8 and the methodolgy used is seen to be useful for analysing any "nested" scoring system (e.g. tennis is 3–nested: points-games-sets). A new tennis scoring system is also proposed.

Chapter 9 contains a brief discussion of some of the characteristics the designer of a scoring system needs to consider. The study of the importances of points is extended in Chapter 10 and in Chapter 11 team play with associated countback rules is investigated.

In Chapter 12 it is shown that the classical scoring system used in multiple choice examinations can be considerably improved by modifying that scoring system and instructing the examinees to cross any boxes known to be incorrect when the correct box for that question is unknown.

School of Information Sciences
and Engineering,
Canberra College of Advanced Education,
P.O. Box 1, Belconnen, ACT, Australia, 2616.

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M.Sc. Thesis Abstract (The University of Sydney): Ultra-Violet and Tactile Pollination Guides of Some Fabaceae

D. S. Gibbons

The pollination ecology of eighty species, from ten tribes of the Family Fabaceae are investigated in relation to their coevolutionary development with some native and the feral member of the Super Family Apoidea. The literature relating to descriptive analysis of spectral polymorphism is reviewed and mention is made of the history of Australian Pollination Ecology till 1979. Apparently integrated anatomical and morphological adaptions of flower and vector are described. In particular the spectral polymorphism, ultrastructure, mictrotopography and cytology of the flower is examined and the relationship between these and vector vision and tactile sensitive organs investigated. Three generalised patterns of spectral polymorphism of the standard occur in the species studied. It is suggested that these correspond to three pollination strategies based upon different vectors. Microtopography of the petal surface also shows some distinctive differences and it is suggested that insects orientate themselves on the flower using the microscopic parallel ridge systems. The tactile sensors on the insect appendages are of a size consistent with this hypothesis. Vector pressure and ultimate flower pollination is investigated with the imbalance of pressure applied by the introduced feral bee *Apis mellifera* suggesting a negative effect on the native flora and fauna in the Mangrove Creek study area. Statistical analysis of petal surface striae, vector sensitive hairs and hair tips and pollen size and shape is presented and conclusions and predictions relating to flower/vector pollination ecology presented.

School of Biological Sciences,
(Botany, Room 105),
University of Sydney, NSW, Australia, 2006.

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Annual Report of Council for the Year Ended 31st March, 1987

MEETINGS

Nine general monthly meetings and the annual general meeting were held during the year. The average attendance was 21 (range 11 to 36). Abstracts of the addresses were published in the Newsletter. The meetings were all held at the Bowlers' Club of N.S.W., 95 York Street, Sydney.

The Liversidge Lecture for 1986 was delivered by Professor B.G. Hyde of the Research School of Chemistry, Australian National University, on Wednesday, 24th September, 1986, in the School of Chemistry, University of Sydney. The title of the Lecture was: "Inorganic and Mineral Structures Reconsidered: An Alternative View".

The Inaugural Poggendorff Lecture was held on Thursday, 6th November, 1986, at Hawkesbury Agricultural College. Dr. D.J. McDonald, Regional Director of Agriculture at Orange, spoke on "Walter Poggendorff - Pioneer Plant Breeder". Council extends its thanks to Dr. F.G. Swain, Principal of the College, and Dr. O.G. Carter, Deputy Principal, for their hospitality and assistance in arranging the Lecture.

On Saturday, 2nd August, 1986, a seminar entitled "Problems and Prospects of Preserving the Portable Scientific and Technological Heritage" was organised by Dr. R.S. Bhathal in association with the National Trust of Australia. The seminar, which was well attended, was held at the National Trust, Observatory Hill, and discussions are continuing with the N.S.W. Department of Environment and Planning, with a view to having some of the recommendations of the Seminar implemented.

Eleven meetings of Council were held at the Society's office, 134 Herring Road, North Ryde. The average attendance was 10.

ANNUAL DINNER

After investigating potential venues and costs Council regretfully decided that in the present financial climate it would not be wise to hold the Dinner and incur the risk of a large loss to the Society.

PUBLICATIONS

The Journal and Proceedings, Volume 119 Parts 1 and 2 were published in December, incorporating eight papers delivered at the 75th Birthday Seminar in honour of John A. Dulhunty on 4th April, 1986, and four other papers. Part 1 of the Report of Council for 1985 was also included. Council again thanks the voluntary referees who assessed papers for publication. The assistance of Miss H. Basden in processing the printing is gratefully acknowledged.

Ten issues of the Newsletter were published. Council is most grateful to the authors of short articles, which are much appreciated by members.

MEMBERSHIP

The membership of the Society at 31st March, 1987, was:

Honorary Members	14
Company	1
Life	31
Ordinary	274
Absentee	13
Associate	28
Total	361

During the year the deaths were announced with regret of the following members:

Raymond James Wood Le Fevre (26.8.1986); Maurice James Puttock (4.8.1986); William Humphrey Robertson (25.8.1986); and Harry Albert Theodore Scholer (6.12.1986).

AWARDS

The following awards were made for 1986:

Walter Burfitt Prize: Professor Brian Norman Figgis
Clarke Medal: Associate Professor David Ian Groves
Edgeworth David Medal: Dr. Leslie David Field and Dr. Peter Gavin Hall
The Society Medal: Professor Sydney Charles Haydon

SUMMER SCHOOL

A most successful Summer School on "Law" was held from 19th to 23rd January, 1987, at Macquarie University. It was attended by 55 students from about 42 schools. The Summer School was organised on the Society's behalf by Mrs. M. Krysko. The Society's appreciation is extended to Mrs. Krysko, to Mr. J.A. Welch who helped her in the organisation, particularly the barbecue, and to Council members who assisted. Council also wishes to thank the speakers, and the Courts for their assistance with the two half-day visits. Special thanks are extended to Professor J.L. Goldring, Head of the School of Law, Macquarie University, who helped with the program, and to Mrs. D.A. Kok, Deputy Chancellor of the University of Sydney who opened the School, and Miss Justice M.J. Lawrie of the Family Court of Australia who closed the School.
(see Summer School group photograph below)

LIBRARY

Acquisitions by gift and exchange continued as heretofore, the overseas and some Australian material being lodged in the Royal Society Collection, Dixson Library, University of New England, and other Australian material being lodged in the Society's office at North Ryde. Mrs. Grace Proctor has continued to supervise the North Ryde collection, to liaise with Mr. K. Schmude, University Librarian, and other Dixson librarians as occasion required. The Council is grateful to Mrs. Proctor for her continuing voluntary assistance and to Mr. Schmude for his care and concern in ensuring the smooth operation of the Royal Society Collection.

NEW ENGLAND BRANCH

The New England Branch held four very well-attended meetings during 1986. They were:

26 June:	Dr. Ian McIntosh, Department of Physics, University of New England: "Electronics in Agriculture".
11 September:	Professor B.A. Hills, Department of Physiology, University of New England: "Some applications of industrial physics and engineering principles to biomedical theory".
27 October:	Professor J.H. Loxton: "The Love of Numbers" - Presidential Address.
21 November:	Surgeon Vice Admiral Sir James Watt, KBE: "Cook and his Contemporaries: differences in medical emphasis".

DONOVAN ASTRONOMICAL TRUST

The Trust has been preparing a videotape on the Southern Skies for distribution to secondary schools and amateur astronomy groups. The Trust has met every month or so at the Society's Office. The President, as a Trustee, the late Mr. Robertson and Professor Loxton attended meetings.

FINANCE

The Society's financial year extends from January to December. In 1986 a deficit from operations of $955 was incurred. General income increased by $1800 and expenses by about $1100, compared with 1985. As in 1985, the largest increase in costs arose from the Journal ($4600 increase) and is tending to consume all the additional investment income that can be achieved, leaving little scope for expanding other services to members.

The Library Fund benefited by generous donations from seventeen members, totalling $630.

The grossed result of the Trust Funds balances shows an increase of $1100 compared to 1985.

The Council is aware that it is essential to avoid erosion of the Society's capital resources by incurring repeated deficits. Economies are exercised wherever possible, and substantial voluntary assistance in many aspects of the Society's operation has become absolutely essential. Unfortunately, the prospects for 1987 suggest that there will be no scope for relaxing the financial constraints.

Mr. A.M. Puttock, FCA, the Society's accountant and auditor, again gave valuable financial advice and assistance, which it is a pleasure to acknowledge.

ABSTRACT OF PROCEEDINGS

The Annual General Meeting and nine General Monthly Meetings were held at the Bowlers' Club of N.S.W. Abstracts of the proceedings of these meetings are given below.

In addition the Liversidge Research Lecture was delivered on 24th September, 1986, by Professor B.G. Hyde at the University of Sydney. The title of the Lecture was "Inorganic and Mineral Structures Reconsidered: An Alternative View". The Inaugural Poggendorff Lecture was given on 6th November, 1986 by Dr. D.J. McDonald, at the Hawkesbury Agricultural College. Its title was "Walter Poggendorff - Pioneer Plant Breeder". On 2 August, 1986, a seminar entitled "Problems and Prospects of Preserving the Portable Scientific and Technological Heritage" was held in association with the National Trust of Australia.

APRIL 2nd

974th General Monthly Meeting. Location: Bowlers' Club of N.S.W. The President, Associate Professor J.H. Loxton, was in the Chair, and 36 members and visitors were present.

The deaths were announced of Trevor Taylor (19.10.85), Norbert Thomas Wright (29.10.85), Torrence Edward Kitamura (1.1.86), and William Broderick Smith-White (8.2.86).

It was announced that the formal transfer of the Royal Society Library Collection to the custody of the Dixson Library at the University of New England had taken place on 23 March, 1986. This ceremony was followed by a dinner to mark the 25th Anniversary of the New England Branch and about 80 members and friends were present. On 24th March, a symposium on "Biological Evolution" was held, and Professor D.P. Craig delivered the 25th Anniversary Address entitled "Science, its private and its public face".

119th Annual General Meeting. Followed the 974th General Monthly Meeting. The Annual Report of Council and the Annual Financial Report were adopted.

The following awards for 1985 were announced: Clarke Memorial Medal: Professor Hugh Bryan Spencer Womersley; the James Cook Medal: Dr. Donald Metcalf; The Society Medal: Dr. Dalway John Swaine; and the Edgeworth David Medal: Dr. Simon Charles Gandevia and Dr. Brian James Morris.

Messrs. Wylie and Puttock, Chartered Accountants, were elected Auditors for 1986.

The following Office-Bearers were elected for 1986/87:

President:	Mr. M.A. Stubbs-Race
Vice-Presidents:	Associate Professor J.H. Loxton
	Dr. R.S. Bhathal
	Professor T.W. Cole
	Professor R.L. Stanton
	Dr. R.S. Vagg
Hon. Secretaries:	Dr. D.J. Swaine
	Mrs. M. Krysko (Editorial)
Hon. Treasurer:	Dr. A.A. Day
Hon. Librarian:	Miss P.M. Callaghan

Members of Council: Dr. D.G. Drummond, Mr. H.S. Hancock, Mr. D.S. King, Professor R.M. MacLeod, Mr. E.D. O'Keeffe, Mr. W.H. Robertson, Dr. F.L. Sutherland, and Mr. J.A. Welch.

The retiring President, Associate Professor J.H. Loxton, delivered his Presidential Address entitled "The Love of Numbers".

The incoming President, Mr. M.A. Stubbs-Race, was installed and introduced to members.

MAY 4th

975th General Monthly Meeting. Location: Bowlers' Club of N.S.W. The President, Mr. M.A. Stubbs-Race, was in the Chair, and 24 members and visitors were present. Phillis Ogilvie Collinson-Smith, David Christopher Hunt, John Anthony Milburn, Lawrence Walter Nichol and Robert William Roderick Thomas were elected to membership.

Papers read by title only: R.L. Stanton: "The Influence of Sedimentary Environment on the Development of Stratiform Oretype"; D.F. Branagan: "The Sydney Floods of November 1984 and Engineering Geology".

A talk on "Catching Dust Electrically" was given by Dr. E.C. Potter of CSIRO Division of Fossil Fuels, North Ryde.

JUNE 4th

976th General Monthly Meeting. Location: Bowlers' Club of N.S.W. The President, Mr. M.A. Stubbs-Race, was in the Chair, and 33 members and visitors were present. Brian Andrew Hills, Thomas John Sinclair and Thomas Rome Gillespie.

Papers read by title only: D.K. Tompkins: "Academic Studies and the Coal Industry. The Sampling of Coal as a Bulk Commodity"; J.A. Dulhunty: "Mesozoic Garrawilla Lavas Beneath Tertiary Volcanics of the Nandewar Range"; R. Helby et al: "The Age of the Permian Sequence in the Stroud-Gloucester Trough"; and J.L. Cook and E.K. Rose: "Meson Source Densities for Excited States of the Nucleon".

A lecture on "Making Ornamentals from Australia's Flora" was delivered by Dr. P.B. Goodwin of the Department of Agronomy and Horticultural Science at the University of Sydney.

JULY 2nd

977th General Monthly Meeting. Location: Bowlers' Club of N.S.W. The President, Mr. M.A. Stubbs-Race, was in the Chair, and 19 members and visitors were present. Dr. Alison Rodger was elected to membership. It was announced that Sir Gustav Nossal and Emeritus Professor Arthur John Birch had been elected Honorary Members by the Council.

Professor K.L. Williams of the School of Biological Sciences, Macquarie University, gave a talk on "What can slime moulds teach us about ourselves? Some surprises from fundamental and applied research on *Dictyostelium discoideum*".

AUGUST 6th

978th General Monthly Meeting. Location: Bowlers' Club of N.S.W. The Vice-President, Associate Professor J.H. Loxton, was in the Chair, and 11 members and visitors were present. Campbell Maxwell Steele and Glenn Frederick King were elected to membership. The death on August 4th was announced of Maurice James Puttock, former President and Society Medallist.

A talk on "An Australian Initiative in Space - A Menu of Science, Technology and Spin-Offs" was given by Dr. K.G. McCracken, Director, COSSA, Canberra.

SEPTEMBER 9th

979th General Monthly Meeting. Location: Bowlers' Club of N.S.W. The Vice-President, Associate Professor J.H. Loxton, was in the Chair, and 15 members and visitors were present.

Paper read by title only: J.J. Brophy and E.V. Lassak: "The Volatile Leaf Oils of Some Central Australian Species of *Eucalyptus*".

The deaths were announced with regret of Emeritus Professor Raymond Le Fevre FRS FAA on 26th August, and Councillor and former Government Astronomer, William Humphrey Robertson, on 25th August.

A talk was given by Dr. J.S. Croucher of the School of Economic and Financial Studies at Macquarie University on "Optimal Strategies in Sport".

OCTOBER 1st

980th General Monthly Meeting. Location: Bowlers' Club of N.S.W. The President, Mr. M.A. Stubbs-Race, was in the Chair and 22 members and visitors were present. Phillip Mark Rodger and Kelvin Lloyd Grose were elected to membership.

Dr. Colin Hope, Research Fellow in History at the Macquarie University gave a talk entitled "A Buried City in the Egyptian Sahara".

NOVEMBER 5th

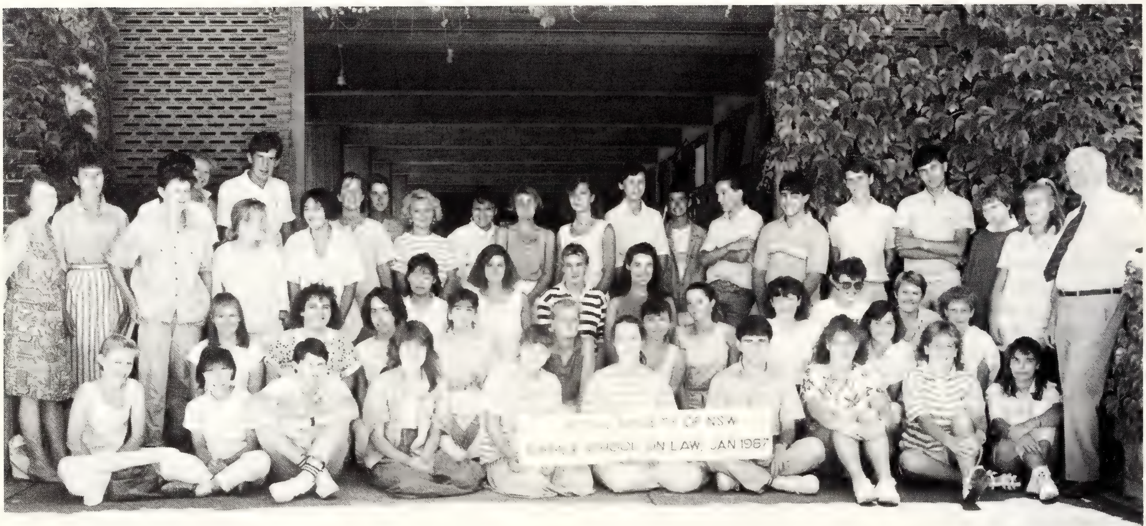
981st General Monthly Meeting. Location: Bowlers' Club of N.S.W. The Vice-President, Associate Professor J.H. Loxton, was in the Chair and 14 members and visitors were present. The Clarke Medal for 1985 was presented to Professor H.B.S. Womersley of Adelaide University.

Dr. Simon Baxter, formerly Senior Forensic Biologist with the Division of Forensic Medicine, N.S.W. Department of Health, delivered a lecture on "Forensic Science: a non-consummated marriage?"

DECEMBER 3rd

982nd General Monthly Meeting. Location: Bowlers' Club of N.S.W. The President, Mr. M.A. Stubbs-Race was in the Chair, and 19 members and visitors were present. Kai Ting Francis Phang Seow and Sandra Anne Walsh were elected to membership.

Dr. P.G. Flood, Senior Lecturer in Geology at the University of New England, gave a talk on "Ideas Concerning Development of the Great Barrier Reef: An Historical Account".



Participants in the Summer School on Law, January 1987, at Macquarie University. Mrs. M. Krysko, Convenor of the School, is on the left, and Mr. J.A. Welch, Assistant Convenor, is on the right.

Awards

WALTER BURFITT PRIZE

Brian Norman Figgis

The award of the Walter Burfitt Prize for 1986 is made to Professor Brian Figgis for his fundamental contributions to the field of physical-inorganic chemistry.

Brian Figgis was born and educated in Sydney. After attending Fort Street Boys' High School he studied for the BSc and MSc degrees of the University of Sydney, then obtained a PhD degree from the University of New South Wales working under the supervision of Professor Sir Ronald Nyholm. After very productive postdoctoral research and a lectureship at University College, London, he returned in 1963 to an appointment in the School of Chemistry at the University of Western Australia. He holds the DSc degree of that University, where he has been Professor of Inorganic Chemistry since 1969. He is a Fellow of the Australian Academy of Sciences, a Fellow of the Royal Australian Chemical Institute, and a recent recipient of the G.J. Burrows Award of that Institute.

Professor Figgis has made innovative and substantial contributions to the literature of inorganic chemistry which have been recognised internationally for three decades. Early in his professional career his collaborative work with Professor Sir Jack Lewis led to the development of magnetic susceptibility measurements as a major research tool for the inorganic chemist. At that time he quickly became the acknowledged world authority on magnetochemistry, and in 1966 produced his definitive monograph "Introduction to Ligand Fields".

Much of his more recent research, carried out in collaboration with Professor Sir Ronald Mason, involves a study of the spin density of electrons in transition metal compounds. The work employs several sophisticated chemical, physical and mathematical techniques including the combination of magnetic measurements and polarised neutron diffraction and X-ray methods. This work is considered to be among the most fundamentally significant inorganic chemistry being carried out anywhere in the world. Its potential value to the understanding of the nature of chemical bonding and the electronic structure of the atom makes its importance extensive to the many branches of science which have a chemical basis.

Such research requires the rare combination of imagination, attention to detail, experimental abilities and theoretical skills that Brian Figgis possesses. The Walter Burfitt Prize is awarded in recognition of published work deemed to be of the highest scientific merit, and Brian Figgis is without doubt a worthy recipient.

THE SOCIETY'S MEDAL

Sydney Charles Haydon

Professor Sydney Charles Haydon was educated at Sutton High Schook, Plymouth and the Universities of Oxford, Wales and Manchester. Following the award of the Corrie Prize, a Ballard Exhibition tenable at the University of Exeter and a State Bursary tenable at the University of Oxford, he read Physics at Oxford from 1943-45 (War Degree) and from 1948-50 (Honours degree) as a student of Professor A.H. von Engel. During the intervening period 1946-48 he was a member of a post-graduate group studying "High Vacuum Techniques and Advanced Electronics" in the Electrotechnology Department of the University of Manchester under Professor Willis Jackson, FRS. He was then directed as part of the National Service requirements to work at the Mullard Radio Valve Co. on problems associated with the development and manufacture of gas discharge devices required by Government Departments.

He joined the research group of Professor F. Llewellyn Jones at the University of Wales in 1950 to work on electrical discharge problems. His Ph.D. was awarded in 1952 for a thesis on "The Electrical Breakdown of Gases at High Pressures", and that part of his researches concerned with the role of photo-ionization was read at a conversazzione held at the Royal Society of London in June, 1953. This was followed by two years of post-doctoral research on electrical breakdown at Swansea with support from the British Electricity Authority before he moved, in 1954, to a lectureship in Physics at the University of New England, in the Department of Professor J.M. Somerville.

At Armidale, his interests in gas breakdown extended to studies of ionization in crossed electric and magnetic fields as well as to the problems of non-equilibrium ionization at high mean electron energies. His outstanding abilities both in research and University teaching led to rapid promotion and in 1965 he was appointed to the Chair of Physics left vacant by the death of Professor Somerville.

For the 22 years since his appointment to the Chair Professor Haydon has continued to lead an active team of research workers investigating a wide variety of ionization, gas discharge and plasma phenomena. Strong emphasis has been given to problems relevant to the developments of new gas lasers and the application of tunable dye laser techniques to improving our understanding, at the fundamental level, of the complex phenomena associated with electrical discharges.

During 1969 he visited the U.S.A. as Visiting Fellow at the Joint Institute for Laboratory Astrophysics, Boulder, Colorado, where his studies of highly transient gas discharges for pulsed lasers led to pioneering developments of tunable dye lasers. These were applied subsequently at the University of New England to establish highly selective laser-induced perturbation techniques for the study of opto-galvanic effects in weakly ionized gases. This work has been significant in unravelling the particular contributions to ionization phenomena made by neutral excited metastable particles. The importance of metastable particles in gas discharge lasers led to a further intensive period of study of their properties at the Clarendon Laboratory, Oxford, in 1976 and resulted in major improvements in the performance of new excimer laser based on the generation of rare-gas monohalides under pulsed gas discharge operation.

Concurrently with these investigations Professor Haydon has also undertaken a series of investigations into the cause of arc-plumes generated at the aerial arrays associated with high power radio-frequency transmitters. The problems originally arose at the Darwin site of Telecom's log-periodic aerial system and the thrust of the work has been to establish, through highly time-resolved spectroscopic and associated image intensification techniques, the mechanisms by which the unwanted gas discharge phenomena are initiated. The high quality of these researches was recognised by the award of the Ayrton Premium for 1985 by the Council of the Institute of Electrical Engineers in London.

Professor Haydon has, by himself and with his close colleagues, published some 80 papers in the scientific literature. He is one of a long and distinguished line of Oxford graduates - which includes Sir T.W. Edgeworth David, one of this Society's prominent members - who have made outstanding contributions to Australian Science and scientific life. He has been an active member of the New England Branch of the Royal Society of New South Wales since 1962, and its dedicated chairman since 1979. He has made an outstanding contribution to the advancement of Science, especially in the field of Physics, and to the Society, and he is a very worthy recipient of the Society's Medal.

CLARKE MEDAL

David Ian Groves

The Society's Clarke Memorial Medal for 1987 is awarded to Associate Professor David Ian Groves, of the Geology Department, University of Western Australia, Perth.

David Groves was born in England in 1942, but began his geological studies in Australia. He graduated with first class honours from the University of Tasmania in 1963. His studies led him into economic aspects of geology and he was awarded his Ph.D. from the University of Tasmania in 1968 for his work on tin deposits of western Tasmania. His ideas on metal deposits in granites, developed as a geologist with the Tasmanian Department of Mines, brought him international repute.

He joined the mineral boom in Western Australia in 1971, as a lecturer at the University of Western Australia, where he studied nickel deposits and their host rocks. His studies expanded to unravel the complex contributions that folding, metamorphism and granite intrusion made to the metallogenic evolution of these ancient Archaean terrains. More lately he has concentrated his research on gold mineralisation in the Archaean and has built up a highly successful team of co-workers. His contributions to the understanding of Australian gold deposits make him a most appropriate recipient of the Clarke Medal, as the Reverend W.B. Clarke was the father of such studies in Australia's early goldrush days.

In personality, David Groves has a rare ability to mix academic and economic aspects of his subject, to inspire geological peers and students alike and to involve himself with both geological and industrial professional bodies. He has made many international links, particularly for studies made in South Africa and England and has been a keynote or invited speaker in countries further afield such as Czechoslovakia, Russia, Canada, Brazil, SWA/Namibia, U.S.A. and Singapore.

His extensive record as an author and as an editor of papers and books, stands at over 140 extended contributions in Australian and international publications. He has won several prizes and awards including the Mt. Lyell Mining and Railway Co. Prize for Economic Geology, the F.L. Stillwell Award of the Geological Society of Australia and the inaugural Gibb Maitland Medal of the W.A. Division of the Geological Society of Australia. The award of the Clarke Medal to him is in good company.

EDGEWORTH DAVID MEDALS

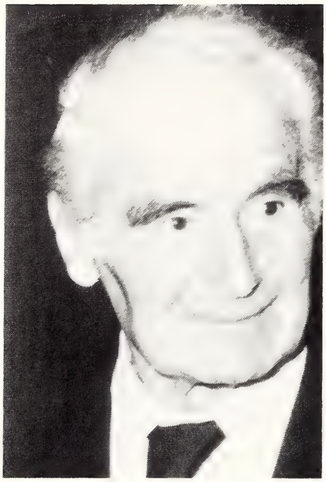
Peter Gavin Hall

Dr. Peter Hall is a graduate of the University of Sydney (B.Sc. and University Medal), the Australian National University (M.Sc.) and the University of Oxford (D.Phil.). After returning to Australia in 1976, he was a lecturer in Statistics at the University of Melbourne and thereafter at the Australian National University, where he is currently Reader in Statistics. He is the author and coauthor of two books and more than one hundred papers published during the last decade. In his main field of interest, namely probability and statistics, he has made major contributions to the rates of convergence, particularly in the central limit theorem, to martingale limit theory, to coverage and other problems in geometric probability and to extreme value theory. He has applied his skills to the modelling of chemical kinetic systems and to some areas of environmental interest, especially to problems concerned with the use of herbicides and possible health effects. His development of what is known as "the leading term approach" is an important advance on rates of convergence in weak limit theorems, described in detail in his book, entitled "Rates of Convergence in the Central Limit Theorem", published in 1982. Although he has several joint publications, most of his papers are sole efforts. He is an Associate Editor of five journals. Dr. Hall's outstanding work in statistics has been recognised internationally by the award of the Rollo Davidson Prize. It is surely fitting that Dr. Hall is to receive the Edgeworth David Medal.

Leslie David Field

Dr. Leslie Field was born in Sydney in 1953. He graduated from the University of Sydney with degrees of B.Sc. (University Medal) and Ph.D., and thereafter carried out postdoctoral research at the Hydrocarbon Research Institute in the University of Southern California and at the Dyson Perrins Laboratory in the University of Oxford. On his return to Australia he joined the staff of the Department of Chemistry at the University of Sydney, where is a Senior Lecturer in Organic Chemistry. Dr. Field has contributed significantly to two major areas, mainly the application of nuclear magnetic resonacne spectroscopy (NMR) to the determination of the structure of small molecules, and organometallic chemistry. He studied the mechanism of the biosynthesis of penicillin antibiotics using NMR, thereby showing its applicability to problems of complex mechanistic and synthetic chemistry and biochemistry. His studies have shown that proton NMR is helpful in elucidating the three-dimensional structures of small organic molecules in liquid crystalline solution. Current research on organo-iron and organo-cobalt compounds with hydrocarbon activation is giving important insights into the structures of new compounds, for example, iron-alkyls. Reactions with alkanes and related compounds are bing investigated as possible sources of industrially important compounds. Dr. Field is the coauthor of a book entitled "Hydrocarbon" which has just been published. His research is well recognised overseas and he has clearly contributed notably to the advancement of chemical science in Australia. Dr. Field certainly deserves the award of the Edgeworth David Medal.

Biographical Memoirs



RAYMOND JAMES WOOD LE FÈVRE

Raymond James Wood Le Fèvre, DSc, PhD, FRIC, FRACI, FAA, FRS, died at his home at Northbridge on 26 August, 1986, at the age of 81.

He was born in London in 1905 and was educated at Isleworth County School and Queen Mary College, University of London. In 1928 he became a lecturer and ten years later a Reader in Organic Chemistry at University College, London. He was awarded the degrees PhD (1928) and DSc (1935). For the period 1939-44 he was Hon. Wing Commander and Chemical Adviser to the R.A.F. and the R.A.A.F. in the United Kingdom, the Far East and Australia. Le Fèvre was in Singapore when it fell, and after destroying the R.A.F. stocks of chemical weapons, was evacuated to Australia. In 1944 he was appointed Assistant Director Research and Development (Armament Chemistry), Ministry of Aircraft Production, London. During 1944-46 he was head of the Chemistry Department, Royal Aircraft Establishment at Farnborough.

In 1946 he was appointed Professor of Chemistry at the University of Sydney and in 1948 Head of the School of Chemistry which had then been created by amalgamation of previously existing Departments. In this capacity he served with distinction until his retirement in 1970. His period as Head was characterised by the great impetus he gave to research within the School. He had about 100 research students and co-authors during his term of office. A major work also was the design and planning of the new Chemistry School, constructed in the mid to late 1950s. In 1971 he was made Emeritus Professor of the University of Sydney and later thereafter he continued his research at Macquarie University as an Honorary Professorial Fellow.

Raymond Le Fèvre joined the Royal Society of New South Wales in 1947. He twice served as a member of Council (1948-51 and 1961-74), and was President in 1961. In 1960 he delivered the Liversidge Research Lecture and in 1969 was awarded the Society's Medal for his contributions to chemistry. He was author of a substantive chapter titled "The Establishment of Chemistry within Australian Science - Contributions from New South Wales" in "A Century of Scientific Progress", The Centenary Volume of the Royal Society of New South Wales (1968).

He had a whole host of academic and community involvements, and was the recipient of many awards and distinctions. He was, *inter alia*, a Fellow of the Royal Institute of Chemistry, Fellow of the Royal Australian Chemical Institute and N.S.W. Branch President and Smith Medallist (1952); President, Section B, ANZAAS (1956); Masson Lecturer (1967); Chemical Society of London Lecturer for Australia (1968); Coronation Medallist (1953); Fellow, Queen Mary College, London (1962); Trustee of the Museum of Applied Arts and Sciences Sydney (1947-75) and the Mitchell Library (1947); Member of the Pure Foods Advisory Committee, NSW Health Department (1947-70); Member of the Developmental Council of the N.S.W. University of Technology (1948-50); and Member of the Rotary Club of Sydney since 1948.

He was a Foundation Fellow and Member of the First Council of the Australian Academy of Science (1954). In 1959 he was elected Fellow of the Royal Society of London. His contributions to the University of Sydney were honoured in 1985 by the award of an Honorary Doctorate of Science.

Raymond Le Fèvre had an international reputation in Physical-Organic Chemistry. His research interests embraced a wide range of programs, notably optical activity from conformational causes; the interplay of steric and electropolar effects in substituted aromatics; the utilisation of dipole moments in relation to molecular structure, environment and state; kinetic and spectral studies of azo-compounds; electric and magnetic birefringences and Rayleigh depolarisations of molecular substances in solution. He was author or coauthor of approximately 450 research papers and several reviews as well as the author of a book titled "Dipole Moments". Much of his most notable work on dielectric and electro-optic properties in relation to molecular structure, was shared with Cathie, his wife, herself a DSc.

M. Aroney

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WILLIAM HUMPHREY ROBERTSON

William Robertson, who died suddenly on 25th August, 1986, was a well-known member of the Royal Society of New South Wales. He joined this Society in 1949 and served on the Council for the years 1965-72, 1976-80 and 1984-86. He was elected President in 1977-78. He contributed 35 astronomical papers to the Journal and Proceedings. In 1975 he was awarded the Medal of the Royal Society of New South Wales.

William Robertson was born at Blackheath, N.S.W., on October 27th, 1918, and attended Katoomba High School. He graduated from the University of Sydney in 1940 with Honours in Mathematics and Physics.

After one year as a teacher at Penrith High School, Bill Robertson was appointed to Sydney Observatory as Assistant Astronomer in 1942. This marked the start of along career in astronomy during which he took part in all facets of the work of the Observatory. His main activities were in catalogue work and minor planet observations.

Initially his principal task in catalogue work was to continue the plate constant computations needed to convert plate measures to standard coordinates. Right from the start Bill Robertson tackled this work with skill and industry. Before long the leeway in the preparation of plate constants for the astrographic volumes was overcome. Through his energetic approach to plate constant calculations and his extensive supervisionary work, Bill Robertson made a significant contribution towards the completion of the Melbourne and Sydney sections of the Astrographic Catalogue.

In 1953 Bill Robertson commenced observing minor planets photographically. These observations were intended to improve their orbital elements and to use the observations of specially selected minor planets to make corrections to the fundamental astronomical coordinate system. By 1968 so few examples of minor planets needing orbital corrections had been found that this programme was discontinued. The observations of special minor planets with well known orbits has continued. The reductions of the measures made on plates taken for this purpose were made with care and patience. The results obtained by Bill Robertson were comparable, in accuracy and in absence of serious errors, to that achieved by other Southern Hemisphere observatories.

In 1975 Bill Robertson was appointed Government Astronomer following the retirement of Dr. Harley Wood. He proved an able administrator. As his 8 year stewardship coicided with increased computer facilities and with the use of the long-screw Grubb-Parsons measuring machine, he actively encouraged increased scientific output. In this period some 18 astronomical papers were published in this Journal. The zone photographic work was completed in this period. Bill Robertson, together with other staff members, actively participated in the reduction of plates taken for the Sydney Southern Star Catalogue. The decision to close Sydney Observatory disappointed him. Fortunately it was possible to publish the results for a declination range $-51^{\circ}00'$ to $-63^{\circ}30'$. The standard error of a catalogue position based on 4 images is $0''.10$ in either co-ordinate, a distinct improvement on an Astrographic Catalogue position.

Bill Robertson had been a member of the International Astronomical Union since 1961. He attended General Assemblies of the Union in 1970, 1973, 1976 and 1979. He was a member of the organising Committee of Commission 20 of the Union (which deals with the positions of Minor Planets and Comets) from 1973-79. He was appointed by the Academy of Science to the National Committee for Astronomy during the period 1975-82. He had been a member of the Astronomical Society of Australia since 1966 and of the British Astronomical Association from 1942.

In his professional life his sense of humour, calmness and attention to detail were qualities his colleagues greatly respected. He liked gardening and was active in the Presbyterian Church.

K.P. Sims

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William Humphrey Robertson

Maurice James Puttock

MAURICE JAMES PUTTOCK

It is with great sorrow that we record the death of Maurice Puttock on 4 August 1986, after a long and courageous battle against an illness which extended over three years. His passing was a great loss, not only to his family and those of us who were privileged to be included amongst his close friends, but also to the community at large.

Maurice was born and educated in England. Before coming to Australia to join CSIRO he worked at the National Physical Laboratory (NPL), Teddington, and from whence, during World War II,he enlisted in the Royal Air Force, later transferring to the Fleet Air Arm of the Royal Navy. After the end of the war he graduated from the University of London, where he obtained B.Sc.Eng. with Honours, and returned to the NPL to become a Scientific Officer. For some time he worked there with Dr. H. Barrell on a new determination of the length of the Imperial Standard Yard, and that of the Metre, in terms of wave-length of light.

Following his arrival in Australia during 1952, he assumed the leadership of the Length Metrology Group at the National Standards Laboratory (now the National Measurement Laboratory) where he was responsible for the Australian National Standards of Length and for the maintenance of a first-class engineering measurement facility with a world-wide reputation for excellence. With his wealth of experience in the field of precision measurement, combined with his ever-ready willingnesss to discuss problems however mundane, Maurice's counsel was sought frequently by both industry and research alike.

The scope of his expertise was applied to such items as the alignment of steel rolling mills and large building structures (e.g. Centre Point Tower, Sydney) to the metrological requirements in the design of Interscan, and the measurement of wear, in situ, of railway tracks used in the heavy ore industry.

Perhaps one of his most noteworthy achievements was his contribution, in association with Harry Minnett, to the upgrading of the 64-metre radio telescope at Parkes, N.S.W. As a result, the resolution of this telescope was increased by several orders of magnitude (from wavelengths of 210mm to those of 3mm) and placed the instrument amongst the leaders of its class in the world. Truly this was a masterly achievement.

Another, which will serve as a memorial for many years to come, is to be found in the success of the National Measurement Laboratory complex at West Lindfield, N.S.W. For 10 years or more, during the design and construction stages of this project, Maurice worked tirelessly with Ron Kemp to provide the necessary liaison between the architects and contractors, and the Commonwealth Scientific and Industrial Research Organization (CSIRO), to ensure that the special requirements demanded of the complex were fully realized.

During his professional life Maurice gave generaously of his time and energy, having served on numerous national and international committees. He served as a CSIRO representative on the Councils of both the Standards Association of Australia and the National Association of Testing Authorities, was a Commissioner with the National Standards Commission and, in addition, took his place as a member on many of the working committees associated with these organizations. At the international level he was Chairman of the International Standards Organization Technical Committee ISO/TC3, the work of which was nearing completion when he became ill. With great courage he managed to attend the final meeting of the Committee in Brussels during 1985 and bring the work to a satisfactory conclusion.

Maurice published a number of papers in the field of Metrology; he had been a part-time lecturer at the University of Sydney, the then New South Wales University of Technology (now the University of New South Wales) and the Sydney Technical College; he was also the author of 'A Standard Text Book on Engineering Metrology for Technical Students'.

He was a Member of both the Institute of Mechanical Engineers (England) and the Institute of Mechanical Engineers (Australia) as well as an Associate of the Institute of Physics (London) and of the Australian Institute of Physics.

Maurice Puttock joined the Royal Society of New South Wales in 1960 and was elected to Council in 1967. During his 17 years on Council he was President in 1971; Vice President 1972-76 and 1979-83; Honorary Secretary 1977-78. He was awarded the Society's Medal in 1978.

Maurice was a committed Christian, and throughout his life evidence of his faith and conviction could be seen in all his activities. He was survived by his wife, Pat, three children and four grandchildren to all of whom the Society extended its very deepest sympathy.

J.W. Humphries

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HARRY ALBERT THEODORE SCHOLER

Harry Scholer had a surprising range of interests. As a civil engineer he specialised in hydrology and became a Fellow of the Institution of Engineers, Australia. He was also the first president of the Animal Liberation Movement; a member of the management committee of the Total Environment Centre in Sydney; and a vice-president of the North Shore (Sydney) Historical Society and editor of its journal. He was elected to membership of the Royal Society of New South Wales on October 5th, 1960, and was also a Companion of the Royal Aeronautical Society.

Before his retirement he worked in the Harbours and Rivers Branch of the New South Wales Public Works Department, and was for a number of years seconded to the University of New South Wales' Water Research Laboratory. Subjects of his publications included work on the geomorphology of N.S.W. coastal rivers and flood mitigation in the Hawkesbury, Georges and Hunter Rivers. He published one paper in the Journal and Proceedings of the Royal Society of New South Wales entitled: "Discharge of Sands by Sandy Bed Streams and the Regime of Leveed Rivers in Coastal Flood Plains" (Vol. 109, 1976).

Care for the environment was a hallmark of his work, which sometimes aroused antagonism. For example, people who liked to water ski and drive power boats were upset by his findings that such activities could have serious adverse effects on rivers and their banks. He was also concerned about sand mining and the development of land in flood-prone areas.

Harry Scholer was a nephew of the German architect Friedrich Scholer, who with Paul Bonatz designed such noted buildings as the Hanover Stadhalle and Stuttgart railway station. A branch of the family had been established in Australia since the 1860s. Harry's father, Hans, and a partner ran the country-town equivalent of a department store - the Palace Emporium at Casino, N.S.W. Harry was born at Casino on December 2nd, 1914. One of his earliest memories, he told friends, was of an anti-German crowd gathered outside the store during World War I.

In recent years he combined his devotion to animals with his interest in history by organising a memorial to Trim - the cat who accompanied Matthew Flinders on his circumnavigation of Australia and other voyages. Trim's memorial tablet is installed beside Flinders' statue in Macquarie Street, Sydney.

Harry Scholer died on December 6th, 1986, aged 72. He was married twice - firstly to Australian writer Patricia Clare, then to Grace Cohagan, an American social worker, who predeceased him. He was buried at Richmond Lawn Cemetery after a service at St. Matthew's Church of England, Windsor.

John Welch

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Spelling follows "The Concise Oxford Dictionary".

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